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AMERICAN JOURNAL

OF THE

MEDICAL SCIENCES.

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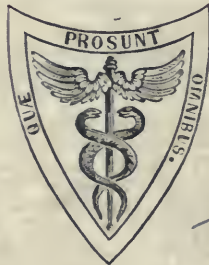
ISAAC HAYS, M.D.,

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FELLOW OF THE PHILADELPHIA COLLEGE OF PHYSICIANS; MEMBER OF THE
AMERICAN MEDICAL ASSOCIATION; OF THE AMERICAN PHILOSOPHICAL SOCIETY; OF THE
ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA,
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NEW SERIES.

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TO READERS AND CORRESPONDENTS.

A number of communications are on hand which shall receive early attention.

The following works have been received:—

Health of the Navy. Statistical Reports on the Health of the Navy for the years 1837, 1838, 1839, 1840, 1841, 1842, and 1843. (In continuation of Parl. Paper, No. 436, of Sept. 1849.) Part II., East India Station. (Presented to Parliament by Her Majesty's command.) (From Sir William Burnett.)

The Census of Ireland for the year 1851, Part III. Report on the Status of Disease. Presented to both Houses of Parliament by command of Her Majesty. Dublin: 1854. (From the Census Commissioners of Ireland.)

Epilepsy and other Affections of the Nervous System which are marked by Tremor, Convulsion, or Spasm; their Pathology and Treatment. By CHARLES BLAND RADCLIFFE, M. D., Licentiate of the Royal College of Physicians, Assistant-Physician to the Westminster Hospital, etc. London: 1854. (From the Author.)

Lectures on the Diseases of Infancy and Childhood. By CHARLES WEST, M. D., Fellow of the Royal College of Physicians; Physician to the Hospital for Sick Children, etc. Second American, from the second and enlarged London edition. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

The Science and Art of Surgery; being a Treatise on Surgical Injuries, Diseases, and Operations. By JOHN ERICHSEN, Professor of Surgery in University College, and Surgeon to University College Hospital. Edited by JOHN H. BRINTON, M. D. Illustrated by three hundred and eleven engravings on wood. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

Handbook of Chemistry, Theoretical, Practical, and Technical. By F. A. ABEL, Professor of Chemistry at the Royal Military Academy, Woolwich, and Assistant-Teacher of Chemistry at St. Bartholomew's Hospital; and C. L. BLOXAM, Formerly First Assistant to the Royal College of Chemistry. With a Preface, by Dr. Hoffman; and numerous illustrations on wood. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

Woman; her Diseases and Remedies. A Series of Letters to his Class. By CHARLES D. MEIGS, M. D., Professor of Midwifery, and the Diseases of Women and Children in the Jefferson Medical College at Philadelphia, etc. etc. Third edition, revised and enlarged. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

A Universal Formulary; containing the Methods of Preparing and Administering Official and other Medicines. The whole adapted to Physicians and Pharmacutists. By R. EGLESFELD GRIFFITH, M. D. A new edition, carefully revised and much extended. By ROBERT P. THOMAS. With Illustrations. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

Lithographic Portrait of the late ROBERT MONTGOMERY BIRD. (From Dr. E. R. Mayer.)

Diseases of the Heart and the Aorta. By WILLIAM STOKES, Regius Professor of Physic in the University of Dublin, etc. Philadelphia: Lindsay & Blakiston, 1854. (From the Publishers.)

Types of Mankind; or Ethnological Researches, based upon the Ancient Monuments, Paintings, Sculptures, and Crania of Races, and upon their Natural, Geographical, Philological, and Biblical History. Illustrated by Selections from the unedited papers of Samuel George Morton, M. D. (late President of the Academy of Natural Sciences, Philadelphia), and by Additional Contributions from Prof. Agassiz, LL. D., W. Usher, M. D., and Prof. H. S. Patterson, M. D. By J. C. NORT, M. D., of Mobile, Alabama, and GEO. R. GLIDDEN, formerly U. S. Consul at Cairo. Philadelphia: Lippincott, Grambo, & Co., 1854. (From the Publishers.)

The Preservation of Health, with Remarks on Constipation, Old Age, Use of Alcohol in the Preparation of Medicines. By JOHN C. WARREN, M. D., Emeritus Professor of Anatomy and Surgery in Harvard University. Boston: 1854. (From the Author.)

Remarks on some Fossil Impressions in the Sandstone Rocks of Connecticut River. By JOHN C. WARREN, M. D., President of the Boston Society of Natural History. Boston: 1854. (From the Author.)

Clinical Lectures on Pulmonary Consumption. By THEOPHILUS THOMPSON, M.D., F. R. S., Fellow of the Royal College of Physicians, London; Physician to the Hospital for Consumption and Diseases of the Chest, etc. Philadelphia: Lindsay & Blakiston, 1854. (From the Publishers.)

Clinical Report on Dysentery; based on an Analysis of Forty-nine Cases; with Remarks on the Causation, Pathology, and Management of the Disease. By AUSTIN FLINT, M.D., Professor of the Theory and Practice of Medicine in the University of Louisville, Ky. Buffalo: 1853. (From the Author.)

Clinical Report on Chronic Pleurisy, based on an Analysis of Forty-seven Cases. By AUSTIN FLINT, M.D., Prof. of Principles and Pract. of Med. in the University of Buffalo, N. Y. Buffalo: 1853. (From the Author.)

History of the Epidemic Yellow Fever at New Orleans, La., in 1853. By E. D. FENNER, M.D., one of the Visiting Physicians to the New Orleans Charity Hospital, etc. etc. New York: 1854. (From the Author.)

Outlines of the Principles and Practice adopted in the Orthopædic Institution of Brooklyn. By LOUIS BAUER, M.D., Physician and Surgeon (Berlin), etc. etc.; and RICHARD BARTHELMESS, M.D., Physician and Surgeon (Wurzburg, Bavaria), etc. etc. New York: 1854.

A Report to the Indiana State Medical Society on Asiatic Cholera, as it prevailed within the State of Indiana, during the years 1849, 1850, 1851, and 1852. With Observations on the Laws which govern its Progress. By GEORGE SUTTON, M.D. Indianapolis: 1854. (From the Author.)

Vaccination, and the Causes of the Prevalence of Smallpox in New York, in 1853-'54. From the New York Journal of Medicine. By JOSEPH C. HUTCHINSON, M.D., Physician to the Brooklyn City Dispensary. New York: 1854. (From the Author.)

A Monograph on the Fœtal Circulation. With three illustrations on wood. By E. R. PEASLEE, A.M., M.D., Professor of Anatomy in Dartmouth College, and in the New York Medical College, etc. From the American Medical Monthly, May, 1854. New York: 1854. (From the Author.)

A Critical Examination of a Pathological Specimen of Softening of Invertebral Fibro-Cartilages. From the New York Journal of Medicine. By LOUIS BAUER, M.D. New York: 1854.

Transactions of the Fourth Annual Meeting of the Medical Society of the State of North Carolina. Held at Fayetteville, N. C., May, 1853. Wilmington, N. C.: 1854.

Transactions of the Medical Association of the State of Alabama. At its Seventh Annual Session, begun and held in the City of Montgomery, January 10-12, 1854. With Constitution, Code of Medical Ethics, and List of Members. Mobile: 1854.

Seventh Annual Report of the Regents of the University of the State of New York on the Condition of the State Cabinet of Natural History, and the Historical and Antiquarian Collection annexed thereto. Made to the Senate, Jan. 18, 1854. Albany: 1854. (From Dr. T. R. Beck.)

Sixty-Seventh Annual Report of the Regents of the University of the State of New York. Transmitted to the Legislature, March 1, 1854. Albany: 1854. (From Dr. T. R. Beck.)

Constitution and By-Laws of the Medical Association of Texas. Instituted January 17, 1853. Incorporated by the Fifth Legislature, 1853-1854. Proceedings of the last Annual Meeting, together with the Anniversary Address. Delivered by GEO. CUPPLES, M.D., November, 1853. Austin: 1854.

Report of the Board of Trustees of the Insane Asylum of the State of California. Submitted to the Legislature, January 20, 1854.

Eleventh Annual Report of the Managers of the State Lunatic Asylum of the State of New York. Transmitted to the Legislature, February 8, 1854. Albany: 1854.

Thirty-Seventh Annual Report on the State of the Asylum for the Relief of Persons Deprived of the Use of their Reason. Published by direction of the Contributors, third month, 1854. Philadelphia: 1854.

Remarks on the Efficiency of Quarantine, as a means of preventing the spread of Epidemic Diseases in cities and other populous communities; being

the Anniversary Oration before the South Carolina Medical Association for 1854. By WM. T. WRAGG, M. D. Charleston: 1854. (From the Author.)

Annual Address before the Philadelphia County Medical Society. Delivered February 8, 1854. By JOHN F. LAMB, M. D., the late President. Published by the Society. Philadelphia: 1854.

Lecture Introductory to the Course on Chemistry and Pharmacy in the National Medical College, Washington, D. C. By LEWIS H. STEINER, M. D., Professor of Chemistry and Pharmacy. Delivered October 31, 1853. Chambersburg, Pa.: 1853.

Valedictory Address to the Graduates of the Medical Department of Pennsylvania College. Delivered at the Public Commencement, March 4, 1854. By JOHN J. REESE, M. D., Professor of Medical Chemistry and Pharmacy. Published by the Class. Philadelphia: 1854.

Address to the Graduates of the Kentucky School of Medicine. Session, 1853-'54. By R. J. BRECKENRIDGE, Professor of Materia Medica and Clinical Medicine. Published for the Graduates. Louisville: 1854.

The following Journals have been received in exchange:—

Gazette Médicale de Paris, January, February, March, April, 1854.

Annales Medico-Psychologiques. Par MM. Les Docteurs BAILLARGER, BRIERRE DE BOISMONT et CERISE. January, April, 1854.

Revue de Thérapeutique Medico-Chirurgicale. Par A. MARTIN LARVER. January, February, March, April, May, 1854.

Le Moniteur des Hôpitaux. Rédacteur en chef M. H. DE CASTELNAU. March, April, May, 1854.

Archives d'Ophthalmologie comprenant les travaux les plus important sur l'Anatomie, la Physiologie, la Pathologie, l'Hygiène, et la Thérapeutique de l'Appareil de la Vision. Par M. A. JAMAIN. January, February, March, 1854.

Archives de Physiologie, de Thérapeutique, et de Hygiène. Sous la direction de M. BOUCHARDAT. January, 1854.

Medical Times and Gazette. March, April, May, 1854.

Dublin Medical Press. March, April, May, 1854.

Association Medical Journal. Edited by JOHN ROSE CORMACK, M. D. February, March, April, May, 1854.

The Edinburgh Medical and Surgical Journal. January, 1854.

The Dublin Quarterly Journal of Medical Science. February, May, 1854.

Monthly Journal of Medical Science. February, March, April, May, 1854.

The Journal of Psychological Medicine and Mental Pathology. Edited by FORBES WINSLOW, M. D. April, 1854.

The British and Foreign Medico-Chirurgical Review. April, 1854.

The Medical Chronicle or Montreal Monthly Journal of Medicine and Surgery. Edited by Drs. WRIGHT and MCCALLUM. April, May, June, 1854.

The Peninsular Journal of Medicine and the Collateral Sciences. Edited by E. ANDREWS, A. M., M. D. March, April, May, June, 1854.

The Stethoscope. Edited by Drs. ATKINSON, HAXALL, BOLTON, LEWIS, MERRITT, and CABELL. April, May, June, 1854.

The Medical Examiner. Edited by SAMUEL L. HOLLINGSWORTH, M. D. April, May, June, 1854.

New York Medical Times. Edited by HENRY D. BULKLEY, M. D. April, May, June, 1854.

Memphis Medical Recorder. Edited by A. P. MERRILL, M. D., and C. T. QUINTARD, M. D. March, May, 1854.

St. Louis Medical and Surgical Journal. Edited by M. L. LINTON, M. D., and WM. M. MCPHEETERS, M. D. March, May, 1854.

The New York Medical Gazette. Edited by D. MEREDITH REESE, M. D. April, May, June, 1854.

The Western Journal of Medicine and Surgery. Edited by LUNSFORD P. YANDELL, M. D. November and December, 1853; March, April, May, 1854.

Southern Medical and Surgical Journal. Edited by L. A. DUGAS, M. D. April, May, June, 1854.

The American Medical Monthly. Edited by E. H. PARKER, M. D. April, May, June, 1854.

The New Jersey Medical Reporter. Edited by S. W. BUTLER, M. D., and JOS. PARRISH, M. D. April, May, June, 1854.

The New Hampshire Journal of Medicine. Edited by G. H. HUBBARD, M. D. April, May, June, 1854.

The Boston Medical and Surgical Journal. Edited by J. V. C. SMITH, M. D., and G. S. JONES, M. D. April, May, 1854.

New York Journal of Pharmacy. Edited by Drs. ANTISELL, TORREY, and EDERLIN, and BENJAMIN CANAVAN. April, June, 1854.

Buffalo Medical Journal. Edited by AUSTIN FLINT, M. D., and S. B. HUNT, M. D. April, May, June, 1854.

The Medical Reporter. A Quarterly Journal. Published under the direction of the Chester and Delaware County Medical Societies. April, 1854.

The North-Western Medical and Surgical Journal. Edited by W. B. HERICK, M. D., and H. A. JOHNSON, M. D. March, April, 1854.

New Orleans Medical News and Hospital Gazette. Edited by Drs. CHOPPIN, BEARD, SCHLATER, and BOYER. March, April, May, June, 1854.

Kentucky Medical Recorder. Edited by H. M. BULLITT, M. D., and R. J. BRECKENRIDGE, M. D. March, April, May, 1854.

The American Journal of Science and Arts. Conducted by Profs. B. SILLIMAN, B. SILLIMAN, JR., and JAMES D. DANA, in connection with Profs. GRAY and AGASSIZ, and Drs. BURNETT and GIBBS. May, 1854.

American Journal of Pharmacy. Edited by WM. PROCTOR, JR. May, 1854.

The American Journal of Dental Science. Edited by Drs. HARRIS, BLANDY, and PIGGOT. April, 1854.

The Western Lancet. Edited by L. M. LAWSON, M. D., and T. WOOD, M. D. April, May, June, 1854.

The Virginia Medical and Surgical Journal. Edited by G. A. OTIS, M. D., and J. B. McCAW, M. D. April, May, 1854.

The New York Journal of Medicine and the Collateral Sciences. Edited by S. S. PURPLE, M. D., and S. SMITH, M. D. May, 1854.

The Ohio Medical and Surgical Journal. Edited by JOHN DAWSON, M. D. May, 1854.

The American Journal of Insanity. Edited by T. ROMEYN BECK, M. D. April, 1854.

The Southern Journal of the Medical and Physical Sciences. Edited by Drs. KING, JONES, CURREY, WOOD, RAMSAY, and ATCHISON. May, 1854.

The New Orleans Medical and Surgical Journal. Edited by BENNETT DOWLER, M. D. May, 1854.


The Charleston Medical Journal and Review. Edited by D. J. CAIN, M. D., and F. PEYRE PORCHER, M. D. May, 1854.

The Western Medico-Chirurgical Journal. Edited by J. F. SANFORD, M. D. March.

Iowa Medical Journal. Conducted by the Medical Department of Iowa University. May, 1854.

Communications intended for publication, and Books for Review, should be sent, *free of expense*, directed to ISAAC HAYS, M. D., Editor of the American Journal of the Medical Sciences, care of Messrs. Blanchard & Lea, Philadelphia. Parcels directed as above, and (carriage paid) under cover, to John Miller, Henrietta Street, Covent Garden, *London*; or to John Wiley or G. P. Putnam, *New York*; or W. D. Ticknor, *Boston*; or M. Hector Bossange, Lib. quai Voltaire, No. 11, *Paris*, will reach us safely and without delay. We particularly request the attention of our foreign correspondents to the above, as we are often subjected to unnecessary expense for postage and carriage.

All remittances of money, and letters on the *business* of the Journal, should be addressed *exclusively* to the publishers, Messrs. Blanchard & Lea.

 The advertisement-sheet belongs to the business department of the Journal, and all communications for it should be made to the publishers.

CONTENTS

OF THE

AMERICAN JOURNAL

OF THE

MEDICAL SCIENCES.

NO. LV. NEW SERIES.

JULY, 1854.

ORIGINAL COMMUNICATIONS.

MEMOIRS AND CASES.

ART.	PAGE
I. Statistical Account of the Cases of Amputation performed at the Pennsylvania Hospital from January 1, 1840, to January 1, 1850. By George W. Norris, M. D., Surgeon to the Hospital. - - - - -	13
II. On the Climate and Salubrity of Fort Moultrie and Sullivan's Island, Charleston Harbour, S. C., with Incidental Remarks on the Yellow Fever of the City of Charleston. By John B. Porter, M. D., Surgeon, U. S. Army. - - - - -	21
III. On the Movements of the Glottis in Respiration. By John C. Dalton, Jr., M. D. New York. (With two wood-cuts.) - - - - -	75
IV. Extracts from the Records of the Boston Society for Medical Improvement. By Wm. W. Morland, M. D., Secretary. - - - - -	79
V. Observations on the Pathology of Cases of Yellow Fever, admitted into the Pennsylvania Hospital during the Summer of 1853. By Thomas Hewson Bache, M. D. - - - - -	121
VI. Case of very extensive Fistula, successfully treated by a New Method. By Horatio G. Jameson, Sen., M. D., formerly of Baltimore, Maryland, now of Philadelphia. - - - - -	129
VII. Cases of False Anchylosis of the Lower Jaw. By C. S. Fenner, M. D., of Memphis, Tenn. - - - - -	131
VIII. A Case of Hydrometra. By Lewis Shanks, M. D., Professor of Obstetrics, in the Memphis Medical College. - - - - -	133

REVIEWS.

IX. Human Ovulation and Menstruation. (Beiträge zur Lehre von der Menstruation und Befruchtung.) Prof. Dr. Th. L. W. Bischoff. In Henle and Pfeuffer's Zeitschrift für rationelle Medecin, IV. (N. S.) Hft. i. pp. 129-175. 1853. - - - - -	137
X. An Enquiry into the Pathological Importance of Ulceration of the Os Uteri. Being the Croonian Lectures for the year 1854. By Charles West, M. D., Fellow of the Royal College of Physicians; Physician Accoucheur to St. Bartholomew's Hospital; and Physician to the Hospital for Sick Children, London, 1854. 8vo. pp. 95. - - - - -	146

BIBLIOGRAPHICAL NOTICES.

ART.	PAGE
XI. Clinical Report on Dysentery, based upon an Analysis of Forty-nine Cases, with Remarks on the Causation, Pathology, and Management of the Disease. By Austin Flint, M. D. Buffalo, 1853—pp. 90.	
Clinical Report on Chronic Pleurisy based on an Analysis of Forty-seven Cases. By the same—pp. 58.	155
XII. First Annual Report to the General Assembly of Kentucky relating to the Registry and Returns of Births, Marriages, and Deaths, from January 1 to December 31, 1852.	157
XIII. Lectures on the Diseases of Infancy and Childhood. By Charles West, M. D., Fellow of the Royal College of Physicians, Physician to the Hospital of Sick Children, Physician-Accoucheur to, and Lecturer on Midwifery at, Saint Bartholomew's Hospital. Second American from the second and enlarged London edition. Philadelphia: Blanchard & Lea, 1854. 8vo. pp. 486.	168
XIV. The Diseases of the Heart and the Aorta. By William Stokes, Regius Professor of Physic in the University of Dublin, Author of "The Treatment and Diagnosis of the Diseases of the Chest," &c. Philadelphia: Lindsay & Blakiston, 1854. 8vo. pp. 689.	169
XV. Practical Observations on Gout and its Complications, and on the Treatment of Joints stiffened by Gouty Deposits. By T. Spencer Wells, Fellow of the Royal College of Surgeons of England, Member of the Royal Institution of Great Britain, of the Pathological and Epidemiological Societies, &c., late Assistant Surgeon in Malta Hospital. London: 1854. 12mo. pp. 288.	174
XVI. Statistical Reports on the Health of the Navy (British) for the Years 1837 to 1843, including Seven Years. Part II., East India Station. Presented to Parliament, June 1, 1853. Folio, 94 pp.	179
XVII. History of the Yellow Fever at New Orleans, La., in 1853. By E. D. Fenner, M. D., one of the Physicians to the New Orleans Charity Hospital, &c. &c. New York: 1854. 8vo. 84 pp.	184
XIX.—Woman; her Diseases and Remedies. A Series of Letters to his Class. By Charles D. Meigs, M. D., Professor of Midwifery, and the Diseases of Women and Children, in the Jefferson Medical College at Philadelphia, &c. &c. Third edition, revised and enlarged. Blanchard & Lea, Philadelphia, 1854. 8vo. pp. 672.	192
XX. Handbook of Chemistry, Theoretical, Practical, and Technical. By F. Abel, Professor of Chemistry at the Royal Military Academy, Woolwich, &c., and C. L. Bloxam, formerly First Assistant of the Royal College of Chemistry, with a Preface by Dr. Hoffmann, and numerous illustrations on wood. Philadelphia: Blanchard & Lea, 1854. 8vo. pp. 681.	192
XXI. Elementary Chemistry, Theoretical and Practical. By George Fownes, F. R. S. Edited, with additions, by Robert Bridges, M. D., Professor of Chemistry in the Philadelphia College of Pharmacy. A new American from the last revised London edition. Philadelphia: Blanchard & Lea, 1853. 12mo. pp. 555.	193
XXII. Remarks on the Origin of the Yellow Fever which prevailed in Philadelphia, in 1853. R. La Roche, M. D. (Transactions of the College of Physicians of Philadelphia, Vol. II. N. S., No. IV.)	194
XXIII. The Mastodon Giganteus of North America. By John C. Warren, M. D., late Professor of Anatomy in the University of Cambridge; President of the Boston Society of Natural History; Member of the American Philosophical Society; of the American Academy of Arts and Sciences; of the Philadelphia Academy of Natural Sciences; of the Academy of Naples and Medical Society of Florence; Honorary Member of the Medico-Chirurgical Society of London; Corresponding Member of the Academy of Medicine of Paris, &c. &c. Boston, 1852. 4to. pp. 219. With twenty-seven Plates.	

- Address to the Boston Society of Natural History. By John C. Warren, M. D., President of the Society. Boston, 1853. 8vo. pp. 48.
- Remarks on some Fossil Impressions in the Sandstone Rocks of Connecticut River. By John C. Warren, M. D., &c. Boston, 1854. 8vo. pp. 54. With a Plate.
- The Preservation of Health. With Remarks on Constipation, Old Age, and Use of Alcohol in the Preparation of Medicines. By John C. Warren, M. D., &c. &c. Boston, 1854. 8vo. pp. 140. - - - - - 208

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES IN THE MEDICAL SCIENCES.

FOREIGN INTELLIGENCE.

ANATOMY AND PHYSIOLOGY.

	PAGE		PAGE
1. Structure of the Spleen. By Mr. Huxley. - - -	207	Livers of the Mammalia and Birds. By Dr. Gibb. - -	211
2. Vascular Tissue of the Schneiderian Mucous Membrane. By Dr. Kohlrausch. - -	208	9. On the Deposit of Fat in Certain Conditions of the Bodies of the Lower Animals. By Dr. Crisp. - - -	212
3. Mucous Membrane of the Stomach. By Ecker. - -	208	10. Bernard on the Conditions under which Certain Substances, usually retained in the Blood, pass into the Urine. - -	212
4. The Anatomy and Surgical Relations of the Fascia of Scarpa. By Mr. J. Struthers. - -	208	11. Case of Diarrhoea Adiposa, confirmative of Bernard's Views, with regard to the Functions of the Pancreas. By Mr. Marston. -	212
5. The Process of Repair in Teeth. By Dr. S. J. A. Salter. - -	209	12. The Effects of the Recumbent Position during Syncope, Physiologically Considered. By Mr. Richardson, - - -	214
6. Elasticity of Arteries considered as a Cause of Animal Heat. By Dr. Winn. - - -	209		
7. Starch in the Brain. By Mr. Busk. - - -	210		
8. On the Relation that Fat bears to the Presence of Sugar in the			

MATERIA MEDICA AND PHARMACY.

13. Local Anæsthesia. By Dr. Hardy. - - -	215	15. Modes of Exhibiting Cod-liver Oil. - - -	218
14. Production of Local Anæsthesia. By Dr. Snow. - - -	217	16. Medicinal Constituents of the Lemon. By Dr. Cogswell. -	219

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

17. Pyrosis, its Causes, Pathology, and Treatment. By Dr. George Budd. - - -	221	18. Treatment of Diabetes. By Goolden. - - -	222
		19. Topical Uses of Iodine. -	223

	PAGE		PAGE
20. Abortive Treatment of Small-pox by Zinc Plaster. By Dr. John Hughes Bennett. - -	224	29. Report regarding the Cases of Hydrophobia which occurred in France during the year 1852. By M. Ambroise Tardieu. -	232
21. Formula for Gouty, Rheumatic, and Neuralgic Affections. By Dr. Gafford. - - -	225	30. Hydrophobia in a Cretin—Remarkable Development of the Intelligence during the Paroxysms of the Disease. By M. Niepce. - - -	233
22. Cotyledon Umbilicus in Epilepsy. By W. H. Ranking, M.D. - - -	225	31. Hydrophobia Five Years after the Bite—Death—Autopsy. -	234
23. Oxide of Zinc and Hyoscyamus for arresting Colliquative Sweatings in Phthisis. By Dr. Theophilus Thompson. - - -	225	32. Morbid Changes in the Mucous Membrane of the Stomach. By Dr. Hanfield Jones. - -	237
24. On the Influence of Belladonna in counteracting the Poisonous Effects of Opium. By Dr. Thos. Anderson. - - -	225	33. Waxy Degeneration. By Dr. W. T. Gairdner. - - -	239
25. Local Use of Veratria, especially in Scrofulous Affections of Joints. By Dr. J. B. Klingner. - - -	226	34. Metastasis, in its Practical Bearings. By Mr. Hunt. -	239
26. Exercise of the Vocal Organs in Ulceration of the Larynx. By M. Trousseau. - - -	229	35. Fatal Effects of Imperfect Union of the Umbilical Vein after Childbirth; Jaundice as the Result of the Imperfect Closure of the Ductus Venosus. By Mr. Henry Lee. - - -	240
27. Neuralgic Headache. By J. Murphy, M.D. - - -	229	36. Portion of Intestine discharged after Intussusception. By Dr. Paterson. - - -	241
28. Hysterical and Spasmodic Cough. By Dr. Andrew Wood.	232		

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

37. Phlebitis, especially in Connection with the Deligation of Veins. By Mr. Weeden Cooke.	241	45. Amputation at the Shoulder-Joint. By Mr. Alexander Ure.	252
38. Popliteal Aneurism; Long-continued Compression—Eventual Solidification of the Sac. By Mr. Poland. - - -	243	46. Amputation at the Knee-Joint. By H. G. Potter, Esq. - -	253
39. New Mode of Treating Ulcers from Irritation of the Nails. By Mr. Ure. - - -	246	47. Excision of the Knee-Joint. By Mr. R. J. Mackenzie. - -	254
40. Gunshot Wound inflicted by the Minie Rifle. By Mr. Tufnel.	246	48. Dieffenbach's Operation for Ununited Fracture. By Mr. Mackenzie. - - -	255
41. Dislocations of both Femora at the same time in the same Subject. By H. L. Prichard, Esq.	247	49. Puncture of the Bladder through the Symphysis Pubis. By Dr. D. Leasure. - - -	256
42. Dislocation Upwards and Backwards of the Metatarsal Bone of the Great Toe alone. By Dr. J. H. Power. - - -	248	50. Ovariectomy, with reference to its Introduction into Legitimate Practice. By Mr. G. B. Childs.	257
43. Radical Cure of Reducible Inguinal Hernia by a New Operation. By Spencer Wells. -	249	51. Treatment of Ovarian Dropsy by Injections of Iodine into the Cysts. By Prof. Simpson. -	259
44. Abnormal Obturator Artery in Femoral Hernia. By Mr. Mackenzie. - - -	251	52. Summary of the Statistics of some of the more important Operations performed in the principal London Hospitals during the last six months of 1853. -	260

MIDWIFERY.

	PAGE		PAGE
53. Double Uterus—Twins. By M. Hohl. - - - - -	263	during Pregnancy—Recovery. By Dr. Gibson. - - - - -	269
54. Chloroform in Midwifery. By S. W. J. Merriman. - - - - -	263	57. On some of the Diseases included under the term "Prolapsus Uteri," their Diagnosis and Treatment. By Dr. Snow Beck. - - - - -	271
55. Cases of Hysteria and Spurious Pregnancy. By Dr. Alexander Keiller. - - - - -	265	58. Vaginal Cystocele mistaken for Prolapsus Uteri—Operation—Cure. By Mr. J. B. Brown. - - - - -	272
56. Rupture of an Ovarian Cyst			

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

59. Order of Succession in the Putfactive Process. By Dr. Casper. - - - - -	274	62. Recovery after taking a Large Dose of Prussic Acid. By W. H. Burnam. - - - - -	276
60. Suicidal Suffocation. - - - - -	275	63. Upon the Legal and Social Rights of Malformed Beings. By J. Bierbaum. - - - - -	276
61. Mistaken Personal Identity. By Dr. Kinlock. - - - - -	275		

MISCELLANEOUS.

64. Meteorological Changes in Relation to Epidemic Diseases. By Dr. Richardson. - - - - -	277
---	-----

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

A Case of Labour, in which the Rupture of the Membranes, and Reclosure of the Os Uteri occurred ten hours after its commencement. By Joseph Parrish, M.D. - - - - -	279	Tapeworm expelled by Infusion of Pumpkin Seeds. By D. Leasure, M. D. - - - - -	281
Hemorrhage from the Bowels in a New-born Infant. Recovery. By W. B. Young, M. D. - - - - -	280	A Remarkable Case of General Emphysema. By E. K. Beaver, M. D. - - - - -	281

DOMESTIC SUMMARY.

Vesico-Vaginal Fistula of Seven Years' Duration cured in Thirteen Days. By Dr. J. Marion Sims. - - - - -	283	treated by Iodide of Potassium. By Dr. J. D. Griscom. - - - - -	286
Vesico-Vaginal Fistula successfully treated. By Dr. N. Bozeman. - - - - -	283	Cold as an Anæsthetic Agent. By Dr. Thos. Wood. - - - - -	287
Varicocele successfully treated. By Dr. Juriah Harriss. - - - - -	284	Apparatus for Accommodation of the Eye to Distinct Vision at Different Distances. By Dr. Von Roth. - - - - -	288
Efficacy of Cod-liver Oil in Consumption. By Prof. Wood. - - - - -	285	Double Monstrosity. By Prof. H. V. N. Miller. - - - - -	289
Lead Diseases treated by the Iodide of Potassium. By Dr. H. S. Swift. - - - - -	286	Twins of different Colour. By Dr. A. F. Attaway. - - - - -	290
Membranous Croup successfully		Report of the Committee on Medical Education to the American Medical Association, in 1853. - - - - -	290
		Note from Dr. La Roche. - - - - -	291



GRADUATES OF JEFFERSON MEDICAL COLLEGE OF PHILADELPHIA,

MARCH, 1854.

At a Public Commencement, held on the 11th of March, 1854, the degree of DOCTOR OF MEDICINE was conferred on the following gentlemen by the HON. EDWARD KING, LL. D., President of the Institution; after which a Charge to the Graduates was delivered by PROFESSOR ROBLEY DUNGLISON.

NAME.	STATE.	SUBJECT OF THESIS.
Abbott, Luther J.	Ohio.	Opium.
Allison, Thomas H.	Pennsylvania.	Dysentery.
Archer, Edward C.	Virginia.	Scarlatina.
Armstrong, Richard	Pennsylvania.	Clinical Practice.
Austin, William	South Carolina.	Dysentery.
Baptist, William H.	Alabama.	Typhoid Fever.
Barnes, Henry F.	Indiana.	Strangulated Inguinal Hernia.
Barret, R. Layton	Virginia.	Typhoid Fever.
Barr, Robert	Pennsylvania.	Phenomena of Inflammation.
Bartleson, Samuel P.	Pennsylvania.	Diabetes.
Bates, Thomas B.	South Carolina.	Conduct of a Physician.
Bates, Thomas J.	Virginia.	Remittent Fever.
Bell, James M.	Mississippi.	{ Inquiry into the Nature and Treatment of Poisoning by Reptiles.
Bellangee, J. Barton	New Jersey.	Enteromeseenteric Fever.
Berryhill, Samuel G.	Pennsylvania.	Enteromeseenteric Fever.
Bethune, Roderick A.	Alabama.	Mania à Potu.
Birdsong, Miles J.	Texas.	Menstruation.
Bishop, J. Leander	Nova Scotia.	{ Obligations of Medicine to Chemical Science.
Blake, Joseph C.	North Carolina.	Anatomy of the Human Liver.
Bloodgood, Delavan	New York.	Emansio Mensium.
Bloom, Benjamin F.	Alabama.	Pneumonia.
Booton, John G.	Virginia.	Crural Phlebitis.
Bosbyshell, Charles B.	Illinois.	Typhoid Fever.
Bosset, William C.	Pennsylvania.	Acute Dysentery.
Boughan, John F.	Virginia.	Dyspepsia.
Bowes, George A.	Pennsylvania.	Cynanche Trachealis.
Bowers, James A.	Tennessee.	Epidemic Dysentery.
Brandt, Jeremiah	Pennsylvania.	Erysipelas.
Brown, Henry T.	Virginia.	Dysentery.
Buck, Erastus, Jr.	New York.	Cold, as a Morbific and Remedial Agent.
Burnell, Thomas H.	England.	Incised Wounds.
Burrongs, Alphonso J. L.	Georgia.	{ Remedial Influence of General Blood- letting in Fevers.
Bush, Robert H.	Virginia.	The Liver and its Functions.
Camden, Thomas B.	Virginia.	Acute Dysentery.
Carlton, Benjamin F.	Georgia.	Cholera Infantum.
Carter, James W.	Virginia.	{ The Atmosphere, and its Influence over the Animal Economy.
Cass, Edward	Ohio.	Spermatorrhœa.
Chancellor, Edward L.	Virginia.	Pathological Relations of the Blood.
Chase, Enos G.	New York.	Ovarian Tumors.
Chenault, Robert C.	Kentucky.	The Female Pelvis.
Chipman, James L.	Nova Scotia.	Remedies for Inflammation.
Clark, Samuel V.	Mississippi.	Pleurisy.
Clarke, Rowan	Pennsylvania.	Abortion.
Clements, G. Myrick	Georgia.	Delirium Tremens.
Cole, Edward C.	Virginia.	Bilious Remittent Fever.
Cole, Howson W.	North Carolina.	Incised Wounds.
Coleman, Asa	Indiana.	Intermittent Fever.
Collins, Richard T.	Kentucky.	Etiology of Typhoid Fever.
Compton, William M.	Mississippi.	{ Relative Value of Physical and Vital Signs as Means of Diagnosis in Diseases of the Lungs.
Conant, Oscar F.	Mississippi.	Dysentery.
Cooke, William T. B.	Virginia.	Gastrô-hysterotomy.
Corley, James A.	South Carolina.	Miasmatic Fever.
Craig, John T.	South Carolina.	Enteric Fever.
Crawford, William H.	Alabama.	Intermittent Fever.
Crow, Calvin A.	Alabama.	Scarlatina.
Daingerfield, John Elliot	Virginia.	Dysentery.
Davis, Miranda G.	Mississippi.	Symptoms of Pneumonia.
Davidson, William J.	Virginia.	The Veins and their Diseases.
Dean, Richard C.	New Jersey.	The Classics of Anatomy.
Dillard, Thomas H. B.	Virginia.	Diphtheritis.
Drake, William W.	Tennessee.	Typhoid Fever.
Dugger, James M.	Georgia.	Intermittent Fever.
Dwinelle, James E.	New York.	Intermittent Fever.
Earl, John W.	North Carolina.	Dysentery.
Eberhart, George	Georgia.	

NAME.	STATE.	SUBJECT OF THESIS.
Eherle, Jacob K.	Pennsylvania.	Chronic Hepatitis.
Eldridge, Erwin J.	Maryland.	Embryulcia.
Eskridge, John M.	Georgia.	Structure and Functions of the Liver.
Evans, Robert M.	Mississippi.	Femoral Hernia.
Ewell, Joseph F.	Kentucky.	{ Physical Signs of the Diseases of the Heart and its Membranes.
Ewing, James P.	Tennessee.	Phrenitis.
Ewing, William D.	Virginia.	Dyspepsia.
Fennell, James W., Jr.	Alabama.	{ Fatal Termination of an old Burn by Can- cerous Degeneration.
Fithian, Joseph, Jr.	Ohio.	Meluturia.
Fontaine, Abraham W.	Virginia.	Nervous Influence on Organic Action.
Foster, David W.	Mississippi.	Influence of Climate.
Foster, George B.	Massachusetts.	{ Ulcers, their Varieties, Causes, and Treat- ment.
Fruit, Richard B.	Pennsylvania.	Pulsus Arteriosus.
Fulkerson, Putnam S.	Missouri.	Typhoid Fever.
Gay, W. Douglas	Kentucky.	{ Adaptation of the Physical System of Man to the External World.
Gilbert, Silas Terrell	New York.	{ Effects of Mercury when taken into the System, and the Propriety of using it as a Remedial Agent.
Gleeson, John K.	Louisiana.	Puerperal Fever.
Goodell, William	Turkey.	Physiological Correlation.
Goodrich, Robert A.	Virginia.	Typhoid Fever.
Gregory, Oscar	Virginia.	Acute Gastritis.
Griesemer, Calvin H.	Pennsylvania.	Auscultation and Percussion.
Grimes, Franklin T.	Kentucky.	Acute Pleurisy.
Haldeman, George W.	Pennsylvania.	Woman and her Peculiarities.
Hall, A. Douglas	Pennsylvania.	Metro-peritonitis.
Hall, John L.	South Carolina.	Congestive Fever.
Hall, William Hansell	Georgia.	Water.
Halsey, Luther F.	Pennsylvania.	Depletio Sanguinis.
Haring, John J.	New York.	{ Anatomy, Functions, Diseases, and Pa- thological Indications of the Tongue.
Harris, Sampson H.	Mississippi.	Wounds.
Harris, Thomas S.	Virginia.	Mercury, its Oxides and Chlorides.
Harrison, Marcellus T.	Missouri.	Functions of the Spleen.
Hart, Byron	Pennsylvania.	Empiricism.
Hart, William P.	Tennessee.	Propter Uterum Mulier est id quod est.
Harter, M. Lair (M. D.)	Pennsylvania.	Lobelia Inflata.
Haskell, Charles Henry	Massachusetts.	Typhoid Fever.
Head, Joseph (M. D.)	Illinois.	Emetics.
Henderson, Jophanus	Maine.	Laryngitis.
Hendrix, H. Walter	South Carolina.	Intermittent Fever.
Hezlep, William B.	Pennsylvania.	Pneumonia.
Hicks, Edwin S.	Virginia.	Croup.
Hill, Lafayette	Tennessee.	Fœtal Circulation.
Hilleary, John W. (M. D.)	Maryland.	Tracheotomy.
Hillyer, Eben	Georgia.	Hernia Inguinalis.
Hitch, John W.	South Carolina.	Dysentery.
Hoffman, William F.	Pennsylvania.	Uterine Hemorrhage.
Hollifield, Horatio N.	Pennsylvania.	{ Chemical History and Therapeutical Ap- plications of Mercury.
Holman, William P.	Mississippi.	Puerperal Peritonitis.
Holmes, Henry J.	Mississippi.	Cynanche Trachealis.
Homan, John C.	Virginia.	Gastric Digestion.
Hoover, Andrew S.	North Carolina.	Typhoid Fever.
Hopkins, Thomas B.	Texas.	Yellow Fever.
Houston, Armstrong P.	South Carolina.	Absorption.
Howard, William A.	Georgia.	Pathological Anatomy.
Hoyt, William D.	Georgia.	Atelectasis Pulmonum.
Humphrey, William F.	Connecticut.	Preparatory Education of a Physician.
Hunter, Duke W.	Missouri.	Application of Chemistry to Medicine.
Hurt, Munford B.	Virginia.	Pneumonia.
Huston, Robert M. (M. D.)	Virginia.	Jaundice.
Inglesby, William G.	South Carolina.	{ Differential Diagnosis between Typhus and Typhoid Fever.
Ingram, James M.	Tennessee.	Signs of Pregnancy.
Ives, Charles L.	Connecticut.	Auscultation and Percussion.
Jacobson, Edward H.	Pennsylvania.	{ Influence of Exercise on the Health of the Skin.
Johnson, James W.	South Carolina.	Typhoid Fever.
Johnston, Thomas	Pennsylvania.	Pyrosis.
Johnston, William F.	Kentucky.	Gonorrhœa.
Jones, William W.	Kentucky.	Chemical Research.
Kennedy, Stewart	Pennsylvania.	{ Epidemic Cholera, as it prevailed in Chambersburg, Pennsylvania, in 1852. Inflammation.

NAME.	STATE.	SUBJECT OF THESIS.
Kerr, Boyle	Pennsylvania.	Cataract.
Kinnard, Michael C.	Tennessee.	Dysentery.
Knox, William A.	Illinois.	Gun-shot Wounds.
Kurtz, Samuel L.	Pennsylvania.	Insanity.
Lacey, Daniel P.	Pennsylvania.	Hygiene.
Lavery, Theodore C.	Pennsylvania.	Puerperal Fever.
Laws, James	Pennsylvania.	Influence of Climate on Disease.
Lennard, Joseph M.	Alabama.	{ Typhoid Fever as it prevailed in Coosa County, Alabama.
Livingood, Louis A.	Pennsylvania.	Asthma.
Mason, Robert E.	Tennessee.	Pneumonia.
Maxwell, John Hampden	South Carolina.	Structure of the Human Body.
McAllister, John C., Jr.	Pennsylvania.	Amenorrhœa.
McBride, Alexander (M. D.)	Ohio.	Acute Pleuritis.
McClanahan, John P.	Ohio.	Intermittent Fever.
McCormick, John	North Carolina.	Hydræmia.
McCormick, William H.	Pennsylvania.	Scarlatina.
McDonald, Edward H.	Georgia.	Opium.
McDonough, James B.	Pennsylvania.	Dyspepsia.
McDuffie, Hector	North Carolina.	Typhoid Fever.
McKay, Haden E.	Kentucky.	Yellow Fever.
McLatchy, Harris O.	Nova Scotia.	Pleurisy.
McLean, Hugh	Pennsylvania.	Anatomy.
McMannen, Charles T.	North Carolina.	{ Dysentery, as it occurred in North Caro- lina in 1852 and 1853.
McQuiddy, Robert I.	Kentucky.	Pericarditis.
McReynolds, William F.	Kentucky.	Acute Dysentery.
Melton, Robert D.	Alabama.	Philosophy of Death.
Merrill, S. Randolph	New Hampshire.	{ General Character and Treatment of one hundred cases of Intermittent Fever at Patterson, New Jersey.
Miller, George W.	Pennsylvania.	Erysipelas.
Miller, John J.	Pennsylvania.	Puberty, and its Period.
Miller, Luther M.	Pennsylvania.	Syphilis.
Miller, William W.	New York.	Yellow Fever.
Minor, J. Gilmer	Virginia.	Delirium Tremens and its Treatment.
Mitchell, R. Pinckney	Tennessee.	Respiration.
Moore, Dunklin D.	South Carolina.	Dysentery.
Moore, John R.	Pennsylvania.	Puerperal Fever.
Murchison, Kenneth B.	Georgia.	Dysentery.
Nebinger, William P.	Pennsylvania.	Intermittent Fever.
Newton, James F.	Kentucky.	Regions and Viscera of the Abdomen.
Nice, Curtis J.	Pennsylvania.	Traumatic Hemorrhage.
Nixon, Oliver W.	Ohio.	{ Electricity, and its Application as a Re- medy.
O'Brien, John M. (M. D.)	North Carolina.	Traumatic Irritation.
Page, John J.	Kentucky.	Generation.
Paschall, Gideon W.	Kentucky.	Variola.
Patterson, Duncan N.	North Carolina.	Typhoid Fever.
Patteson, Robert P.	Virginia.	Inguinal Hernia.
Pearce, Enoch, Jr.	Ohio.	Dysentery.
Pendleton, Edmund S.	Virginia.	Hernia.
Perkins, Willis M.	North Carolina.	{ Symptoms and Treatment of Acute Dysen- tery.
Peters, Penington L.	Georgia.	Symptoms and Treatment of Dyspepsia.
Plana, Joaquin	Cuba.	{ Mode of detecting the Acid in a Mineral Salt.
Pollard, Leonidas C.	Virginia.	Typhoid Fever.
Pollock, William	Pennsylvania.	Pneumonia.
Potter, Samuel B.	Ohio.	Puerperal Fever.
Primrose, Harry C.	Pennsylvania.	Chemistry.
Pusey, David C. (M. D.)	Kentucky.	Autumnal Fever.
Quesenberry, Vevion	Virginia.	Pleuritis.
Rankin, D. Nevin	Pennsylvania.	Hemorrhage.
Raper, Thomas J.	Pennsylvania.	Intermittent Fever.
Reber, William	Mississippi.	Diagnosis.
Redd, Thomas M.	Kentucky.	The Medical Profession.
Reynolds, Jacob E.	Maryland.	Theory of Inflammation.
Rice, William R.	Virginia.	Acute Laryngitis.
Richardson, Wm. Marshall	North Carolina.	Abortion.
Ringwalt, Levi Z.	Pennsylvania.	Scarlatina.
Robberson, Edwin T.	Missouri.	Empiricism.
Roberts, William C.	North Carolina.	Scarlatina.
Robins, Edwin S. C.	Pennsylvania.	Baptisia Tinctoria.
Rogers, James K.	Pennsylvania.	Entero-mesenteric Fever.
Rogers, Orville T.	Virginia.	Typhoid Fever.
Sanford, Joseph B.	Virginia.	Epidemic Cholera.
Sanford, Leonard J.	Connecticut.	Fever.
Saunders, William M.	Virginia.	Analogy of the two Organisms.

NAME.	STATE.	SUBJECT OF THESIS.
Scales, James M. A.	Tennessee.	Cholera Infantum.
Shepherd, Francis C.	Virginia.	Asthma.
Sheppard, Leander W.	New Jersey.	Hydrophobia.
Shultz, B. Franklin	Pennsylvania.	Croup.
Simpson, Joseph	Delaware.	Constipation.
Sinclair, John	Canada.	Acute Rheumatic Pericarditis.
Slicer, J. Edwin (M. D.)	Virginia.	Fractures.
Smith, Francis F.	Maryland.	Impetigo.
Smith, John W.	Virginia.	Incised Wounds.
Smith, John W. C.	Mississippi.	Protective Power of Vaccination.
Smith, Joseph T.	New York.	Value of Anatomy.
Smith, Richard E. C.	Alabama.	Typhoid Fever.
Smith, Robert S.	Virginia.	Effects of Alcohol.
Smith, William T.	Pennsylvania.	Milk Sickness.
Spann, James T.	Mississippi.	Pleuritis.
Spooner, Edward A.	Massachusetts.	Character of the Physician.
Stokes, N. Newlin	New Jersey.	Membranous Croup.
Stone, Alfred B.	Massachusetts.	Endocarditis.
Storer, John H.	Pennsylvania.	Scarlatina.
Strain, David E.	Virginia.	Femoral Hernia.
Strayer, Joseph B.	Virginia.	Entero-mesenteric or Typhoid Fever.
Stribling, Charles C.	Georgia.	Typhoid Fever.
Swan, Samuel M.	Pennsylvania.	Delirium Tremens.
Taggart, Charles	Ohio.	Intermittent Fever.
Taylor, James W.	Virginia.	Inguinal Hernia.
Taylor, Samuel G.	Virginia.	Spermatorrhœa.
Teague, Thomas J.	South Carolina.	Intermittent Fever.
Teeter, Edwin Conrad	Virginia.	Depletion in Inflammatory Diseases.
Thompson, George W.	Pennsylvania.	Physiology of Digestion.
Thornton, John S.	Pennsylvania.	Cholera Infantum.
Todd, L. Beecher	Kentucky.	Circulation of the Blood.
Tomb, Robert Johnston	Pennsylvania.	Eclampsia Gravidorum. et Parturientium.
Tucker, Gustavus A. R.	Virginia.	Fœtal and Adult Circulation.
Turner, John B.	Georgia.	Mania.
Unsold, John H.	Virginia.	Typhoid Fever.
Van Kirk, Joel K.	Pennsylvania.	Dysentery.
Van Pelt, Joseph T. K.	Pennsylvania.	{ Clinical Record of one hundred and forty- two Cases of Labour.
Van Valzah, Samuel B.	Pennsylvania.	Hæmatemesis.
Vaughan, Bolivar A.	Mississippi.	Morbid Epigenesis.
Waddell, Douglas S.	North Carolina.	Diabetes Mellitus.
Walker, James	Kentucky.	Indigestion.
Walker, Joseph R.	Tennessee.	Circulation of the Blood.
Walker, Mark	New Hampshire.	Character of the Physician.
Walker, Thacker V., Jr.	Georgia.	{ Epidemic Dysentery prevalent in Barbour County, Alabama, in 1853.
Walker, William A.	Georgia.	{ Epidemic Dysentery, as it prevailed in Harris County, Georgia, in 1853.
Wallace, Horatio	Arkansas.	Phthisis Pulmonalis.
Wallace, Jonas C. B.	New York.	Sulphate of Quinia.
Walter, Philip S. P.	Pennsylvania.	Management of Natural Labour.
Walters, William L.	Virginia.	Pneumatosis.
Ware, Augustus C.	Georgia.	Typhous Dysentery.
Warner, Charles F.	New York.	{ The Microscope, its History and Import- ance to the Medical Profession.
Warren, Silas E.	Pennsylvania.	Miasma.
Webb, Robert T.	North Carolina.	{ Properties of Pus, and Theory of Suppu- ration.
West, Nelson G.	Maryland.	Digestion.
Westbrook, Etheldred E.	Mississippi.	Epidemic Cholera of Deer Creek.
Whitaker, Jacob	New Jersey.	Pleuritis.
White, William A.	Virginia.	Electricity.
Wilbur, Lloyd	New Jersey.	{ Importance of General Principles in the Practice of Medicine.
Woods, William Semple	Pennsylvania.	Typhoid Fever.
Woodward, Richard H.	Virginia.	The Circulation.
Workman, Benjamin F.	Mississippi.	Phenomena of Thirst.
Wurts, Charles Stewart, Jr.	Pennsylvania.	Appendages of the Eye.
Wysong, Rutherford	Virginia.	Physiology of Generation.
Young, Edward	Pennsylvania.	Traumatic Tetanus.
Young, Wesley W.	North Carolina.	Intermittent Fever.

[Total 270.]

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THE
AMERICAN JOURNAL
OF THE MEDICAL SCIENCES
FOR JULY 1854.

ART. I.—*Statistical Account of the Cases of Amputation performed at the Pennsylvania Hospital from January 1, 1840, to January 1, 1850.* By
GEORGE W. NORRIS, M. D., Surgeon to the Hospital.

IN the Nos. of this Journal for August, 1838, and May, 1840, we gave statistical tables of all the capital amputations performed at the Pennsylvania Hospital from 1830 to 1839 inclusive, with the view of calling attention to the great mortality which followed these operations. At that time, no statistical accounts of amputations on a large scale done by individuals, or in hospital practice, in this country, were known to me, and only a few loose statements, without any detail of particulars, could be had from Europe. The tables alluded to attracted some notice, and the profession have since been furnished with similar ones from two other American, as well as from several British hospitals. The list now given is a continuation of those which I have already published, carried down for another ten years, viz: to January 1, 1850, and is drawn up in a manner similar to them, with a summary appended of the twenty years' experience of our Institution, together with a comparison of the general results from those American hospitals which have published materials for it.

In these tables, all amputations in which the operation was performed within twenty-four hours after the occurrence of the accident are included under the head of immediate, and the cases treated of this class were generally of the most desperate kind, resulting from railroad accidents, machinery, &c., where the soft parts were as seriously injured as the bones.

No.	Date.	Name.	Age.	Disease or injury.
	1840			
1	May 12	James Bass	24	Compound and comminuted fracture of legs
2	Sept. 20	Henry Sailor	41	Compound and comminuted fracture of leg
	1841			
3	Jan. 16	Peter Hart	39	Lacerated arm
4	May 13	John Resor	22	Caries of knee-joint
5	June 16	David Reeside	38	Compound fracture of leg
6	June 28	William Dern	31	Compound and comminuted fracture of leg
7	July 21	Redman Fellow	45	Compound and comminuted fracture of leg
8	Aug. 22	Patrick Hughes ¹	40	Amputated leg
9	Sept. 23	John Menough	46	Compound and comminuted fracture of legs
10	Sept. 26	Samuel Gilfrey	26	Gunshot wound of hand
11	Oct. 6	Lewis Jones	16	Gunshot wound of hand
12	Dec. 27	George McLaughlin	31	Lacerated and Fractured leg and foot
13	Dec. 31	William Fisher	38	Compound and comminuted fracture of legs
	1842			
14	Jan. 19	James McMullin	60	Deformity of foot
15	March 15	John Slack	27	Compound and comminuted fracture of leg
16	March 25	Hugh Bennet	37	Forearm torn off, and humerus fractured
17	June 27	Patrick Scanlin	21	Forearm torn off, and lacerated and fractured arm
18	July 1	Henry Smith	10	Compound and comminuted fracture of leg
19	Aug. 10	John Kane	28	Compound and comminuted fracture of leg
20	Aug. 16	William Eason	33	Mashed foot
21	July 31	George Wiley	35	Compound fracture of leg
22	Sept. 2	Charles Vise	13	Forearm torn off
23	Sept. 23	Philip Bouvier	38	Gunshot wound of hand
24	Oct. 29	Philip Riley	25	Compound and comminuted fracture of leg
25	Nov. 30	John Smith	31	Compound and comminuted fracture of legs
	1843			
26	April 18	Miller Brown		Caries of elbow
27	July 22	William Wright	46	Compound fracture of forearm, and laceration
28	Aug. 4	William Hardy	10	Comminuted fracture of hand
29	Sept. 17	Henry Gray	18	Gunshot wound of wrist and hand
	1844			
30	Feb. 18	Owen Tighe	25	Compound and comminuted fracture of legs
31	Feb. 22	John Brown	7	Compound and comminuted fracture of leg
32	Feb. 22	William Latham	22	Compound fracture and laceration of hand
33	March 15	John Matthews	42	Comp. fract. and lacer. of wrist and forearm
34	Jan. 17	John McNamee	39	Tumour on the leg
35	June 8	William Smith	5	Fracture and laceration of foot
36	July 8	James Crawford	18	Gunshot fracture of head of humerus
37	July 31	Charles Campbell	23	Compound fracture of leg
38	July 27	Caspar Albert	11	Compound fracture of foot and hand
39	Aug. 10	David Graham	13	Compound fracture of thigh
40	Sept. 11	Edward McHugh	15	Deformed and ulcerated leg
41	Aug. 24	Joseph Wolf	40	Sloughing after fracture of astragalus
42	Sept. 7	Martin Urick	12	Compound fracture of forearm
43	Oct. 11	John Gordon	12	Compound fracture and laceration of foot
44	Oct. 12	Elihu Foster	36	Laceration from bursting of a gun
45	Oct. 7	Thomas Murphy	52	Compound fracture of foot
46	Dec. 6	Henry Damell	16	Compound fracture of arm
47	Dec. 11	Hugh McCarty	10	Compound fracture of hand
	1845			
48	Jan. 16	Jeremiah Cummeey	9	Compound fracture of knee-joint
49	May 20	Miles Griffin	16	Fracture and wound of hand from a gun
50	June 17	Joseph Jackson	25	Compound fracture of leg
51	May 28	Garrick Sharpe	26	Compound fracture of leg
52	Nov. 17	Michael Curwein	16	Arm torn off, and severe contusion of body
53	Sept. 20	Daniel Cliff	27	Lacerated hand and wrist
54	Dec. 6	John Kline	22	Caries of knee-joint
	1846			
55	Jan. 31	Henry Trite	25	Lacerated hand and forearm from a gun
56	Feb. 3	John McAnall	26	Compound fracture of elbow, and dislocation of humerus of same side

¹ The limb was amputated in consequence of an injury, three days before admission, at Havre de Grace.

Part amputated.	Immediate or otherwise.	Result.	Period of discharge or death.	Date of amputation.
			1840	1840.
Leg	Immediate	Died	May 13	
Leg		do.	September 27	September 23.
			1841	1841.
Arm	do.	Cured	March 11	
Thigh		do.	September 7	May 29.
Leg	do.	Died	June 20	
Leg	do.	do.	July 1	
Leg	do.	do.	August 5	
Leg	do.	Cured	February 19	
Leg	do.	Died	September 26	
Forearm	do.	Cured	October 28	
Forearm	do.	do.	October 27	
Leg	do.	do.	January 29, 1842	
Legs	do.	do.	February 25, 1842	
			1842	1842.
Leg		do.	September 27	January 23.
Leg	do.	do.	June 30	
Forearm	do.	do.	May 18	
Shoulder-joint	do.	do.	October 5	
Leg	do.	do.	August 10	
Leg	do.	Died	August 14	
Leg		do.	September 1	
Thigh		Cured	November 5	August 31.
Arm	do.	do.	November 2	
Forearm	do.	do.	October 29	
Leg	do.	do.	March 18	
Legs	do.	Died	December 3	
			1843	1843.
Arm		Cured	September 13	July 19.
Arm	do.	do.	September 16	
Forearm	do.	do.	October 28	
Forearm	do.	do.	October 28	
			1844	1844.
Leg		Died	February 24	February 19.
Thigh	do.	Cured	March 22	
Hand (partial)	do.	do.	April 27	
Forearm	do.	do.	May 15	
Leg		do.	June 24	April 6.
Leg	do.	do.	August 7	
Shoulder-joint	do.	do.	August 20	
Leg		do.	November 6	August 7.
Foot (partial)		Died	August 10	August 7.
Thigh	do.	do.	August 11	
Knee-joint		Cured	March 1	September 17.
Leg		do.	December 2	September 19.
Forearm		do.	November 30	October 9.
Foot (partial)		do.	March 14	October 12.
Forearm	do.	do.	November 11	
Leg		Died	October 19	October 16.
Arm	do.	Cured	February 7	
Forearm	do.	do.	January 25	
			1845	1845.
Thigh	do.	Cured	June 29	
Forearm		do.	June 28	June 4.
Leg	do.	Died	July 6	
Leg		Cured	July 23	June 21.
Arm	do.	Died	November 18	
Forearm	do.	Cured	December 3	
Thigh		do.	March 7	December 31.
			1846	1846.
Forearm	do.	Cured	April 24	
Arm	do.	do.	April 22	

No.	Date.	Name.	Age.	Disease or injury.
57	Jan. 26	James Appleton	41	Simple fracture of thigh, followed by dry gangrene of foot
58	April 6	William Logue		Ulceration from burn, and necrosis of humerus
59	June 10	Jesse Workwell	46	Fractured and lacerated hand
60	June 23	Alexander Murray	16	Lacerated hand from a circular saw
61	July 8	John Glendinning	29	Compound and comminuted fracture of leg
62	July 8	David Fitzsimmons	11	Forearm torn off
63	July 23	David Jones	46	Lacerated and fractured forearm
64	Aug. 28	Jacob Young	49	Compound and comminuted fracture of leg
65	Oct. 2	James McCormack	30	Compound and comminuted fracture of leg
66	Oct. 22	Edmund Wheeler	33	Compound fracture of leg
67	Dec. 31	David Eikins	37	Gunshot wound of hand
	1847			
68	March 25	John Masterson	22	Compound fracture and laceration of feet
69	April 13	William Riggles	23	Compound fracture of leg
70	April 16	John Magee	35	Comminuted fracture of foot
71	May 4	John Campbell	31	Compound fracture of leg and foot
72	May 10	Thomas Higgins	48	Compound fracture of leg
73	May 11	Thomas Dickerson	12	Fracture and laceration of wrist and hand
74	April 27	Mary Goodwin	17	Tumour on leg
75	Feb. 14	William W. Pelham	26	Caries of ankle-joint
76	June 19	James Love	25	Caries of foot
77	June 21	Daniel Holt	18	Compound fracture and laceration of forearm
78	June 25	James Caldwell	43	Compound fracture of leg
79	Aug. 3	George Myers	11	Compound fracture of arm
80	Aug. 15	John Sloan	29	Compound fracture of leg
81	Aug. 24	James Leamy	15	Fracture and laceration of hand
82	Sept. 11	Isaac Willis	16	Compound fracture of ankle
83	Oct. 22	Nero Purnsley	41	Compound and comminuted fracture of leg
84	Sept. 25	Jeremiah Brew	25	Compound fracture of leg
85	Nov. 5	Thomas Fry	15	Gunshot wound of wrist-joint
86	Dec. 7	John Kirks	22	Compound and comminuted fracture of elbow
87	Dec. 6	John Quigley	31	Compound fracture of leg
88	May 5	Margaret Rafferty	23	Caries of ankle and foot
89	Nov. 27	William Walz	32	Hemorrhage after compound fracture of forearm
90	Nov. 5	James C. Riley	17	Caries of knee-joint
91	Dec. 25	Thomas Downs	14	Compound fracture of wrist from bursting a gun
92	Dec. 27	Charles Lun	26	Caries of wrist-joint
	1848			
93	Jan. 17	Amos Kelly	20	Arm torn off by machinery
94	Jan. 17	John Shields	23	Gunshot wound of wrist and hand
95	Feb. 21	George Ludwig	68	Compound and comminuted fracture of arm
96	May 3	James Lithgow	41	Wound from circular saw
97	Feb. 16	Edward McGregor	24	Caries of knee-joint
98	May 23	Robert Simpson	12	Lacerated and fractured arm
99	June 23	Benjamin Murphy	19	Comminuted and lacer. arm, and fractured thigh
100	June 28	Henry McGuigan	43	Comp. fract. and laceration of arm and forearm
101	Aug. 1	William Connell	14	Compound and comminuted fracture of forearm
102	Sept. 21	Martha Lister	30	Caries of knee-joint
103	Nov. 9	Thomas Donahue	9	Lacerated and fractured hand and wrist
104	March 30	Isabella Ford	32	Ununited fracture of thigh
105	Nov. 30	James Nealis	10	Compound and comminuted fracture of leg
106	Dec. 16	John Ziegler	38	Compound and comminuted fracture of foot
	1849			
107	Jan. 10	John Magaw	9	Comp. fract. of leg, and fract. arm and forearm
108	Jan. 26	Matthew Hay	49	Burned hand
109	March 4	Sarah McQuade	45	Burned forearm and arm
110	April 17	Edward Pratt	42	Tumour on the leg
111		Charles Barker	24	Caries of the knee
112	May 11	Matthew D. Neal	45	Comminuted fracture and laceration of hand
113	Aug. 4	Jacob Bachman	46	Compound fracture and dislocation of elbow
114	Sept. 1	Yemens Silverthorn	31	Compound fract. of forearm and arm, and of leg
115	Sept. 27	Martin Low	16	Compound fracture of foot
116	Sept. 22	John McGlover	36	Caries of ankle following compound fracture
117	Dec. 8	Josiah Lewis	36	Compound and comminuted fracture of leg

Part amputated.	Immediate or otherwise.	Result.	Period of discharge or death.	Date of amputation.
Leg		Cured	May 30	March 26.
Arm		Died	May 28	April 15.
Hand (partial)	Immediate	Cured	July 18	
Hand (partial)	do.	do.	August 6	
Leg	do.	do.	September 5	
Arm	do.	do.	September 16	
Arm	do.	do.	October 7	
Leg	do.	Died	October 4	
Thigh	do.	do.	October 15	
Leg		do.	October 27	
Forearm	do.	Cured	February 10 1847	1847.
Feet (partial)	do.	do.	July 1	
Leg	do.	do.	August 18	
Leg	do.	Died	April 30	
Leg	do.	Cured	July 1	
Leg	do.	Died	May 12	
Forearm	do.	Cured	June 17	
Thigh		do.	September 9	May 15.
Leg		do.	June 16	May 15.
Foot (partial)		do.	July 29	June 20.
Forearm	do.	do.	July 21	
Leg	do.	do.	September 18	
Arm	do.	do.	September 18	
Leg	do.	do.	October 23	
Hand (partial)	do.	do.	October 9	
Leg		do.	November 16	September 29.
Leg		Died	November 10	October 23.
Thigh		Cured	February 21	October 30.
Forearm	do.	do.	November 18	
Arm	do.	do.	February 2	
Leg		Died	December 28	December 11.
Leg		Cured	April 1, 1848	December 15.
Arm		Died	December 26	December 16.
Thigh		Cured	April 1, 1848	December 22.
Forearm	do.	do.	February 23	
Forearm		do.	February 23, 1848 1848	January 5, 1848. 1848.
Arm	do.	do.	April 22	
Forearm	do.	do.	March 10	
Arm	do.	do.	May 12	
Forearm	do.	do.	June 23	
Thigh		do.	August 23	May 13.
Shoulder-joint	do.	do.	July 22	
Arm	do.	do.	October 9	
Shoulder-joint	do.	do.	August 25	
Arm	do.	do.	September 29	
Thigh		do.	November 18	October 4.
Forearm	do.	do.	February 4	
Thigh		do.	January 1	November 15.
Thigh	do.	do.	February 3	
Leg	do.	Died	December 21 1849	1849.
Thigh		Cured	April 1	January 20.
Forearm	do.	do.	June 25	
Arm		do.	May 14	March 7.
Leg		do.		April 21.
Thigh		do.	August 22	April 28.
Forearm	do.	do.	July 12	
Arm	do.	do.	September 6	
Arm	do.	do.	October 13	
Foot (partial)	do.	do.	November 20	
Leg		do.	December 1	October 17.
Leg	do.	Died	December 11	

From the above Table it appears that from January, 1840, to January, 1850, there were 120 capital amputations performed on 117 patients. Of these, 16 were of the thigh, 45 of the leg, 1 at the knee, 6 at the feet, 4 at the shoulder-joint, 20 of the arm, 24 of the forearm, and 4 of the hand.

Eighty of the 120 operations were primary, being done for recent injuries within twenty-four hours after the occurrence of the accident, and of these, 63 were cured, and 17 died.

Twenty-one were secondary, of which 12 were cured, and 9 died.

Nineteen were for the cure of chronic affections, of which 18 were cured, and 1 died.

Fourteen of the whole number in the Table were done at the joints, of which 13 were cured, and 1 died.

Fifty-two of the amputations were of the upper extremity, of which 49 were cured, and 3 died.

Sixty-eight were of the lower extremity, of which 42 were cured, and 23 died.

A general summary of the Tables now furnished, and of those heretofore published in the Nos. of this Journal, already referred to for the ten years from 1830 to 1840, gives the following results:—

Of 200 amputations upon 196 patients, performed during the 20 years from 1830 to 1850, 148 were cured, and 48 died.

Of these, 115 were primary, of which 27 died.

“ 41 were secondary, of which 16 died.

“ 44 were for the cure of chronic diseases, of which 5 died.

Eighty-four of the amputations were of the upper extremity, of which 8 died.

One hundred and twelve were of the lower extremity, of which 39 died.

Twenty-one were amputations at the joints, of which 4 died.

Forty-nine of the 196 patients operated on, were under 20 years of age, of whom 45 were cured, and 4 died.

Fifty-six were between 20 and 30, of whom 45 were cured, and 11 died.

Forty-seven were between 30 and 40, of whom 29 were cured, and 18 died.

Thirty-seven were between 40 and 50, of whom 24 were cured, and 13 died.

Five were upward of 50, of whom 4 were cured, and 1 died.

The age of two was not noted; they were both adults.

Summary of the published Tables of the amputations performed in the Pennsylvania, Massachusetts General, and New York City Hospitals:—

	No. of Amputations.	Cured.	Died.
Pennsylvania Hospital ¹ (Jan. 1830 to Jan. 1850)	200 on 196 patients	148	48
Mass. Gen. Hospital ² (Jan. 1822 to Jan. 1850)	146 on 141	109	32
N. Y. City Hospital ³ (Jan. 1839 to April 1851)	154	100	54
	<hr/> 500 on 491	<hr/> 357	<hr/> 134

The following Table exhibits the mortality after operations upon the upper and lower extremities in the three Institutions mentioned :—

1. UPPER EXTREMITY.

	No. of Amputations.	Cured.	Died.
Pennsylvania Hospital	84	76	8
Massachusetts General Hospital	22	19	3
N. Y. Hospital	56	43	13
	<hr/> 162	<hr/> 138	<hr/> 24

2. LOWER EXTREMITY.

	No. of Amputations.	Cured.	Died.
Pennsylvania Hospital	112	73	39
Mass. Gen. Hospital	119	90	29
N. Y. Hospital	100	59	41
	<hr/> 331	<hr/> 222	<hr/> 109

The following shows the mortality after amputations, when done for the cure of chronic diseases and injuries, in the same Hospitals :—

1. CHRONIC DISEASES.

	No. of Amputations.	Cured.	Died.
Pennsylvania Hospital	44	39	5
Mass. Gen. Hospital	85	74	10
N. Y. Hospital	50	37	13
	<hr/> 179	<hr/> 150	<hr/> 28

2. INJURIES.

	No. of Amputations.	Cured.	Died.
Pennsylvania Hospital	153	110	43
Mass. Gen. Hospital	56	34	22
N. Y. Hospital	104	63	41
	<hr/> 313	<hr/> 207	<hr/> 106

No anæsthetic agent was inhaled in any of the cases included in the above Tables from the Pennsylvania Hospital. In many of those treated at the Boston and New York Institutions, these agents were employed.

The effect of anæsthetics upon the mortality, after surgical operations, is a matter of much interest, to which sufficient attention has not yet been given,

¹ Norris, Amer. Journ. of the Med. Sci. vols. xxii. and xxvi., 1838 and 1840.

² Hayward, Amer. Journ. of Med. Sci. vols. xxvi. and xxi. N. S., 1840 and 1851.

³ Buel, Amer. Journ. of Med. Sci. vol. xvi. N. S., 1848, and Lente, Trans. of Amer. Med. Assoc. vol. iv. Philad., 1851.

and it is desirable for this purpose that the results of operations done on a large scale, with and without them, should be furnished. Dr. Lente, in his Report from the New York Hospital, has noticed the greater mortality after amputations in the series of cases collected by him (1848 to 1851), than in those furnished from the same Institution by the previous reporter, Dr. Buel (1839 to 1848). The Tables of the former giving 39.68 per cent., while those of the latter show only 28.57—a great difference in favour of the latter. To explain it, he remarks: “We have coincidences of fatality, when almost all our operations will prove fatal in spite of us, and coincidences of success, when the most desperate cases will recover, and few or none die. We have coincidences of certain accidents, and often those reputed the most rare, will happen, one after another in quick succession, and so through all the branches of surgery. Therefore the difference in mortality might be attributed to the occurrence of one of these coincidences; and we may look for an unusual success during the next two or three years.” Immediately afterwards, he adds: “*Anæsthetics* came into general use about the period of the commencement of these statistics. May not the employment of these have had its influence upon the mortality? We do not deny that it may have had some influence in augmenting the fatality of operations, but we have seen no reason to infer that it has, except, perhaps, the fact that *union by adhesion* seems to have been much less frequent since the introduction of anæsthetics into this hospital than before.”

The last Table of Dr. Hayward, too, seems rather adverse to the use of anæsthetics in amputations, since of 52 successive operations in which they were employed, 13 died, or 1 in 4; while of the 22 immediately preceding to whom they were not administered, only 4 died, or 1 in 5½. It must be remarked, however, that Dr. Hayward does not himself think that inhalation had anything to do with their increased mortality, for in speaking of it, he says: “It may not be amiss to add that no fatal effects have followed their administration, nor has any serious ill consequence in a single instance ensued from it.”

ART. II.—*On the Climate and Salubrity of Fort Moultrie and Sullivan's Island, Charleston Harbour, S. C.; with Incidental Remarks on the Yellow Fever of the City of Charleston.* By JOHN B. PORTER, M. D., Surgeon, U. S. Army.

THE substance of the following observations is taken from a Special Report to the Surgeon-General of the Army, dated March 31, 1854.

In large cities it is often difficult to trace the origin of disease; but this is not the case in small places, where the movements of every person can be easily ascertained. This is especially the fact in relation to the yellow fever of 1852, on Sullivan's Island and at Fort Moultrie, the movements of every person, particularly if attached to the garrison in any capacity whatever, being easily traced at the commencement of the epidemic, as well as both previously and subsequently.

Sullivan's Island is situated in what geologists term the *Charleston basin*; and we will here quote sufficient for the purpose of illustrating the medical topography of the island and vicinity, from the instructive paper of Dr. Geddings on the medical topography of South Carolina.

"In physical aspect, as well as in the essential elements of the soil, a wide difference exists between the lower and upper regions of the State. In the former, the country is, for the most part, flat, and as the streams have but little fall, rendering drainage difficult, the earth is generally surcharged with water to within a few feet of the surface, and at many points extensive bay-galls or ponds exist, which, in rainy seasons especially, are completely filled with water, and often remain so for several months in the year; but in periods of drought, they, as well as many of the streams fed by them, become completely dry. *

* * The swamps, or marshes, within the limits of salt water (and the same is true of the margin of the ocean generally), consists of a profound bed of blue mud, containing a large amount of vegetable matter, and generally covered by a luxuriant growth of marsh sedge (*Spartina glabra*). Immense tracts of this kind exist throughout this entire region of country, which in many places present vast level, prairie-like plains, as far as the eye can see, nearly the whole of which are overflowed by high tides. * * * Examined in its totality, in relation to its geological character, South Carolina may be divided into two parts of nearly equal extent. The first, comprising all the lower part of the State, and extending from the shores of the ocean to a distance of a hundred, or a hundred and twenty miles into the interior, belongs exclusively to the tertiary formation, in which all the essential features of the several divisions of that geological epoch are found more or less strikingly characterized at different points.

"To enter into a minute description of the relative position and extent of these formations belonging to the tertiary period, would be of little interest in a medico-topographical point of view. It may be remarked that the fossiliferous beds, composing the different formations of the tertiary, have interposed between them variable layers of sand, mud, and, in many places, clay, which give them great irregularity, and often render it difficult to trace their precise limits. * * * The sand-beds of the buhrstone are intermingled with those of the pliocene, which pass into the superincumbent beds of the pliocene, and the latter are, in turn, blended with the moving sands of the coast. The cretaceous formation is found underlying the tertiary, and while all these overlap the granite, at the boundary line above alluded to, as forming the limit between the tertiary and primitive regions of the State, we find the calcareous beds ex-

tending from the line of the buhrstone towards the ocean, forming an immense deposit of several hundred feet in thickness, constituting what Prof. Tuomy has designated the *Charleston basin*, which he supposes to extend over an area of seventy-five miles in length, by sixty in breadth. A great portion of this peculiar formation is composed of an immense bed of marl, intermixed with fossil shells, and various other organic remains. It underlays the city of Charleston, and extends out into the ocean, and is found near the coast, at variable depths below the surface, generally from forty to sixty feet. It has been perforated by the borings of the artesian well, now in progress in the city, and is there several hundred feet in thickness.

"The leading characteristics of the sea-coast, with its numerous islands, inlets, and estuaries, have been already alluded to in our general remarks on the physical features of the State. In a geological point of view, however, the margin of the ocean presents some notable peculiarities which deserve a passing remark. In general terms, it may be remarked that so far as the superficies is concerned, the whole of this region consists, for the most part, of vast areas of light, fine sand, marsh-mud, and occasionally of clay to a limited extent, superimposed upon the post pliocene beds; the latter, in many situations being exposed, or very near the surface. This is the most general character, not only of the numerous islands, but likewise of the principal part of the main land bordering immediately upon the ocean. In consequence of these characters and formations, exposed as they are to an influence exercised upon them by the numerous rivers emptying into the ocean, on the one hand, and to, perhaps, a still greater modifying agency from the winds and tides, on the other, are more liable than any other geological formations in the State, to great and sudden changes of form and general arrangement."

Sullivan's Island is situated on the north side of the small bay which forms Charleston Harbour, making a part of it; the west end or cove being between four and five miles from the City of Charleston, and extending to the east about three miles, being from one-quarter of a mile to one and a half or two miles in breadth. It is a sand island, and is but slightly elevated above the level of the sea, severe storms having been known to carry the waves over it so as almost to submerge it. Boats have been rowed from Fort Moultrie down Middle Street, within a recent period, and the residents of the island were obliged to repair to the fort for shelter. After the general medical topography of the low country by Dr. Geddings, as quoted, that of Sullivan's Island, sufficient for all practical purposes to the physician, is soon given: sand on the surface, and black sand or mud below this. In dry seasons, water is found from a few inches to two or three feet beneath the surface; and in wet seasons, the whole island, except the hills of sand-drift, seems to be nearly under water. From the main land on the north the island is separated by a channel about three-quarters of a mile wide at high tide, which is nearly fordable at low water.

Fort Moultrie is on the south side of the island, on the main channel or entrance into Charleston harbour, being nearly one mile from the cove, and between five and six miles from the city. Charleston (St. Michael's Church) is in lat. $32^{\circ} 46' 33''$ N.; long. $79^{\circ} 55' 38''$ W. By the observations of the Coast Survey, Fort Moultrie is in lat. $32^{\circ} 45' 31''$ N.; long. $79^{\circ} 51' 15''$ W. The fort is surrounded on three sides by the village of Moultrieville.

"To prove that yellow fever can be imported into a place, I should take an island in the middle of the ocean; I would surround it with other islands pos-

sessing the same characteristics of climate and soil, and peopled by the same race. I would have this island healthy for at least fifty years. Then I would have arrive at it a ship scourged with yellow fever; this ship should implore succor, and it should be granted; yet, as a wise precaution, some kind of quarantine should be established. Of all the inhabitants of the island, some few only should be brought into the immediate presence of the sick. Within the period fixed by experience as the stage of incubation of this disease, one or two of these few, and these only, should begin to suffer from the same disease; then from these sufferers the disease should gradually spread as from a centre, until the whole island was infected. While, however, this one island was thus ravaged, the adjoining islands which the infected vessel had not visited should remain perfectly free, in order to prove that there was no (so styled) epidemic constitution. Then, if all these conditions were fulfilled, I would confess that yellow fever *may* be contagious, and *can* be imported."—*Brit. and For. Med.-Chir. Rev.* Oct. 1852, p. 215.

The same Review vouches the truth of the foregoing supposed circumstances at Boa Vista, even to the health "for at least fifty years," for "no epidemic has ever been recorded until this." If Boa Vista, situated within the tropics, in lat. $16^{\circ} 5' N.$, has been without an epidemic fever for fifty years, until the arrival of the *Eclair*, Sullivan's Island, more than $32^{\circ} N.$ of the equator, and more than $90^{\circ} N.$ of the Tropic of Cancer, must be still more salubrious, for, "no epidemic has ever been recorded;" and there has been so much said about its salubrity; its health is annually announced with such a flourish of trumpets by the Charleston newspapers and their correspondents; and the Army Medical Statistics have been so often invoked in its favour; that, for all epidemics, in all time, some arrival from Castle Pinckney or Charleston, some Bann or *Eclair*, will surely be found to convey the "germ of contagion" to this favoured spot, which, left to itself, would have never known disease or death. A brief medical history of Sullivan's Island, previous to the epidemic yellow fever of 1852, may not be unacceptable.

There are three classes of advocates for the salubrity of this island, viz: 1. Those who assert that no disease can originate here. 2. Those who assert that no fever can originate. 3. Those who assert that yellow fever cannot possibly originate. We shall see.

Dr. B. B. Strobel, of Charleston, a strong advocate for the *transmissibility* of yellow fever—in other words, a strong contagionist—gives the following account in his Essay:—

"Sullivan's Island is a mere sand-bank free from ponds, marshes, and vegetation, constantly exposed to the refreshing breezes of the sea, and resorted to by strangers and persons of leisure as a delightful retreat from the noise and bustle of the city. Now, according to the reasoning of our adversaries, it is impossible that yellow fever can be generated there from local causes. Yet instances are on record of its having prevailed there epidemically in 1817, and if we mistake not in 1838-39.¹ Be this as it may, we cannot be mistaken as to the occurrences of 1817, for we distinctly recollect that an attempt was then made to attribute the fever to some local cause, such as a gutter, or a small pond. The yellow fever had not prevailed in Charleston from the year 1807 up

¹ We are permitted to cite one case, that of a young lady who came from the country, and proceeded to Sullivan's Island before the fever prevailed in Charleston. She took the fever and was very ill.

to this time, a period of ten years. Upon the revival of commerce, however, at the close of the war of 1812, a sudden irruption of yellow fever took place, at a time when the city was filled with strangers. Sullivan's Island was then considered as a safe retreat from the fever, and a very large number of persons went there in hopes to avoid it—many of them were in indigent circumstances, and of course crowded together in narrow, confined apartments. Under these circumstances the fever was *transmitted* from Charleston to that place, and many persons died of it." Pp. 200, 201.

"*Under these circumstances,*" that is, the presence of persons in "indigent circumstances," and these persons "crowded together in narrow, confined apartments," "*the fever was transmitted from Charleston.*" These persons were in the worst possible situation, in a state of fear and anxiety, in narrow and confined apartments, crowded together in hot weather, and in a filthy condition; for poverty and filth, somehow or other, are almost inseparable. Indeed, crowded together in this way, it would be nearly impossible for a better class of people to keep themselves comfortable or decent. We have here the efficient causes of malignant fever, which, in the colder climate of the northern States, or Ireland, would produce typhoid or typhus fever, and which, in the meridian of Sullivan's Island did, in my opinion, produce yellow fever.

Again, in 1817, Dr. Strobel says:—

"Many persons arriving in our harbour, and learning the condition of the city, proceeded from the vessels immediately to the island, where they sickened and died." P. 201.

Certainly, these persons could not have *transmitted* the fever from Charleston to the Island, for they were never in the city, but proceeded directly from the vessels as soon as they arrived in the harbour. How came they to sicken and die of yellow fever on Sullivan's Island?

The young lady from the country, spoken of by Dr. Strobel, could not have *transmitted* the disease from Charleston, in 1839, for she proceeded to Sullivan's Island before the fever prevailed in Charleston. On Sullivan's Island "she took the fever and was very ill." Dr. Lebbly stated to me recently that several cases of severe fever occurred in 1839, back of Fort Moultrie, among females who had not left the Island since April. We shall recur to the fevers of this year again.

It appears, then, that the first yellow fever of Sullivan's Island on record occurred in 1817, and, from the evidence supplied by Dr. Strobel himself, I have little hesitation in saying that the disease originated there.

In 1824, yellow fever again prevailed on Sullivan's Island. This epidemic was of a very malignant character. In proof of this assertion, we will refer to the valuable paper on this awful epidemic, by Dr. Thomas Y. Simons, in the *Carolina Journal*, for January, 1825.

"Independent of the distresses which occurred in the city, the disease broke out with dreadful malignancy among those who had sought refuge from its ravages on Sullivan's Island, a complete sand-bank, about six miles from the city, which has hitherto been considered a secure retreat." P. 3.

This epidemic was much more severe among the inhabitants and residents of the Island than in the garrison of Fort Moultrie. "On referring back to 1824, it is found that, whilst this disease prevailed with great malignity in the city, not more than twelve cases, none of which proved fatal, appeared on the Island, in a strength of seventy." (*Army Med. Statistics*, p. 201.) The number sick in garrison was at the rate of 171 per 1,000, with not a single death, while the residents of the Island outside of the garrison were severely afflicted, and the mortality was very great. We shall return to this subject.

"In the third quarter of 1834, there are five cases of febris icterodes reported, two of which proved fatal. Of these cases, two originated in Charleston, and the other three at Castle Pinckney."—*Army Med. Stats.* p. 201.

We will transcribe the important letter of Dr. Lebbey to Dr. Strobel, which is interesting in relation to the fevers of Sullivan's Island, and other points, as also in connection with the subject of contagion.

FORT MOULTRIE, January 16, 1840.

DEAR SIR: In reply to your letter of the 6th inst., requesting any information in my possession relative to the fever which has prevailed in the city of Charleston for several seasons past, I have to inform you, that the first cases of yellow fever came under my notice in August, 1827, at Fort Johnson. Three men belonging to a company of United States artillery, stationed at that post, remained a night in the city during the existence of yellow or stranger's fever. On the ninth day after, they were attacked with the disease, within a few hours of each other. There was no doubt of the nature of the disease, it being a fever of but one paroxysm, and yielded on the sixth day favourably. These individuals were strangers to the climate, and had never been here before. They were natives of Germany, Ireland, and Massachusetts. They were not removed from their quarters (the hospital at the time being out of repair); and during the period of their sickness, ten or twelve men slept in the same room with them, who were alike strangers to the climate; yet there were no other cases of disease in the garrison. The next occurrence of the disease was in 1831, at Castle Pinckney. One of the Irish labourers died in the city of supposed yellow fever. This was an intemperate man. Several other cases of fever occurred afterwards, among the labourers, who were brought to the hospital at Fort Johnson; but, according to my humble opinion, they proved to be bilious remittent fever, were treated as such, and recovered.

In 1832, the disease appeared in the city of Charleston, and from two or three of the first cases being traced to the Irish labourers at the castle, it was supposed to have originated there; and, if I recollect aright, I think a deputation of the medical members of the Board of Health were sent over to ascertain, if possible, the cause or causes of its origin. It was ascribed by those gentlemen to a quantity of shells taken from the adjacent oyster banks, for the purpose of filling up the parade-ground. From this opinion, however, coming as it did from so highly respectable a source, and from gentlemen of known scientific attainments, I differed at the time, with due deference to their experience. And in obedience to an order of the commanding officer of the engineer department, I examined the premises, and was of opinion that the causes of the fever were the opening of two privies in the north wall, which had been bricked up for a number of years; their contents, in a decomposed state, taken out in the month of August, and exposed to the action of the sun on the bank near by—together with the exposure of the lower story of the quarters to the action of the atmosphere—it being necessary to rip up the floors, which had sunk down to the surface of the ground, and had remained in that state for some years. It will also be recollected that this fort had been used for some time previous as a lazaretto. The exposure of a surface which had been so long protected from the action of the sun's rays, is it not more than probable that when thus exposed the exhalations therefrom would engender disease of some kind? In

this instance, likewise, every one of the cases brought to Fort Johnson was bilious remittent fever. Among those attacked were Lieutenant (now Captain) Brewerton, the master carpenter, two masons, and several black labourers. Yellow fever prevailed in Charleston that season, I think, to some extent. The venerable Dr. Philip G. Prioleau attended Captain Brewerton with me as a consulting physician, who may perhaps recollect the case.

The next occurrence of fever under my notice, and which was confirmed yellow fever, was in September, 1838, the mate of one of the lighters, an Englishman by birth, about eighteen years of age, who had been in the harbour about seven or eight months. He slept one night in the city, in September; on the fourth day he sickened, and although he was among children at Sullivan's Island, and others who were liable to contract the disease, no other instance of it occurred in the family. He recovered.

The next occurrence of the fever was on the 16th of June, 1839.¹ Captain R—, of the U. S. lighter Valiant, lying in the cove of Sullivan's Island, was taken on this date. He reported at the hospital, Fort Johnson, on June. This man had not been in the city, from all the information I could obtain, for twelve days at least previous to his attack. From the history he gave of himself, and the symptoms exhibited, I was of opinion that he was labouring under intermittent fever, resembling that form commonly known as country fever. In the evening there was a distinct intermission, the fever going off with copious perspiration. Between this period and the evening of the 21st, there were two distinct paroxysms. On the morning of the 22d it assumed the continued form; on the 23d he commenced ejecting black vomit, bleeding from his blisters, mouth, and rectum; and on the 24th he died.

The next case admitted was on the 20th of June, from the lighter Sea Flower, lying at the wharf at Fort Johnson. This vessel had been engaged for some time in transporting materials from this post to Fort Moultrie; the captain had not been to the city for many weeks. His symptoms were exactly like the first at its commencement—the paroxysms distinct, assuming the double tertian form, and going off in profuse perspiration, occasioning great prostration—it yielded to a very energetic treatment, although there was great gastric inflammation, and some hemorrhage from the bowels. He was a very temperate, regular man in his habits, while the former was the reverse.

The third case was admitted on the 22d of June. This individual was sent on board the Valiant as soon as her captain was taken sick, to relieve him. He was a northerner by birth, in high health, and a perfectly temperate man. This case was well-marked yellow fever from its commencement; it yielded on the night of the fifth day, and he recovered. I would here remark that this man had been frequently to and from the city, transporting stone to the breakwater. On the next day a third case from the Valiant was brought to the hospital; this was a black man, with bilious congestive fever; and on that evening another black, with the same fever, from the lighter Fame, from Charleston. Immediately after the admission of the third case from the Valiant, I addressed a note to Captain Bowman, the commanding officer, expressing my suspicion that there were local causes about the Valiant calculated to generate disease, and suggesting to him the propriety of removing her crew (who were northern men) immediately, and to have the vessel ventilated and well cleansed. Captain Harvey, of Charleston, was placed in command of her, who stated to me that he found a large quantity of decomposed cabbages and potatoes, and other vegetable matter, in a putrid state, in her lockers, and a quantity of dirt and filth between her ceiling. Several of the negroes engaged in cleaning her were taken sick with intermittent and congestive fever. She was scuttled, and allowed to remain full of water for some days. After this she was pumped out, and continued perfectly healthy the rest of the season.

¹ In Dr. Strobel's book, the figures 1839, following the word June, are omitted, which Dr. Leiby assures me is a typographical error, and has authorized me to correct in the proper place. Several important errors of a similar kind occur in the book, which was badly got up. Without any authority from Dr. Leiby, and taking the passage as it is printed, it is evident that the June after the September of 1838 must necessarily have been the June of 1839.

The next case occurred at Fort Moultrie; an Irishman, who had not been in the city from May; this occurred the latter part of August. On the 18th of September, the assistant of Captain Bowman was taken with yellow fever. He had been in the city on the 10th and 17th. He recovered. On the 20th Captain Bowman was taken with bilious inflammatory fever. He had not been from Sullivan's Island during the season. Several other cases occurred among the families residing back of the fort; while they assumed a continued form of fever, and in many of their symptoms were nearly allied to yellow fever, it was very doubtful whether they could be properly placed under the head of genuine typhus icterodes. Many cases of inflammatory, or broken-bone fever, so called, occurred in October, among the workmen.

In reply to the latter part of your letter, "whether they (the lighters) did not communicate with vessels from Havana or Matanzas, lying at the quarantine ground?" I have taken considerable trouble to ascertain this fact, from every source from which a correct knowledge of facts could be derived, and I have no hesitation in asserting that, with one exception, there never was any communication with the quarantine vessels and the government lighters, or with any one in the service of the United States. The exception alluded to was the captain of the steamer *Sumpter*. On one occasion, he sent his boat on board the *John C. Calhoun*, some days after she had been boarded by Dr. Simons. No ill effects resulted from it, for neither of the negroes who were in the boat were sick afterwards. The steam-boat continued to perform her regular duties between the city and the fort throughout the season, and though her engineer and mate were entire strangers, there were no cases of fever on board.

I have thus endeavoured to place at your disposal a plain statement of facts, as they occurred, under my own observation, almost from the commencement of my professional career. I have most carefully excluded any opinion of my own upon the contagiousness of yellow fever, or that fever which has prevailed in the city of Charleston for the last two seasons; but, in conclusion, pardon me for entertaining the idea, and at the same time expressing it, that there were sufficient causes, both local and general, to have produced an epidemic of some kind. It was to be expected from the peculiarity of the season of 1838, as well as the extreme hot sun by day, in the latter part of May and beginning of June, and the cold north winds; that blew by night, at the commencement of the summer of 1839.

If these sheets can be of any service to you, please make what use you deem proper of them.

I remain, &c.

(Signed) ROBERT LEBBY.

DR. B. B. STROBEL, *Charleston.*

—*Strobel, on the Transmissibility of Yellow Fever*, pp. 190-196.

These are the records, much more than I expected to find, in relation to the epidemics of Sullivan's Island during the present century, or indeed since the first settlement; and from them two prominent points are established, that yellow and other fevers did originate, from 1817 to 1839, at Charleston, Castle Pinckney, Sullivan's Island, and on board lighters in the service of the United States in the harbour of Charleston; and that none of these fevers were contagious. That malignant fever originated on Sullivan's Island, in some of these years, there is no sort of doubt. We see that these malignant epidemics made their appearance somewhat periodically, and at intervals of much less than fifty years. We have no evidence that there has ever been a city, town, or village entirely free from a malignant epidemic fever of some kind for the space of fifty years, especially from the equator to lat. 35° N.

From 1839 to 1849, yellow fever did not appear in Charleston, but in the autumn of the last year the disease prevailed, and there were a number of fatal cases.

Abstract of the Quarterly Reports of Sick at Fort Moultrie, S. C., for the year 1849. Taken sick during the year.

DISEASES.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Totals.	Deaths.
Feb. intermit. quot. . . .	3	...	1	4	...
Feb. intermit. tert. . . .	4	1	1	2	8	...
Feb. remittens	4	1	5	...
Diarrhœa	6	1	...	1	4	7	4	1	...	2	2	...	28	...
Dysentery acut. . . .	3	1	2	...	1	3	10	...
All others	19	12	12	28	11	16	19	4	5	5	8	3	142	...
Grand total	35	15	13	29	15	24	27	5	10	10	10	4	197	...

MEAN STRENGTH FOR 1849.

Month.	Officers.	Enlisted men.	Total.
January	8	103	111
February	8	95	103
March	8	92	100
April	8	91	99
May	8	93	101
June	8	90	98
July	4	77	81
August	4	54	58
September	6	46	52
October	6	53	59
November	7	52	59
December	7	51	58
Aggregate	82	897	979
Average	6.83	74.75	81.58

The tables show the health of the troops at Fort Moultrie for the year, diseases strictly climatic being separated from all others. But there was much more disease than appears on the reports. On that for the quarter ending September 30, are the following remarks: "During September there was considerable sickness, much more than appears on the face of the report, as a large number of women and children, belonging to the post, and left behind by troops in the field, were present. Their diseases were principally intermittents and dysentery, the last severe in many instances, but none of the cases were fatal." In the report for the quarter ending December 31, are the following remarks: "In October, while yellow fever prevailed in Charleston, there was considerable sickness which does not appear on the face of the report, being among females and children, principally bowel affections. November was more healthy than October, and December still more healthy, though in all the quarter there has been more sickness than appears on the report." It is to be regretted that the diseases of women and children were not registered.

Abstract of the Meteorological Register at Fort Moultrie, S. C., for the year 1849.

MONTHS.	BAROMETER.		THERMOMETER ATTACHED: MEAN.				THERMOMETER DETACHED: MEAN.				Highest degree.		Range.	Dew-point: mean.	COURSE OF WIND AT SUNRISE.						Prevall- ing.	COURSE OF WIND AT 3 P. M.						Prevall- ing.	Rain: quantity.	Rainy days.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																														
	Max.	Min.	Range.	Sun- rise.	9 A.M.	3 P.M.	9 P.M.	Sun- rise.	9 A.M.	3 P.M.	9 P.M.	N.			N.E.	S.E.	S.W.	E.	W.	N.		N.E.	S.E.	S.W.	E.	W.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																		

Abstract of the Meteorological Register of Fort Moultrie, S. C., for the year 1850.

MONTHS.	BAROMETER.		THERMOMETER ATTACHED: MEAN.		THERMOMETER DETACHED: MEAN.					Highest degree.	Lowest degree.	Range.	Hottest day: Range.	Coldest day: mean.	Range.	COURSE OF WIND AT SUNRISE.								Prevail- ing.	COURSE OF WIND AT 3 P. M.								Prevail- ing.	Rainy days.	Rain: quantity.																																																																																																																																																																																																																																																																																																																																																																																																																																								
	Max.	Min.	Range.	Sun- rise.	0 A.M.	3 P.M.	9 P.M.	Sun- rise.	0 A.M.							3 P.M.	9 P.M.	Av'ge mean temp.	N.	N. E.	S. W.	S. E.	N. W.		S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.				N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.	S. W.	S. E.	N. W.

February 5. Coldest day of the year, as below:—

Sunrise. 9 A. M. 28

9 P. M. 33

Daily mean. 32

February 26. Severe thunderstorm last night; two houses struck by lightning. Rain, 1.40 inches.

February 28. Plum in full blossom.

March 13. Peach in blossom.

July 2. Thermometer at 93° at 2 P. M., and 93° at 5.30 P. M.

July 16. Hottest day of the year, as follows:—

Sunrise. 9 A. M. 85

3 P. M. 94

9 P. M. 84

Daily mean. 90

The year 1850 was sickly, as will more fully appear hereafter.

*Abstract of Quarterly Reports of Sick at Fort Moultrie, S. C., for 1850.
Taken sick during the year.*

DISEASES.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Totals.	Deaths.
Feb. contin.	1	1	...
Feb. quot. int.	1	1	...	2	...
Feb. tert. int.	1	1	...	3	...
Feb. remittens	11	25	2	...	1	39	...
Diarrhœa	1	...	1	1	2	6	11	...
Dysentery acut.	1	1	1	1	...	4	...
All others	8	1	8	4	4	9	9	5	5	...	7	10	70	2
Grand total	8	1	9	4	5	13	10	17	31	3	12	17	130	2

MEAN STRENGTH FOR 1850.

Month.	Officers.	Enlisted men.	Total.
January	7	52	59
February	7	52	59
March	7	48	55
April	7	45	52
May	6	42	48
June	5	41	46
July	5	39	44
August	5	39	44
September	4	40	44
October	4	40	44
November	8	88	96
December	12	180	192
Aggregate	77	706	783
Average	6.41	58.83	65.25

Deaths, 2; one in July, from convulsions caused by obscure disease of the brain; one in September, from apoplexy. Mortality per cent. for the year, 1.53.

These general reports and tables give but an imperfect idea of the fevers in August and September; and it will be necessary to notice the epidemic more particularly.

From the meteorological tables, it will be seen that the summer of 1849 was comparatively cool and wet, and that the summer of 1850 was very hot and dry; but no one can form an idea of the continued and intense heat during the summer of the latter unless he had been a resident, or has an opportunity to study the daily meteorological observations of that period. From the 26th day of June to the 8th of September, there were only six days in which the daily mean of the thermometer did not range from 80° to 90°; there was only one day in July when the daily mean of the thermometer was not 80° and above; there were only five days in August in which

the daily mean was not 80° and above; and there were twelve days in September in which the daily mean was 80° and above. In July, the thermometer ranged as follows: S. R. from 74 to 86; 9 A. M. 80 to 89; 3 P. M. 81 to 95; 9 P. M. 77 to 86. August: S. R. 73 to 84; 9 A. M. 79 to 90; 3 P. M. 78 to 94; 9 P. M. 77 to 86. September: S. R. 60 to 80; 9 A. M. 65 to 88; 3 P. M. 76 to 91; 9 P. M. 67 to 85.

Health of Charleston, previous to the Epidemic.—Yellow fever in the autumn of 1849; scarlatina, and other eruptive fevers, during the following winter, and in the spring of 1850.

Health of Sullivan's Island.—In the summer of 1849, bowel complaints and intermittents. No yellow fever in the fall of 1849, as in Charleston. In the following winter there were catarrhal affections and diarrhœa, particularly among children. Pertussis was common. In April, 1850, scarlatina broke out, supposed to have been introduced from Charleston, although direct contagion could not be traced in many of the cases. A number of severe cases, of the anginose and malignant varieties, occurred in the months of April, May, and June, several of which proved fatal. The chlorate of potass, and the chlorine mixture, as recommended by Watson, were tried, but without the least benefit. In some of the last cases inunction was tried, as I thought, with good effect. Olive oil was used. Oily or greasy applications to the surface appear to act by softening and preserving the cuticle, and perhaps the cutis vera itself, in a healthy state; the action of the skin is not checked as when the cuticle dries, cracks, and peels off; and, in consequence, dropsical effusion, the most dangerous of the sequelæ, is not so common, extensive, or fatal. In June and July, bowel affections, including cholera infantum, were common and severe among children. Prickly heat (*lichen tropicus*) was universal, from infancy to threescore and ten; boils were almost universal; and phlegmonous and carbuncular abscesses were extremely common.

The epidemic fever began in Charleston in July, if I mistake not, but its appearance on Sullivan's Island was later, and it spread rapidly and universally, scarcely a single person in a family escaping. The fever did not, for some reason or other, attack the garrison of Fort Moultrie this year until it had spread to almost every part of the island, and for a time there seemed to be a protecting line drawn around the public grounds; but when the fever did come, it came like an avalanche, and swept all before it. The first case of the epidemic fever in garrison occurred on the 20th of August, and the epidemic was nearly over by the end of September, there being only two cases in the month of October.

Symptoms.—A few of the prominent ones will be given. Cephalalgia was general in the early stage, often severe in the orbital and frontal regions; occasionally the eyes were suffused and a little injected—all going off as the disease progressed, or was under treatment; and pain of the back and limbs

was universal. Gastric irritability was frequent, and pain or tenderness of the epigastrium almost universal. The skin was sometimes hot and dry; oftener there was not much heat, but a tendency to clammy perspiration, or cold and profuse sweat. The patients with hot and dry skin were regarded in much the most favourable light, having less of the congestive character. Tongue of all appearances, clean, yellow, white, moist, or dry. In some of the most serious cases the tongue had red edges, becoming dark-brown and dry in the progress of the disease. Hemorrhage was common, generally uterine in the female, often causing abortion in pregnancy; in the male, epistaxis. As the tendency of this epidemic was to recovery, these varieties of hemorrhage mostly came on early in the disease, and were often critical, or the precursors of convalescence.

Treatment.—The disease being a fever with tendency to congestion, the treatment was similar to that adopted in the Florida fevers in 1839, '40, and '41. The following is from my report to the Surgeon-General.

“The cases of fever which came under my own observation both in garrison and beyond it, very much resembled those at Fort King, Florida, in 1841. These exhibited considerable tendency to congestion, like the Florida fevers. One of the worst cases of congestive fever I have ever seen originated on this island, in the person of a married female, of good habits and character, who had not been from the island for more than six weeks previous to the attack, and who had not stayed from the island at night for more than six months.”

“Sulphate of quinia was the main remedy. It was given, with calomel, as a cathartic, early in the disease, and this dose was generally followed by ol. ricini. In a few cases, where there was vigour of the circulation without gastric irritation, quinia was given with tart. ant. potassæ until the pulse was reduced. A combination of sulph. quin. and capsicum was an invaluable remedy. These two articles were often given at the very commencement, and almost always in the progress of all cases. There was tendency to congestion, often from the first, almost always in the progress; hence the value of this combination. In a case (a company laundress) of relapse, attended with rapid and feeble pulse, great thirst, pungent heat of skin, &c. quinia and capsicum were freely used, and in the course of eighteen hours the patient was considered out of danger. Unfortunately, in about twenty-four hours from the second attack of fever, abortion of a three months' fœtus (with considerable hemorrhage) occurred, reducing the patient to an extreme condition. The combination of quinia and capsicum was steadily continued, in addition aeth. sulph., brandy, &c., until the urgent symptoms passed off, and the patient recovered.”

The case of congestive fever alluded to terminated fatally, with all the symptoms of apoplexy. A few additional remarks will close the treatment. Venesection was scarcely thought of. Free cupping, often dry, to the cervical spine, lumbar region, and epigastrium, was general. Sinapisms were in universal use; and blisters were often employed in the severe cases, oftenest to the epigastrium, and frequently over the cervical spine. In all cases, from the beginning, quinia was the sheet-anchor, nearly always combined with the

infusion of capsicum. In a few grave cases, with brown and dry tongue, calomel in broken doses was combined with quinia, with excellent effect. Ptyalism was avoided, occurring only once, slightly, in a severe case with dry tongue, sordes, &c. So soon as slight tenderness of the gums came on, improvement was rapid. The various preparations of opium were in general use at night, morph. sulph. most frequently. Wine, brandy, and malt liquors, according to the taste of the patient, were pretty freely used.

MEDICAL STATISTICS OF FORT MOULTRIE, EPIDEMIC OF 1850.

	No. present.	No. of fever cases.	Deaths.	No. exempt from fevers.
Officers and men	48	38	0	10
Women, children, and servants	49	43	0	6
	<hr/> 97	<hr/> 81	<hr/> 0	<hr/> 16

We have 38 fever cases among 48 officers and men, or 79.16 per cent.; equal to 791 per 1,000. Women, children, and servants, 49 present, and 43 cases of fever; 87.75 per cent., or 877 sick per 1,000. In a total of 97 persons, there were 81 sick with fever, 83.50 per cent., or 835 per 1,000; and the number of persons who escaped fever was only 16, or 16.50 per cent. of the whole; equal to 165 per 1,000. This in less than two months, the first being taken sick August 20, the second August 23, and the last October 14. Indeed, deducting two cases on the 5th and 14th of October, we find that the whole of this sweeping epidemic took place from the 20th of August to the 30th of September, or in the space of forty-two days.

CASE 1.—Mrs. S——, residing in Middle Street, near the steamboat landing, or ferry wharf, was taken with congestive fever; but I did not see her until a day or two had elapsed, or on the 12th of August. Died on the 14th. This is the case alluded to in my report to the Surgeon-General. She had not been in town, or from the island, for more than six weeks, as she told me on my first visit, and had not slept from home for months.

CASE 2.—The infant of Mrs. S——, above mentioned, seven months old, was taken with malignant cholera infantum on the 18th of August, and died on the 27th. Had not been in Charleston, or from the island, during the summer. The late Dr. G. W. Ramsay saw both of these cases with me, and we had several conversations in relation to their origin, both of us being satisfied that they originated on Sullivan's Island.

CASE 3.—A child of Corporal Dounan, Company I, 2d Artillery, eight months old, was taken with cholera infantum in the month of June, and on the 10th of July it was considered moribund. The condition of the child at this date was as follows: emaciation extreme; pulse small, and very frequent; stomach had become quiet, and the bowels much more so than previously; skin clammy; no use of the limbs, apparently; thumbs drawn down into the palms; slept with one eye partially opened, as we often see in the cases of very sick children, but the other eye was wide open during sleep; ulceration of both cornea—in fact, the patient was given up. Regarding the state of the patient as hydrecephaloid, and consequently that the smallest depletion must prove fatal, the general treatment was continued: small and repeated doses of aqua ammoniæ, sulph. quin., brandy, wine-whey, minute doses tinct.

opii, &c. Tinct. ferri was used after small doses of acet. plumbi were withdrawn. The child lingered until cold weather came, and finally recovered, to the surprise of every one. Lieut.-Col. Erving will recollect this case. The child had not left the island during the spring and summer.

CASE 4.—Elizabeth Dounan, mother of case 3, was taken with the prevailing epidemic in September. This case, of relapse and abortion, has been already sufficiently alluded to. There was no arthritic disease in this case, neither was there any eruption, except prickly heat, which she had had during the whole summer. Had not been in Charleston for weeks.

CASE 5.—A child of private Bronsman, Company I, 2d Artillery, thirteen months old, was taken with cholera infantum in the month of July, and in August was given up as lost. As cold weather came on the child gradually recovered, but was troubled with diarrhoea during the whole of the subsequent winter. Did not leave the island during the summer; never slept out of garrison parade. This child, and Corporal Dounan's, would have been sent to Aiken, or to Buncombe, or to the White Mountains, had not the means been wanting.

CASES 6, 7, 8.—In a small, uncomfortable house, near the ferry wharf, three persons resided, a man, his wife, and another woman. The wife was taken with the epidemic fever on the 23d of August, the other female on the 24th, and the man on the 25th. All had the fever severely; all complained of intense pains in the frontal and orbital regions, and all had hemorrhage. The two females had profuse uterine hemorrhage, and the man had epistaxis. No boils, or eruptions, except prickly heat. No arthritic disease. All had severe pains in the lumbar region and limbs.

CASE 9.—Judy, a female black servant of my own, about 55 years of age, had the fever severely in the last part of August. Had intense pain in the frontal region and orbits, severe pains in the lumbar region, and epistaxis. Had this woman been only 25 or 30 years of age, there would probably have been uterine hemorrhage, but the critical time of life having passed, epistaxis occurred. She did not leave the island during the summer, and never left the house and yard (between the fort and hotel, on Middle Street) except on Sunday afternoon. Recovered.

CASE 10.—My other servant, a dark coloured boy verging to black, 17 or 18 years of age, had the fever still more severely in the early part of September. He had intense pain in the frontal and orbital regions, suffusion of the eyes, and profuse hemorrhage from the nares. He was severely sick for several days. Had not been in town for weeks. Recovered.

CASE 11.—Mr. G——, residing in Middle Street, about half-way between the fort and ferry wharf, was taken with this fever on the 4th of September. Had visited Charleston daily, but never stayed over night. Had intense pains in the frontal region and orbits, and back and limbs, and the disease speedily assumed the congestive character. At one time he was considered in a very dangerous condition. Recovered.

CASE 12.—Corporal David Dounan, Company I, 2d Artillery, was admitted into hospital with the prevailing fever on the 6th of September. When first taken he had severe pain in the frontal region, back and limbs, and tenderness of the epigastrium; in the progress of the case, the tongue had red edges, and its surface became black and dry; sordes; and the heat of skin was pungent to the touch. As the disease advanced, there was restlessness and muttering delirium at night, but there was no delirium during the day. Calomel and quin. were given regularly to this patient; sinapisms, cups, and then blisters, to the mastoids, cervix, and epigastrium were applied; and

camphor and sulph. morph. were administered at night. So soon as the teeth became slightly tender, improvement was rapid. Recovered. This was a very sick man, and for a few days the result was doubtful. Three or four similar cases occurred among the men in hospital.

CASE 13.—Ann C——, of Charleston, residing temporarily on Sullivan's Island, Irish, about 25 years of age, married, dissipated, was taken with the fever in August; had severe pain of the forehead and eyes, back and limbs; uterine hemorrhage, and she finally had a miscarriage. Recovered.

CASE 14.—Ann Boyle, Irish, wife of a private of Company I, 2d Artillery, about 35 or 40 years of age, had the fever in August; had intense pain in the frontal region, back, and limbs; and uterine hemorrhage. Recovered.

CASE 15.—Mary Boyle, daughter of the above, 10 or 11 years of age, had the fever in August; had severe pain in the frontal region, and epistaxis. Recovered.

CASES 16, 17, 18.—Isabella, coloured, residence on the back beach N. E. of the fort, was taken with the epidemic fever on the 20th of August. Had severe pain of the frontal and orbital regions; and the eyes were tender to a slight pressure, and suffused. She was very ill for three or four days. Her son, about 3 years of age, was taken with the fever on the 4th of September. Eyes suffused; epistaxis. Both recovered. She lost an infant with malignant cholera infantum in the last part of July, which child had not been from the island for a number of weeks.

CASES 19, 20, 21, 22.—Margaret Masterson and her family. Margaret was a widow, about 40 years of age, Irish, and a laundress of Company F, 2d Artillery, her husband having been a private in that company. She had the fever severely in the last part of August: intense pain in the supra-orbital region; suffusion of the eyes; and uterine hemorrhage. During convalescence she had an enormous abscess on one arm. Her three children had the fever at the same time, and altogether it was a helpless family, being for several days entirely supported from the hospital. One of these children had epistaxis; another was troubled with boils, both before and after the fever.

These brief notices of cases, made for the purpose of showing the nature and origin of the fever, are sufficient. Whole families were sick with the epidemic disease at the same time, and it was not uncommon to have difficulty in finding well persons sufficient to assist the sick. In one family, on Middle Street, I had four sick patients at the same time, two of them dangerously ill; and in another family in the same street, about one-quarter of a mile from the ferry wharf, there were five sick with fever at the same time, one of whom I considered dangerously ill. In the family of one of the officers, Rev. Mr. Harris's, Post Chaplain, residing at Bulow's house near the fort, there were six fever patients, every person in the house, nearly at the same time, and every one had the fever severely. Such instances were far from uncommon; in fact, they were very common, for the disease was wide spread, universal, over the whole island. The physicians of Sullivan's Island had a hard time of it that year, for although there were three or four of them, there was altogether too much for them to perform; and most of them sickened, sooner or later, during the epidemic, with the prevailing fever. Indeed, so

universal was the fever among the women and children attached to the garrison, that they received their nourishment—tea, arrow-root, chicken broth, beef tea, &c., for a time, entirely from the hospital. Happily, the fever was not of long duration as a general thing, nor was it fatal, but the suffering was great.

What was this fever? Dr. Dickson, Dr. Wragg, and many other of the Charleston physicians, and also several of the Augusta, Savannah, and other southern physicians, think it was the true *dengue*. Then, what is dengue? My limits forbid more than a reference to the descriptions and opinions of Surgeon-General Lawson, at Pensacola, 1828; Dr. Dickson, Charleston; Dr. Waring, Savannah; Dr. Dumaresq, New Orleans; the West India physicians; and many others, all writing about the same period of time, about 1828. From all the authorities, we are warranted in concluding, that dengue, throwing adventitious symptoms aside, is a febrile arthritic disease, coupled with exanthema.

1. *The arthritic affection*.—Dr. Lawson says that “the fever usually ran very high, and continued, without a remission, from twenty to thirty-six hours; after which it subsided, leaving the patient in a state of extreme debility, and labouring under an acute rheumatic affection of the muscular system generally.” Dr. Dickson says: “Of all the symptoms of dengue, the affection of the joints was the most tenacious and troublesome, adhering for weeks to some patients, and constituting a sort of permanent lameness, or loss of mobility.” Dr. Dumaresq observes: “This was a singular termination of the disease, leaving sufferers from the fever hardly able to move about; and, indeed, the appearance of persons in the street must have been truly pitiable to a healthy stranger—here, one seen dragging his legs after him, supported on crutches; and there, another, with limping gait and various contortions of countenance, bespeaking that his tardy progress was made at the expense of his bodily feelings.” Not a single instance of the disease termed *dengue*, as above described, came under my notice during the season, either among my own patients, or among the host of patients treated by other physicians.

2. *The eruption*.—It has been already mentioned that cutaneous complaints were universal before the epidemic fever commenced, as different varieties of lichen, boils, and phlegmonous and carbuncular abscesses. One person, covered with prickly heat, had more than twenty small boils from one elbow to the wrist, answering the description of the phlegmoid eruption of the dengue; but the person in question did not have a single symptom of the epidemic fever, although he had a large and troublesome abscess on one of the nates in the month of September. But from fever he was entirely free. Cases occurred in children, long before the epidemic fever broke out, in whom boils were almost universal, on the chest, neck, and scalp, particularly, there being at the same time a perfect sheet of prickly heat over the body. These eruptions, boils, abscesses, &c. were extremely common long before the

epidemic, and were regarded as the effect of intense solar heat. The entire absence of arthritic disease, and of an eruption coinciding with the fever, led me to conclude, both during the epidemic and afterwards, that the disease was not dengue.

I do not pretend to say that other physicians did not treat cases of dengue, abundance of cases; and it would be nonsense in me to dispute the testimony of members of the profession in Charleston, Savannah, Augusta, and many other places. I simply mean to say that, during the year 1850, I did not see a single case of dengue myself, and describe the fever as it ran its course among my own patients.

The epidemic disease was mostly southern bilious remittent fever, sometimes continued fever, with tendency to congestion. In this, I am convinced that my opinion was correct, as far as it went. But after reflecting upon the prominent symptoms of the fever, and especially after comparing them with the epidemic of 1852, it is my opinion that the disease made a near approach to yellow fever. The prominent symptoms, in the early stage and progress, were those of yellow fever, as given by me in *Fenner's Southern Medical Reports*, vol. ii. pp. 380-381, viz: "Cephalalgia was general in the early stage, often severe in the frontal and orbital regions—occasionally, the eyes were suffused and a little injected—all going off as the disease advanced or was under treatment. Hemorrhage was common; uterine in the female, often causing abortion in pregnancy; in the male, epistaxis." Dr. Fenner speaks of the *hemorrhagic tendency* in the fevers of New Orleans this year. Everything considered, it is my opinion, that only "one turn more of the screw" was wanting to develop well-marked yellow fever, or in other words, malignant yellow fever; and it may be, after the uncommonly hot and dry weather of the spring and summer, from the first of April to the last of July, that the one thing required to bring out the disease in a marked degree, was a deluge of rain in the month of August, or by the first of September. The quantity of rain which fell from the first of August to the 12th of September, 1852, was 13.55 inches; and if the same quantity had fallen in the same time in 1850, no one can estimate the consequences.

Abstract of the Meteorological Register of Fort Moultrie, S. C., for the year 1851.

MONTHS.	BAROMETER.		THERMOMETER ATTACHED: MEAN.						THERMOMETER DETACHED: MEAN.						COURSE OF WIND AT SUNRISE.				COURSE OF WIND AT 3 P. M.				Prevall- ing.	Rainy days.	Rain: quan- tity.															
	Max.	Min.	Range.	Sun- rise.		9 A. M.		3 P. M.		9 P. M.		Av'ge mean temp.	Highest degree.	Lowest degree.	Range.	N. E.		S. E.		W. by N.		Prevall- ing.				N. E.		S. E.		W. by N.										
				9 A. M.	3 P. M.	9 P. M.	3 P. M.	9 P. M.	9 P. M.	N.	N. E.					S. E.	S. W.	N.	N. E.	S. E.	S. W.																			
January	30.557	29.811	0.746	58.09	60.03	63.26	62.57	47.16	50.96	50.45	49.80	51.80	68	20	48	02.0	28.0	34.0	0	2	10	3	0	4	1	1	1	1	N. W.	7	4	9	4	1	3	1	2	N. W.	10	3.07
February	30.579	29.981	0.598	60.31	62.92	66.42	65.49	51.53	57.14	62.10	56.50	56.82	70	28	42	06.0	37.5	28.5	7	5	2	4	4	6	2	1	5	1	N.	1	7	2	3	2	6	4	3	N. E.	8	2.51
March	30.516	29.882	0.634	61.43	64.37	67.56	65.50	54.54	62.16	66.00	60.70	60.30	75	40	35	70.0	48.5	21.5	4	3	2	4	4	3	3	1	5	3	N. W.	2	1	2	0	11	1	5	3	N. E.	7	1.93
April	30.532	29.089	0.843	63.90	65.21	70.55	67.49	59.70	67.03	72.17	65.10	65.95	83	48	35	76.0	57.0	10.0	5	1	13	3	3	2	0	3	3	1	N. W.	2	8	0	4	2	4	6	1	N. E.	9	1.05
May	30.382	29.847	0.535	71.07	73.42	77.23	74.19	67.64	70.38	77.77	72.00	72.70	91	53	38	83.5	58.5	25.0	6	1	5	6	8	4	0	3	3	1	S. W.	2	7	0	8	10	2	1	1	S. E.	5	0.63
June	30.359	29.890	0.440	73.77	78.41	82.64	78.79	74.56	80.80	82.53	77.90	78.55	98	60	38	84.5	65.5	10.0	6	1	5	1	9	1	2	5	6	4	S.	3	6	0	12	6	3	0	4	S.	14	8.25
July	30.280	30.051	0.229	73.88	82.02	89.92	82.02	78.32	83.16	88.67	81.93	83.50	102	70	30	90.5	70.0	14.5	1	3	5	6	12	0	5	6	5	4	S. W.	0	2	0	17	4	3	3	2	S.	18	8.52
August	30.351	29.780	0.571	78.08	81.01	86.07	81.32	78.29	82.16	86.03	81.93	82.10	100	66	26	86.5	74.0	12.5	4	4	2	5	4	11	0	1	0	1	S. W.	3	4	2	10	9	2	0	1	S.	15	5.89
September	30.422	29.859	0.563	69.64	73.85	78.14	73.91	68.06	76.00	79.46	73.76	73.76	91	52	39	83.5	62.0	21.5	14	4	2	0	1	1	0	3	3	0	N.	7	0	2	7	3	2	4	0	N. E.	4	0.20
October	30.335	29.843	0.492	64.89	68.00	73.48	69.16	60.79	69.25	73.71	66.77	67.20	84	41	43	77.0	50.0	27.0	16	2	4	3	0	1	0	3	0	3	N. E.	3	8	2	7	3	1	4	3	N. E.	5	1.25
November	30.453	29.772	0.681	59.18	61.40	66.77	63.35	51.60	57.53	61.00	50.90	56.00	73	39	37	69.5	43.5	26.0	7	8	6	1	0	3	3	2	1	8	N. E.	3	4	2	6	3	0	8	4	N. E.	12	3.34
December	30.599	29.847	0.752	54.70	58.07	62.54	60.65	43.77	48.48	54.29	47.35	49.03	67	21	46	60.5	26.5	34.0	6	4	7	3	0	2	1	8	3	0	W.	4	0	4	2	4	6	7	W.	6	1.10	

Total quantity of rain, 30.06

November. No killing frost in the month.

December 17. Heavy snow during the day and evening. Ice half an inch thick. Thermometer varying from 25 to 32 during the day; 27° at 10 A. M.

DAILY REGISTER.

	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily mean.
17th,	29	29	29	25	29
18th,	21	23	32	29	26.5
19th,	27	27	43	34	35

December 18. Coldest day of the year. Thick ice.

December 19. Snow remains, and there is thick ice.

December 20. Snow partially covers the ground this morning. High wind last night.

Snow ice, thrown out of the bottom of a water-tub this morning, measured two full inches in thickness of solid ice. Snow had nearly all disappeared.

December 21. A little snow remains on north side of building this morning; disappeared during the day.

December 28. Heavy thunderstorm at 7 A. M. Force of wind 7, at 8 A. M. Rain, 0.45 inches.

January 31. Cold day. Thermometer as follows:—

	9 A. M.	3 P. M.	9 P. M.	Daily mean.
Sunrise.	20	24	30	28.

February 1. Thermometer as follows:—

	9 A. M.	3 P. M.	9 P. M.	Daily mean.
Sunrise.	28	35	46	37.5

February 20. Plum in blossom.

February 28. Peach in blossom.

April. High winds during the month.

June 4. Thermometer 98° at 11.30 A. M.

July 27. Thermometer 98° at 2.15 P. M.

Thermometer as follows:—

	9 A. M.	3 P. M.	9 P. M.	Daily mean.
Sunrise.	82	88	87	90.5

Thermometer 100° at 2.30 P. M.

July 29. Thermometer 98° at 12.15 P. M.

August 24. Severe storm, force of wind 8, doing damage on Sullivan's Island, and in other places.

October. No killing frost.

1851. This year was comparatively healthy. The number of companies in Charleston harbour was seven; four at Fort Moultrie, two at Fort Sumter, and one at Castle Pinckney, all under my medical charge. The hospital of Fort Moultrie was the general hospital for the sick in the whole harbour; but I have carefully separated the cases which strictly belong to the garrison of Fort Moultrie from all others.

Abstract. Taken sick during the year.

DISEASES.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Totals.	Deaths.
Feb. catarrhalis	1	1	...
Feb. int. quot.	1	1	1	3	...
Feb. int. tert.	1	4	6	3	2	2	18	...
Feb. remittens.	1	1	...	5	4	3	1	15	...
Feb. contin.	1	2	3	...
Diarrhoea	1	2	1	2	11	7	2	5	2	...	33	...
Dysentery acut.	1	1	2	1	3	6	1	2	7	1	...	25	...
Dysent. chron.	1	1	...
All others	19	14	18	7	8	18	46	46	24	16	17	17	250	4
Grand total	22	21	27	15	18	28	65	56	31	29	20	17	349	4

MEAN STRENGTH.

Months.	Officers.	Enlisted men.	Total.
January	15	183	198
February	16	180	196
March	14	173	187
April	14	169	183
May	13	164	177
June	12	189	201
July	11	199	210
August	10	188	198
September	11	175	186
October	11	199	210
November	12	211	223
December	12	198	210
Aggregate	151	2,228	2,379
Average	12.58	185.66	198.25

Deaths, 4; one in March, from fracture of the cranium and meningitis; one in May, of enteritis, chronic dysentery supervening; one in November, of phthisis pulmonalis; and one in December, a deserter, who had country fever in his absence; was admitted for general debility, and at last died of chronic dysentery. Number of cases during the year, 349. Mortality per cent., 1.14.

We have seen that 1850 was exceedingly unhealthy, and that 1851 was the reverse; it should be expected, therefore, that great meteorological differences would appear, but this is not the fact in relation to solar heat. The mean summer temperature for the two years is shown in the following table.

MONTHS.	1850.					1851.				
	Sun-rise.	9 A.M.	3 P.M.	9 P.M.	Daily mean.	Sun-rise.	9 A.M.	3 P.M.	9 P.M.	Daily mean.
May	66.38	73.61	76.83	70.22	71.61	67.64	76.38	77.77	72.00	72.70
June	71.00	79.66	81.46	75.23	76.23	74.56	80.80	82.53	77.90	78.55
July	79.03	85.70	88.19	82.12	83.61	78.32	83.32	88.67	81.03	83.50
August	79.00	85.00	87.29	81.93	83.14	78.29	83.16	86.03	81.09	82.16
September	73.10	78.90	82.76	77.50	77.93	68.06	76.00	79.46	73.76	73.76

It is to be distinctly understood, that in the meteorological tables for 1850, '51, '52, '53, the highest degree put down in summer, in all cases, is the highest degree at any time of the day, irrespective of the time of regular observation, although the mean temperature is the result of the regular observations. Thus, the highest degree in the table for 1851 is 100, though the highest degree at any regular observation was 99; and so of the rest. The lowest degree is the lowest of the regular observations at sunrise.

From the foregoing little table, it appears that the heat in both years was excessive; but there is not so much difference as might have been expected. The month of July was excessively hot in both years, most so, however, in 1850; and the following extract from the meteorological register of the hospital, showing the temperature for the last eight days of the month, may not prove unacceptable.

JULY.	1850.					1851.				
	Sun-rise.	9 A.M.	3 P.M.	9 P.M.	Daily mean.	Sun-rise.	9 A.M.	3 P.M.	9 P.M.	Daily mean.
24th	81	85	86	82	83.5	80	84	88	82	84.0
25th	80	86	86	81	83.0	81	84	88	80	84.5
26th	79	86	93	85	86.0	76	83	87	83	81.5
27th	80	89	81	81	80.5	79	84	94	84	86.5
28th	80	88	89	82	84.5	82	88	99	87	90.5
29th	77	88	89	84	83.0	82	88	96	78	89.0
30th	83	88	90	85	86.5	81	88	94	84	87.5
31st	82	89	88	85	85.0	78	85	92	78	85.0
Average mean temp.	80.25	87.37	87.75	83.12	84.0	79.87	85.50	92.25	82.00	86.06
Hottest day	86	85	94	84	90.0	82	88	99	87	90.5
Coldest day	74	82	83	79	78.5	70	81	82	77	76.0
Range	12	3	11	5	11.5	12	7	17	10	14.5

But if there was not a vast disparity of solar temperature, there was a great difference in the quantity of rain which fell in the different months and seasons:—

Months.	1850.	1851.	Months.	1850.	1851.
January	2.95	3.67	August	4.03	5.89
February	2.83	2.51	September	2.68	0.29
March	6.01	1.93	October	0.88	1.28
April	2.59	1.65	November	1.99	3.34
May	4.10	0.63	December	4.64	1.10
June	0.78	8.25			
July	2.32	8.52	Total quantity, inches	35.80	39.06

Had there been powerful rains in September of each year, such as fell in September, 1852, no one can tell the consequences.

Abstract of the Meteorological Register of Fort Moultrie, S. C., for the year 1852.

MONTHS.	BAROMETER.		THERMOMETER ATTACHED: MEAN.			THERMOMETER DETACHED: MEAN.			Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.		Range.	
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We come to the epidemic fever of 1852.

Abstract of the Quarterly Reports of Sick at Fort Moultrie. Taken Sick during the year.

DISEASES.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Totals.	Deaths.
Feb. contin.	2	2	...
Feb. icterodes	1	29	3	...	33	4
Feb. int. tert.	1	1	2	...
Feb. remittens	24	3	1	...	28	...
Diarrhoea	1	...	1	2	1	...	5	...
Dysentery acuta	1	1	1	2	...	5	...
All others	23	11	5	6	7	4	6	7	5	5	12	10	101	...
Grand total	23	11	6	8	7	5	9	10	30	38	19	10	176	4

Deaths, 4; three in October, and one in November, all of yellow fever.

MEAN STRENGTH.

Months.	Officers.	Enlisted men.	Total.
January	13	190	203
February	11	156	167
March	8	92	100
April	8	84	92
May	7	78	85
June	8	70	78
July	7	67	74
August	7	64	71
September	7	64	71
October	9	104	113
November	9	98	107
December	9	89	98
Aggregate	103	1,156	1,259
Average	8.58	96.33	104.92

Cases of Fever during the Epidemic—CASE 1.—Private Joseph Dunn, Company K, 2d Artillery, was admitted on the 22d August with bilious continued fever. This was not a severe case, and the patient soon recovered. He had not been in town, or from Sullivan's Island for several months, and there is no hesitation in saying that his disease originated on the island. Hard drinker.

CASE 2.—Col. M.—was taken with severe congestive tertian intermittent on the 29th of August, having been unwell three or four days previous. This case originated in fishing excursions to the west end of Long Island, which is near the east end of Sullivan's Island. The Colonel being exposed, on his return, to the damp evening air and malaria during the passage of his boat from the former island, through the creek or passage between the mainland and the back beach of Sullivan's Island to the fort-wharf. Col. M. stated to me that he had not been subject to any form of intermittent fever for many years. This was a severe and very obstinate case; during every paroxysm there was congestion of the brain, which was so great in the last one that his situation was considered dangerous. Recovered.

CASE 3.—Private George Hepp, Company E, 2d Artillery, was taken with bilious continued fever on the 31st of August. This was a mild case. Hepp was an old soldier, and had just re-enlisted (on the 20th of August), previous to which he states that he resided on a farm four or five miles from Charleston, where the fever was doubtless contracted. Attacked with remittent fever on the 17th September. Sober.

CASE 4.—Private John Roberts, Company K, 2d Artillery, drunkard, was taken sick with mild remittent; origin unknown.

Hitherto the fever was mild, scarcely worth attention, nor would it have been noticed but for what followed. The epidemic really broke out in a decided manner on the 7th and 8th days of September, the rain pouring down in torrents, and the parade being nearly under water. From 7 A. M. of the 7th to 2 P. M. of the 8th, four inches of rain fell. From 7 P. M. of the 4th to 2 P. M. of the 8th, no less than 9.40 inches of rain fell, or 1.88 inches per day; and this quantity added to 8.86 inches in June, 5.43 inches in July, and 4.15 inches in August, makes the enormous quantity of 27.84 inches in three months and eight days. More than an average quantity (4.17 inches) fell in the May previous.

We will first notice the cases in the soldiers' families residing on the parade, which extends from Middle Street (this street runs between the fort and parade) to the back beach; next, the cases which occurred in the fort; and lastly, cases entirely detached from the fort and parade.

CASE 5.—Dominic Duffy, 1st Sergeant of Company K, 2d Artillery, quarters on the east side of the parade, was taken sick with fever, registered remittent, on the evening of the 7th September. Sober; conduct unexceptionable. R. Ol. ricini ʒi . 8th. Removed to hospital early this morning; severe headache; skin hot and dry; pulse full; tongue coated yellow. Oil had operated. Cup the cervical spine. R. Quin. sul. grs. x ; hyd. prot. chlorid. grs. v . 9 A. M. R. Quin. sul. grs. v , at 1. 6 P. M. It is unnecessary to pursue the treatment further. Returned to duty on the 20th. Sergeant Duffy, wife, and Margaret Cooney, who lived with them, went to Charleston last on the 24th day of August, in the 2 o'clock P. M. steamboat, and returned in the 5 o'clock boat, thus being in town but little more than two hours. Having heard that yellow fever was in town, they passed directly through Market Street, from the steamboat wharf, to King Street, and when their purchases were made, all three returned through Market Street to the steamboat. King and Market Streets were the only streets entered. Never remained in Charleston over night. None of them went to town after the 24th of August; Ann Duffy, the wife, was taken with yellow fever on the 16th of October; Margaret Cooney remained in perfect health.

CASE 6.—Mary Stolper, wife of Sergeant Stolper, Company E, 2d Artillery, quarters on the east side of the parade, was ill on the evening of the 7th of September, and early on the morning of the 8th I saw her. The disease was registered as remittent fever; but there was intense pain in the frontal and orbital regions, and of the back and limbs, and tenderness of the eyes on slight pressure, with suffusion. This fever was easily broken up by calomel and ol. ricini, and the free use of quinia; but the intense supra-orbital pain and suffusion induced me to think, and to express the opinion that yellow fever might occur. The case served a good purpose, it put us on our guard. This

patient had not been in Charleston since the 17th of August, twenty-one days at least before the attack, when she visited her brother, who was sick with "some kind of fever," as she expressed it. Never slept in town. This case was in fact a very slight one, and merits notice only as occurring at the particular date, the 7th and 8th of September.

We now come to the Brassard family, quarters on the west side of the parade. Charles Brassard, one year old, was attacked late on the 5th of September with encephalitis, and died on the 6th, eighteen hours after the first visit. Had not been from the island.

CASE 7.—Eleanor Brassard, 7 years of age, was taken with severe fever, registered remittent, in the livery of yellow fever, on the 7th of September. Had not been from Sullivan's Island for twelve months. Had severe pain of the frontal region and orbits, eyes suffused, tenderness of the globes, and intense pain in the lumbar region and limbs. Had epistaxis on the 8th. Took calomel and quinia on the first day, and ol. ricin. and the quinia was continued, more or less, until convalescence was established. This case also served to point out that yellow fever was approaching. This patient was scarcely able to sit up when her mother was taken sick with yellow fever.

CASE 8.—Mary Ann Brassard, mother of Case 7, wife of private Brassard, Company E, 2d Artillery, was attacked with severe yellow fever on the 15th of September. She had intense pain in the frontal region and orbits, tenderness of the eyeballs on slight pressure, suffusion of the eyes, and the vessels of the conjunctiva were injected. She had also severe pain in the back and limbs, and pain in the epigastric region. The youngest child (Charles Brassard) was weaned in June, on account of the milk disagreeing, and the catamenia had appeared three times since, with great regularity "to the very day." Had profuse epistaxis on the night of the 16th. On the morning of the 17th, two weeks after the regular catamenial period, profuse uterine hemorrhage came on, continuing to the 23d, which was much worse at the commencement, on the 17th, 18th, and 19th, than at any time subsequently. Yellow skin on the 18th. This case was treated with calomel, quinia, and ol. ricin. with sinapisms and cups to the mastoids, cervical spine, and epigastrium on the first day, and afterwards, blisters to the cervix and epigastrium. Afterwards, the calomel in smaller doses was continued with the quinia, and morphia at night. Gums touched on the 18th. Was finally put upon ammonia and camphor mixt., wine, chicken tea, beef tea, &c. Recovered. This patient had not been from Sullivan's Island since the 15th of August—exactly one month before the attack of fever. Never had stayed in town over night; and she stated to me during her sickness, and afterwards, that she had not seen a single person sick with fever during the whole summer except in her own family, the daughter mentioned, Eleanor Brassard. And she was confident that her daughter had seen no case of fever whatever, and I certainly *know* that she never did. I have implicit confidence in her veracity. Eleanor Brassard could have seen no fever case, for her own is the first on the record in September, and she never left the parade to run about the island.

CASE 9.—Private Francis Brassard, Company E, 2d Artillery, hard drinker, husband and father of the last two patients, was taken with severe fever, registered remittent, of a yellow fever character, on the afternoon of the 24th of September. He had intense supra-orbital pain, suffusion of the eyes, and slightly injected conjunctivæ. Pain of back and limbs severe. Cal. x grs., and in three hours $\overline{3i}$ ol. ricin; x grs. quin. at tattoo, and same in the night.

This case was trusted almost entirely to quinia. Duty on the 17th October. He was in Charleston last, for an hour or two hours only, on the 16th of September, the day of the infant's death, or eighteen days before the attack of fever. Never slept in town. No member of this family saw a case of fever except their own sick. If Brassard had been taken with fever first, even eighteen days after having visited Charleston on the 6th of September, it would be said that he imported the fever from town, and gave it to his daughter and wife. Instead of this, he was the very last of the family to suffer.

CASE 10.—Private John Hazeltine, Company K, 2d Artillery, was hospital attendant during the whole summer, the hospital being on the west side of the parade. Moderate drinker. Was attacked with remittent fever on the 9th of September; duty on the 18th. He had not been in Charleston since the month of June.

CASE 11.—Sergeant William McNair, Company K, 2d Artillery, quarters on the west side of the parade, was taken on the sick report and admitted into hospital on the morning of the 11th of September, stating that he had been unwell since the 9th. There was no doubt of the nature of this case from the commencement. Was a hard drinker, and he stated to me that he had been drinking more than usual for a few days, on account of the severe duty and bad weather. On the 12th his condition was considered hazardous; very yellow; stomach very irritable, which was relieved by sinapisms and a large blister to the epigastrium, and creasote mixture. Cal. grs. x, morph. $\frac{1}{4}$ gr. at night. Quinia continued from the first, during the day and night. As he became convalescent, symptoms of nervous irritation began to show themselves in a decided manner, which ended in *mania a potu*. He finally recovered; duty on the 20th October. That this was a case of yellow fever is certain; for the intense pain of the frontal region and orbits, tenderness of the eyeballs, suffusion, and injected conjunctivæ on the first day (11th), added to the yellowness and increased injection of the vessels of the conjunctivæ on the 12th, leave no room for doubt. Indeed, there was almost every sign of yellow fever except the black vomit, and there was so much retching and vomiting on the 12th, that this was momentarily expected. This (on the 11th) was the first case of undoubted yellow fever on Sullivan's Island, the case of Mary Ann Brassard (on the 15th) being the second. The patient was last in Charleston on the 1st of July, more than two months previous to the attack, and he had not left Sullivan's Island, since that date; there is, therefore, no doubt of his fever having originated on the island.

CASE 12.—Margaret McNair, wife of the sergeant above named, was taken with yellow fever on the 30th of September. Severe pains of the frontal and orbital regions; eyeballs very sore and tender to the touch; suffusion; vessels of the conjunctiva injected; severe pain of the back and limbs; stomach quiet, though some uneasiness at the epigastrium. Has had three children, and is now six months *enceinte*. Sinapisms to the cervix and epigastrium; cups to the mastoids and cervical spine: Cal. quin. $\bar{a}\bar{a}$ grs. x; ol. ricin. $\bar{z}\bar{i}$, in two hours; and grs. x quin. in four hours from the first dose. Morph. gr. $\frac{1}{4}$, at night, and the quinia was continued in smaller doses. No more calomel was given than the first dose, as fears were entertained that a tendency to miscarriage might be produced by it if persisted in, but the quinia was given pretty liberally until the morning of the 3d October, or nearly three full days from the attack. October 3.—Last night, at 9 and 3 o'clock, took quin. grs. x, and morphia $\frac{1}{4}$ gr. 7 A. M. Patient free of fever; pulse soft and nearly natural; tongue moist; no pain of the head; eyes of nearly natural appearance; very

yellow. Directed mild treatment, an enema, small doses of quinia, iced mucilage, a little beef tea, &c. Unfortunately, about 9 o'clock, A. M., unmistakable labour pains began, slight but frequent convulsions came on, the patient became insensible, the head and forehead became hot and the skin dry, the *ensemble* rendering an unfavourable termination highly probable. In this state but little was attempted; iced mucilage, which she swallowed, was given in small quantities; sinapisms were applied behind the ears and over the cervical spine, and afterwards blisters; and cloths wet in iced water were kept constantly applied to the forehead and whole head. At 2 o'clock P. M., the os uteri was fully dilated, and the membranes ruptured, when the arm and shoulder immediately came down, but it was determined to do nothing just at this stage, for the following reasons: The patient was much exhausted; she had borne children; the pains were frequent and strong; pelvis ample; the soft parts were dilated, and easily dilatable; and the child was not of full size. For these reasons, the case was, for the present, committed to nature. The left shoulder presented—the right cephalo-iliac position of M. Chailly, head to the right, back in front. In a short time the body of the child was forced down by the strong pains, so that the hand nearly protruded from the vulva, when it was determined to render manual assistance; and in the event of this proving unsuccessful, to resort to instruments without delay, by reason of the exhaustion of the patient. In the absence of a pain, the head was carried up to the right of the pelvis as far as possible, bringing down the lower extremities; then assisting a strong pain with the fore and middle fingers, the body and lower extremities were delivered as far as the shoulders, and by the next pain, a very strong one, the head and placenta were delivered together. Fortunately, there was no hemorrhage. As the uteri became fully dilated, the convulsions ceased, the patient gradually recovered from the insensibility, and by the time that delivery was effected (about 4 P. M.) she was quite sensible, though greatly exhausted, the pulse being very feeble. Two or three large doses of brandy and water were given, but this beginning to disagree with the stomach, Port wine and water was substituted, and a few drops of æth. sulph. were frequently given. 10 P. M. More comfortable than could have been anticipated; pulse has risen; stomach quiet. Continued the wine and water through the night in such quantity and strength as to suit the patient; iced mucilage; a little chicken-tea; $\frac{1}{2}$ gr. morph. October 4.—Passed a comfortable night; pulse firm. All the blisters drawn and dressed; omit wine; take small doses of quin. and a little beef-tea. Oct. 5.—Reaction; pulse firm and frequent; hot skin; pain in the frontal region and eyes returned; irritability of the stomach, not excessive however. Cathartic enema; blister to epigastrium; grs. x quin. at 8, 12, and grs. v at 4 P. M.; iced mucilage; chicken tea in small quantities every two hours. Night.—Better. Pain of the frontal region and eyes abated, nearly gone, and in its place buzzing of the ears (quininization) substituted; pulse soft; stomach quiet; skin soft and cool; and in every respect the patient is better. From this time she was put on small doses of quinia, mist. ammon. and camph., beef and chicken tea, &c.; and the diet was gradually improved. Recovered. This patient had not been from Sullivan's Island for more than two months.

How came Margaret McNair to have yellow fever? In my opinion, by being exposed to the very same causes which gave the disease to Mary Ann Brassard, for they resided on the same side of the parade, and their rooms were not twenty feet apart. But the contagionist will account for this case

of fever very differently; that she contracted it from her husband. Then, how came Sergeant McNair to have yellow fever? Of whom did he contract the disease? The learned Brahmin taught his disciples, that the earth rested on the back of a huge elephant, and that the elephant stood on the back of a huge tortoise; and thus the whole matter was satisfactorily accounted for, the tortoise being left to support himself and burden in the best way he could. If yellow fever is contagious, why did not Margaret McNair's little daughter (six or seven years of age), who resided and slept in the same room with her mother during her whole sickness (these particular quarters having but one room), contract the disease? Children are not liable to the disease, say many physicians. Not liable to the disease! How can this be? Why are not children affected by the *materies morbi* of a malignant contagious fever, especially when exposed day and night, as this child was?

During Margaret McNair's illness and miscarriage, five persons were particularly exposed—Ann Duffy, Mary Stolper, Eliza Craig, Catharine Connor, and Margaret Cooney. Ann Duffy was present least of all, and did less service than any of them; she had yellow fever on the 16th of October, and it may be said that she took the disease here. This is not my own opinion; others, however, may think so. Mary Stolper (Case 6) assisted materially, and was present during a part of the labour, but being herself *enceinte*, I sent her away. After the delivery she returned, and rendered efficient assistance during the remainder of the afternoon, and the whole of the evening. Will it be said that the very slight case of fever reported was sufficient to protect M. Stolper from the contagion of malignant yellow fever? If so, it equals vaccination for protection from variola. Eliza Craig, C. Connor, and M. Cooney rendered more assistance than all others, previous to, during, and subsequent to the miscarriage on the 3d of October, and were consequently pre-eminently exposed to contagion; but not one of them had a symptom of yellow fever, or any other fever, during the whole year. Eliza Craig, wife of a drunken private, was much with this case, and also with Mary Ann Brassard and Deborah Doherty, whose case will be referred to; and Craig lived in the same block of quarters with Brassard and Doherty. These blocks are one-story wooden buildings, raised two or three feet from the ground, and each one of them contains three sets of quarters for married soldiers; and Brassard, Craig, and Doherty lived in one block, Craig in the centre, and the other two families having yellow fever. Not one of the Craig family, husband, wife, and two children, had any kind of fever. C. Connor and M. Cooney took care of the patient during the night of 3d October, and took good care of her. C. Connor was greatly exposed to yellow fever during the whole season. She lived in the same quarters with Brassard's wife during her severe illness, and assisted her, and afterwards was with M. McNair, as above stated. She had no fever. M. Cooney was with this case, as described. She lived with Ann Duffy, who had yellow fever, and took care of her during her sickness; nor was she ill a single day during the year. Ought

not these three persons to have contracted yellow fever, if it be contagious? It may be supposed that they had suffered from yellow fever previously. If this had been the case, it would not have been a sure protection, though somewhat of a probable one. But it so happens that not one of them ever had yellow fever in their lives.

CASE 13.—Sarah McClure, Irish, living with a family on the west side of the parade, was taken with remittent fever on the 11th of September. Recovered. This woman had not been from Sullivan's Island for more than a month.

CASE 14.—Deborah Doherty, wife of private John Doherty, Company K, 2d Artillery, quarters on the west side of the parade, primipara, was delivered of twin children on the evening of the 7th of September. On the 12th September she was attacked with remittent fever of a mild character. October 7, attacked with yellow fever, which proved not to be severe. Recovered. She had not been from Sullivan's Island for months, and Craig, whose family never had fever, lived between her and the Brassard family. Her state of health had been such, to my certain knowledge, as to prevent her from leaving her room for more than a month previous to her confinement, nor was she able to leave it until long after the 7th October.

CASE 15.—Private John Doherty, husband of the foregoing, temperate, and of good character, was taken with severe remittent fever on the 21st of September. He had not been from Sullivan's Island since the month of May.

CASES 16, 17.—Henry Dounan, and Mary his wife, parents of Deborah Doherty, landed from an emigrant ship, after a six weeks' voyage from Ireland, on the 18th of September, both being unwell, and on the 20th they were quite ill with a sort of remittent, which soon became continued, and of a typhous character. Resided in Doherty's quarters after landing. Having had a long voyage without any comforts, in a crowded ship, and arriving in a filthy condition, more apprehension was felt in regard to contagion from these two persons than from all the rest of the sick put together, during the whole epidemic. But contagion was not present in these cases, more than in those which originated on Sullivan's Island; for no other cases resembling them made their appearance. They were put on laxatives, moderate doses of quinia, and small doses of calomel; and as soon as their mouths were touched (the man's mouth was not very sore, the woman had severe ptyalism) they were put on small doses of quinia, wine, brandy, ale, and a nourishing diet. They recovered.

CASE 18.—Thomas Williams, Ordnance Sergeant, was taken with remittent fever on the 14th of September. A mild case. Quarters on the west of the parade. Was in Charleston last eight days before the attack. Never stayed in the city over night.

CASE 19.—Private Charles W. Reudiger, Company K, 2d Artillery, hospital cook (the hospital is on the west side of the parade), was taken with remittent fever on the 16th of September. He had not been to town since June, to my certain knowledge. A mild case. Moderate drinker.

CASE 20.—Mary Madden, Irish, arrived at Charleston from New Bedford, Mass., on the 18th September, and on the 22d she was taken with mild remittent at Sergeant Duffy's, on the east side of the parade. Stayed over night in Charleston on the 18th and 19th. A very mild case.

CASE 21.—Private William Connor, Company K, 2d Artillery, was taken with severe remittent fever on the 29th of September. The symptoms were

of the yellow fever type: intense supra-orbital pains, tender eyeballs, suffusion, slight injection of the conjunctival vessels, &c. First day, 8 A. M., cal., quin., aa 10 grs.; ol. ricin. ʒi at 11 A. M.; 10 grs. quin. at 1 and 6 P. M. At 10 and 4, night, 5 grs. quinia; $\frac{1}{4}$ gr. morphia at 10. The treatment afterwards was principally with laxatives, and quinia in smaller doses, until convalescence. Connor was married on the 15th of August, and remained in the fort until the 1st of September, when he stayed at night with his wife (Catharine Connor, before mentioned) on the west side of the parade, at the quarters of Brassard, as stated. Connor never left Sullivan's Island after his marriage on the 15th of August, until the 28th of September, the day before he was taken sick, and the day that Major Roland died of yellow fever at Castle Pinckney, when he was sent to Charleston for a short time, an hour or two only, on duty. Sober; conduct correct. In addition to her exposures to yellow fever already related, Catharine Connor visited her husband daily in the hospital during his illness, at a time when there was plenty of the disease, for Connor did not return to duty until the 17th October; and between the 4th and 12th October, inclusive, there were thirteen cases of real yellow fever admitted. Ought not this woman to have had yellow fever?

CASE 22.—George Whiteside, discharged sergeant of the 2d Artillery; drunkard; quarters on the west side of the parade in a building pertaining to the hospital—his wife being hospital matron—was taken sick with severe remittent fever on the 30th September. Calomel, ol. ricini, and quinia on the first day; mainly quinia on the second and third days. As the fever gave way, his mind wandered; symptoms of delirium tremens commenced on the 3d of October, and on the 4th the disease was established. Opium, morphia, camphor, brandy, &c. were given without effect. October 5, the patient was so violent as to require the strait-jacket for a short time this morning. Cal. 10 grs., opii $\frac{1}{2}$ gr. at 8 A. M.; chloroform $\frac{1}{2}$ drm. (internally), well diluted, at 10 A. M., and repeat at 12 M. No effect. At 2 P. M. gave a drm. of sulphuric ether, to take ten drops of it every half hour until 4 P. M., when another drm. is to be given. He took the first ten drops at 2 $\frac{1}{2}$ o'clock, fell asleep before 3 P. M., and did not wake until after dark. Convalescent from this time; small doses of ether during the night. October 6—ten drops ether hourly; nourishing diet; same at night every four hours. October 7—same; out of danger. This man was in Charleston on the 4th of September, and for the last time on the 15th. Never stayed in Charleston at night.

We have noticed every case of fever which occurred on the parade in the month of September, and will now proceed to the fort.

CASE 23.—Daniel Meck, Company K, 2d Artillery, drunkard, was taken with remittent fever on the 9th September. A very slight case; was not sick with fever again during the year.

CASE 24.—Sergeant Charles Seyfried, Company E, 2d Artillery, was taken with remittent fever on the 10th of September. Not a severe case; duty on the 15th; sober.

CASE 25.—Private John De Goy, Company E, 2d Artillery, a great drunkard, was taken with remittent fever on the 12th September. Not a severe case. De Goy was post-baker until taken sick, and slept in the bake-house, on the west of the parade, and he ought to have been put in the other class of cases.

CASE 26.—James Gallagher, artificer, Company K, 2d Artillery, great

drunkard, was taken with remittent fever on the 12th September. Not a severe case.

CASE 27.—Private George Hepp, sober, who had fever in August (see Case 3), was taken with remittent fever on the 17th September. A pretty severe case.

CASE 28.—Private John P. Emmerson, Company K, 2d Artillery, sober, was taken with remittent fever on the 17th September. This was a smart case of fever. He had not been to Charleston for more than a month.

CASE 29.—Private Vincent Bazelte, Company K, 2d Artillery, sober, was taken with remittent fever on the 21st September. Severe case. Was in town last in June.

CASE 30.—Private John Roberts, drunkard, sick before (see Case 4), was taken with remittent fever on the 25th September. Not a severe case.

CASE 31.—Musician Thomas Banks, Company E, 2d Artillery, was taken with remittent fever on the 25th September. This was not a severe case. Drinker; irregular in his habits.

CASE 32.—Private Edward Murricean, Company K, 2d Artillery, sober, and an excellent hospital attendant from the 9th September, was taken with severe remittent fever on the 26th September. Eight A. M., grs. x cal.; $\frac{1}{2}$ oz. ol. ricin. at 11 A. M.; grs. x quin. at 1—5 P. M., and same at 12 o'clock, night. Sept. 27.—Quin. grs. v at 9, 12, 4, 6; and the case was entrusted almost entirely to quinia from this date. This case put on the yellow fever type, for there was severe supra-orbital pain and suffused eyes. He had not been to Charleston since the month of July.

CASE 33.—Private George Tappey, Company E, 2d Artillery, was taken with plain remittent fever on the 26th September. Sober and correct. He was last in Charleston in the month of May.

CASE 34.—Private William Foster, Company E, 2d Artillery, was taken with remittent fever on the 27th September. Very mild case. Moderate drinker.

CASE 35.—Musician Wm. Stage, Company K, sober, was taken with remittent fever on the 29th of September. Quite a mild case. Though not a drunkard, of irregular habits.

CASE 36.—Brevet Major A——, stationed at Castle Pinckney, was attacked with remittent fever at Fort Moultrie, on the 28th of September. He came from the Castle several days previous to the attack. Not a severe case.

CASE 37.—Private Peter M. Pilgrim, Company K, 2d Artillery, a notorious drunkard, was taken into hospital on the 20th of September, on account of severe contusions which he received while in a state of beastly intoxication. He remained in the hospital until the 27th of September, when he was attacked with severe remittent fever. Had not been in Charleston since the middle of August.

CASE 38.—Patrick, Irish boy, 11 or 12 years of age, servant to the officer's mess in the fort, was taken with well-marked remittent fever in the first part of September, date not put down with the other circumstances of the case. He had been in the habit of going to Charleston to see his parents, and often stayed over night, in the upper part of the city, in the vicinity of ponds and marshes.

We will now briefly view those cases which were wholly unconnected with the fort and parade in the month of September.

CASES 39, 40, 41, 42.—Almost the entire family of Capt. Elzey, 2d Artillery, residing on Cove Street, entirely detached from the fort, was sick with

fever in the month of September. One lady, two white female servants, and one coloured female servant, had smart attacks of remittent fever, and not one of them had been from Charleston in a long time. As it will not be necessary to notice the fever in this family again, we will give three Cases, 43, 44, and 45, all occurring in the month of October. One lady was the subject of a trifling remittent; and one coloured servant and her child, who spent the day at the family residence and slept in a neighbouring house, had smart attacks of the same fever. The lady had not been in town for some time previous, and the two servants had not been from the island during the summer.

CASE 46.—Capt. Elzey was taken sick with severe fever on the 30th of September, after inspection, registered remittent, it being of that type, though it might have been termed yellow fever; for there was intense supra-orbital pain; the eyes had an unnatural appearance, were very tender on slight pressure, suffused, and the vessels of the conjunctivæ were injected. He was taken with chilliness, but did not have a confirmed chill, and there was a tendency to remission towards morning during the whole fever. In the progress of the disease there was a very dusky hue of the face, and the forehead was slightly yellow. Severe pain of the back and limbs. Had a severe attack of jaundice in July; has been quite healthy since. Cups to back of neck; grs. x quin.; ζ i ol. ricin. in two hours; grs. v quin. every four hours from the first dose. No mercurial was given in the whole course of the fever, as the smallest quantity would produce ptyalism, and it was held in reserve in the event of absolute necessity. It was found that five grs. of quinia was about the maximum dose, ten grs. producing ringing in the ears, severe pain in the occipital region, and other nervous irritation. Even the five grs. had often to be omitted at night. During the progress of the disease there was a good deal of restlessness, requiring morphia at night; and on the evening of the fifth day there was so much nervous irritation that the following was necessary: Sulph. morph. $\frac{1}{2}$ gr.; camph. 3 grs.; tart. ant. $\frac{1}{2}$ gr.; in pills at one dose. To this 2 or 3 grs. calomel would have been added had it not been for the idiosyncrasy; but the prescription procured sleep and a comfortable night, and the next morning the patient was better. From this time convalescence commenced. Capt. E. had not been to Charleston for three weeks; and he thinks the fever was brought on by exposure to wet while gunning on Sullivan's Island, in which opinion he is doubtless correct.

The immediate cause of fever in this family seems to be obvious. The rain fell in torrents in the first part of September, and the whole island was nearly covered with water; the kitchen, in the house, had a floor of boards placed directly on the ground, and for a few days the water was so deep on this floor, I was informed by a member of the family, as to render it almost impossible for the servants to prepare the regular meals; and the floors of the servants' lodging-rooms, adjoining the kitchen, were made of brick, the most improper of all materials in a humid atmosphere. No floor ought to be constructed of brick unless previously dipped in boiling pitch or asphaltum. No person ought, in truth, to have occupied the lower floor, and the whole of this house was rendered insalubrious by excessive dampness. Is it surprising that fever should occur in such circumstances? Would it not have been very surprising if the family had continued healthy?

CASE 47.—Mrs. C., wife of J. C., residing on Middle Street, between the fort and hotel, has had three children, miscarried (at about three months) in the first days of August, was taken sick with yellow fever on the 23d of September. Was “regular” just two weeks before. Profuse uterine hemorrhage occurred on the 29th, which lasted four days. This is unusual; states that the regular period lasts only a few hours; and that this is very unusual in quantity, appearance, duration, and in all the symptoms. At my first visit the patient had intense supra-orbital pain; eyes changed, unnatural in appearance, tender on slight pressure, suffused, and the conjunctival vessels were injected; intense pain of the back and limbs; and after a few days, yellowness of the face and neck. Took calomel, quinia, and ol. ricini on the first day, and the quinia was afterwards continued. An unfortunate accident complicated the case early in the disease: A solution of quinia was left at night, one-half to be taken at 12 o'clock, but a bottle of Eau de Cologne was unfortunately mistaken by the nurse for the quinia bottle. This harsh dose created much irritation of the stomach, but the offending cause was fortunately soon expelled; the supervening gastric symptoms, however, were severe, irritability, pain and tenderness of the epigastrium, dry tongue with red edges, &c. Sinapisms, cups, and blisters to the epigastrium; enemata and mild laxatives; moderate doses of quinia with small quantities of calomel and ice, and iced drinks were employed. The mouth became a little sore, and the patient became convalescent. She had been in the habit of visiting Charleston frequently, as often as two or three times a week, but had not stayed there all night since the June previous.

We have come to the end of September, and have noticed almost every case of fever pertaining to the garrison, every one of importance in the month. Let it be recollected that the night of September 30th is completed; that our garrison consisted of Companies E and K, 2d Artillery, during the whole quarter which is now ended; that few or no changes took place in the quarter; that no communication had taken place between the posts of Fort Moultrie and Castle Pinckney, for no communication between the sick and well of the two posts could take place; that none of our really sick patients had been exposed in Charleston, or at Castle Pinckney; and that the movements of all persons who were severely sick could be traced for days and weeks previous to the attack of fever. We will throw out every doubtful case, and we have remaining 27 cases of fever, 24 of continued and remittent, and 3 of yellow fever, which originated on Sullivan's Island. A strict contagionist may contend that Margaret McNair contracted yellow fever of her husband; we will throw this case out, though we believe no such thing, and we have 2 undoubted cases of yellow fever, and 24 cases of other fever, 26 cases in all, without the suspicion of importation or contagion. These cases originated on Sullivan's Island under such circumstances as leave no manner of doubt in relation to their origin. Not one of them had been exposed to any cause of fever except on Sullivan's Island.

It has been said that the garrison of Castle Pinckney introduced the contagion of yellow fever into the garrison of Fort Moultrie. We will examine the facts. Let us see whether the garrison of Castle Pinckney had the con-

tagion to introduce so early as the 11th and 15th of September. The garrison of Castle Pinckney was not withdrawn to Fort Moultrie until the first day of October, and up to this date there was little communication between the two posts, for a man on pass had no inducement to visit the dull and dreary post of Castle Pinckney. In truth, there had been little sickness at Castle Pinckney when that post was abandoned, for Major Roland's death did not take place until the 28th of September, after an illness of three or four days only, of yellow fever contracted in Charleston; and Capt. Allen was taken sick after the funeral of Major R. (which was on the 29th), and left behind when the garrison withdrew to Fort Moultrie. The cases of these two officers were the only cases of yellow fever up to the 1st of October; whereas, at Fort Moultrie there were two cases of yellow fever and abundance of southern bilious fever before Major Rowland was attacked, and before there was a single case at Castle Pinckney; and before the garrison of that post came over, there had been at Fort Moultrie 3 undoubted cases of yellow fever, and 39 cases of bilious fever—42 cases in all, in the month of September—tenfold more sickness than at Castle Pinckney. Not a single sick person, man, woman, or child, came from the Castle on the 1st of October, and only one sick person (Capt. Allen) was left behind. Up to the removal, the garrison of Castle Pinckney was decidedly more healthy than that of Fort Moultrie.

It has been said that the yellow fever was brought from Charleston. This is not the fact. Only a certain class of men apply frequently for passes to town; the married, sober, and steady men rarely ask for passes, nor do the drunkards get them, or often apply for them, for there are *groggeries* enough on Sullivan's Island. Further, all classes were aware of the great sickness in town, and night passes were refused. So early as July 24th, the following communication was made to the officer in command:—

“The heat of the weather, the filthy condition of the streets of Charleston, and the reports in relation to present disease there, render it advisable, in my opinion, for us all to be on the safe side of the question; and I would therefore request the Colonel commanding to consider the expediency of refusing permission to all persons composing the garrison of this post to pass a single night in the city.”

Not one of the Fort Moultrie cases in September had been to Charleston (see Cases 8, 11, 12) for weeks, and not one of them saw a case of yellow fever except in their own families. In all September, there had been four cases of yellow fever unconnected with the fort. Mr. Kennedy, who died at the Moultrie House on the 23d of September, after an illness of five days; the bookkeeper of that hotel, who was at the crisis of the disease on the day of Kennedy's death; Mr. Ferguson, who died at the steamboat landing near the date of Kennedy's death; and Mrs. C. (Case 47.) These particulars can be relied on. These four patients could not possibly have introduced the yellow fever into our garrison, for the best of all reasons—we had it first; two of the cases (8, 11) being the very first cases on the island, and the third

(No. 12) could not possibly have been exposed to any one with the disease except at the fort. The conclusion is irresistible, that yellow fever was not introduced from Castle Pinckney, nor from Charleston, but that it originated on Sullivan's Island.

October 1. The garrison of Castle Pinckney was removed to Fort Moultrie, and from this date our garrison consisted of Companies E, K, M, all of the 2d Artillery. It is a curious fact in the history of the soldier, that no movement of troops can be made without a "big drunk," in spite of the officers; and never was a custom more "honored in the observance" than by Company M; for during the first four or five days after the removal there was scarcely a sober man in the company; and the disease was as contagious as Sir William Pym represents yellow fever to be, for almost every man in Companies E and K had an attack, contracted of Company M. This was the only contagious disease introduced from Castle Pinckney during the year.

October 2. One case of remittent fever from Company E; 4th, two cases of yellow fever from M; 5th, one case of remittent fever from E; one case of yellow fever from M; 7th, one case of yellow fever from K; 8th, one case of yellow fever from K; 9th, one case of yellow fever from K, and one from M; 10th, one case of yellow fever from M; 11th, one case of yellow fever from E, two from K, and one from M; 12th, one case of yellow, and one of remittent from M; 13th, one case of yellow fever from M; 14th, one case of yellow fever from K, two from M; 16th, one case of yellow fever from M; 17th, one case of yellow fever from E, one from M; 19th, one yellow fever from K, two from M; 20th, one yellow fever from M; 23d, one yellow fever from M; 25th, one case of yellow from K; 26th, one from K; 28th, one from K; 31st, one case of yellow from Ordnance Corps.

November 4. Two cases of yellow fever from M; 10th, one case of yellow fever from K; 16th, one case in the field and staff, remittent, the end of the epidemic, there having been severe frost on the morning of the 15th.

Some of the most interesting after the 1st of October will be given, divided into the same classes as in September—those on the parade, those in the fort, and those entirely detached from the fort.

CASE 48.—Bridget Whiteside, wife of Case 22, hospital matron, quarters on the west side of the parade, was delivered of a healthy child on the 27th of September, and seven days after, on the 4th of October, she had an attack of remittent fever, from which she recovered without difficulty. This patient had not been in Charleston for months, and I am certain that she had not been able to leave her room for many days previous to her confinement.

CASE 49.—Deborah Doherty, taken with yellow fever Oct. 7. See Case 14.

CASE 50.—Alexander McDonald, native of Scotland, clerk, on the east side of the parade, was taken with remittent fever on the 15th of October. This was a light case. He had not been to Charleston for many weeks; being a comparative stranger, I advised him to cautiously avoid town during the epidemic, and on the island he saw no sick person. In the month of June previous, he had a severe attack of bilious colic. He left Glasgow in a packet ship on the 1st September, 1851, and arrived in New York on the

21st of the same month. Arrived in Charleston in the last part of October, stayed there about eight days, and came to Sullivan's Island on the 6th of November. He did not leave the island from that date, and was taken sick with bilious continued fever, of a typhoid type, in the last part of December. This was a severe case, and at one time a fatal termination was feared; pulse frequent, but sometimes full, slow, and occasionally losing a beat; skin hot and dry, the heat sometimes amounting to *calor mordicans*; tenderness of the epigastrium and meteorismus; very brown and dry tongue; heavy sleep, sometimes approaching to lethargy, &c. This case had to be treated more like the typhoid fever of the north than most southern fevers. Quinia was of no use, though tried at the beginning of the disease, and after a small quantity of calomel at the commencement, and cups to the cervical spine and epigastrium; small and frequent doses ammon. acet., with a little spts. eth. nitros.; minute doses of ipecac, mild laxatives and enemata, epispastics, mild nourishment, &c. were relied upon. He was not able to resume business before the month of February; and he has never recovered the ruddy complexion with which he arrived from Scotland, but presents the sallow face of a southern resident. He is temperate, and exemplary in his conduct in every respect; and he has since stated to me that he cannot account for this fever; that he saw no sick person; that he did not go out at night or in the evening, and that he never lived more prudently in his life. The year 1851 was healthy, there being but few cases of climatic disease; but in the autumn and first part of winter, fevers and dysentery assumed a typhoid character, the same type of disease prevailing at the time in Charleston, as I was informed by some of the physicians. The fever of 1851, and subsequent bilious colic, probably saved our patient from the yellow fever of 1852.

CASE 51.—Ann Duffy, wife of Sergeant Duffy (Case 5), on the east of the parade, was taken with yellow fever on the 16th of October. Has no children, and the catamenial discharge has always been scanty. Had ended a regular period two days before. Was attacked with fever about 3 o'clock A. M., and I saw her at 7 A. M. She had intense pain in the frontal and orbital regions; eyes tender, suffused, and the vessels injected; severe pain in the back and limbs; dusky appearance of the skin (is a brunette); stomach quiet. Quin., cal., $\bar{a}\bar{a}$ 10 grs.; 1 oz. ol. ricin. at 10 A. M.; quin. 10 grs., cal. 5 grs. at 12 M.; at 5 P. M. 10 grs. quin. and 3 grs. cal.; and 3 grs. cal., 10 grs. quin., and $\frac{1}{4}$ gr. morphia at 10 P. M. Take 10 grs. quin. at 4 o'clock to-morrow morning. 17th. Pain of head and eyes relieved, and appears much better; no more calomel was given; five-grain doses of quinia every four hours. 18th. Face and neck yellow; profuse uterine hemorrhage, which continued until the 30th, though in less quantity than at first. Slight hemorrhage from the mouth and throat; no ptyalism; recovered. Was last in town on the 24th of August. See Case 5.

Margaret Cooney, mentioned before, nursed this patient, and it appears to me that she ought to have contracted the disease, having been repeatedly exposed to the contagion. Mary Madden, Case 20, was with Ann Duffy during the whole fever, and slept in the same room with her; did her slight remittent fever preserve her from the contagion of yellow fever?

CASE 52.—James Quinn, first Sergeant of Company E, sober, conduct unexceptionable, quarters on the east of the parade, was taken with yellow fever on the 17th of October. This case was at first treated with quinia and a small quantity of calomel, sinapisms, and cups to the epigastrium, laxa-

tives, &c., and the fever was considered as broken up; but a reaction or secondary fever came on on the 24th, and it was found necessary to adopt a more decisive course; the patient was therefore put upon quinia and calomel, epispastics to epigastrium and over the cervical spine; brandy was given according to the taste; beef tea, chicken broth, &c. The fever did not give way until the mouth became sore, which could not be effected until the 29th. He was then put upon small doses of quinia, anodynes at night, the brandy was continued, nourishing diet given, and he went to duty on the 23d of November. He had not been from Sullivan's Island since the 26th of August.

In this family there were five persons besides the sergeant—Sergeant Quinn's wife, the oldest boy, aged sixteen, and three other children of various ages. The mother had yellow fever in Charleston a few years before, which may perhaps be considered sufficient protection to her, and children are not so susceptible to yellow fever as adults; but the eldest son can scarcely be considered as under the protection of childhood.

CASE 53.—James Winnie, eight years of age, son of artificer Winnie, Company M, quarters on the west side of the parade, came from Castle Pinckney with the garrison on the 1st of October, and on the 18th of the same month he was taken with remittent fever. The case was not severe. While Company M, from Castle Pinckney, had already suffered severely from yellow fever, brought on by drunkenness and other causes, it is a curious fact that this was the very first fever, of any kind, which had attacked any woman, child, or servant from that post, the original garrison of Fort Moultrie, men, women, children, and servants, having suffered severely from febrile diseases during the whole of September, and to this very day. Not one of the non-military from Castle Pinckney suffered from an attack of fever until the 18th of October, eighteen days after removing to Fort Moultrie. This patient did not leave Sullivan's Island after the 1st of October.

CASE 54.—John Riley, about eighteen months old, son of Sergeant Riley, M, quarters on the east of the parade, was taken with remittent fever on the 19th of October. This was the second case belonging to the non-military from Castle Pinckney. Had not been from Sullivan's Island since the 1st of October, the day he came from the Castle. This child had a lighter attack of the same fever on the 2d of November.

CASE 55.—Bridget Tighe, about twenty-five years of age, living in Sergeant Riley's family, was taken with yellow fever, twenty-two days after her arrival from Castle Pinckney. Had not been from Sullivan's Island since the 1st of October. She was put on calomel and quinia until the gums were made somewhat sore, when she recovered without difficulty. Sergeant Riley had but one room for quarters during the epidemic; the family cooked there, ate there, slept there, and Bridget went through yellow fever there; yet both the Sergeant and his wife escaped every kind of disease. How could this be, if yellow fever is contagious? for every member of the family was receiving the *materies morbi* into the stomach and lungs, both day and night, during the progress of Bridget's case. That the woman had yellow fever there is no doubt, for the intense supra-orbital pain, suffusion, injected conjunctival vessels, and yellow face and neck, all seem sufficient to establish that fact.

CASE 56.—Mary Ann Winnie, mother of Case 53, wife of artificer Winnie, was taken with yellow fever on the 26th of October, twenty-six days after

the removal from Castle Pinckney. Had not been from Sullivan's Island since her arrival on the 1st. There was intense pain of the frontal and orbital regions; the eyes were painful, tender, suffused, and the conjunctival vessels were highly injected; tenderness of the epigastrium. She was immediately put upon calomel and quinia; cups to the cervical spine and epigastrium, and subsequently blisters, &c. Ptyalism was produced as soon as possible, this being the intention from the beginning on account of the severity of the attack. She finally recovered under the use of small doses of quinia, brandy, wine whey, mist. ammon. carb., anodynes, nourishing diet, &c. Winnie did not contract the disease, for he had no fever during the year.

CASE 57.—William Winnie, infant son of the above, was taken with remittent fever on the 2d of November, a month after arriving here from Castle Pinckney. Had not been from Sullivan's Island since the 1st October.

We have thus given a brief history of every interesting case on the parade, including every person among women and children from the castle who had fever; and we find that 5 cases, 3 of remittent and 2 of yellow, occurred among 9 persons who came from that post, not a single case occurring among them until 18 days after their arrival. We will give a few interesting cases from the fort.

CASE 58.—Charles Platt, 1st sergeant of company M, sober as regards spirituous liquors, but an habitual opium-taker, was admitted into the hospital on the morning of the 5th of October, at 8 A. M. Had not reported before, and had entirely passed the first stage of the disease; very yellow; eyes yellow and injected; skin clammy; pulse slow and feeble, &c. Black vomit came on at 12 M. on the 6th, and he died early on the morning of the 8th. After announcing his death to the commanding officer, the communication goes on as follows:—

“Sergeant Platt, it is said, had been ill two or three days before reporting to the surgeon of the post, but there is certainly no doubt of his having been hard sick more than 24 hours before reporting. In yellow fever, more than in any other disease, three, four, five, or six hours at the outset are of immense consequence; and no man with yellow fever can expect to recover after 24 hours' delay. The undersigned therefore respectfully requests all company officers to endeavour, by every means in their power, to impress this important fact on the mind of every person connected with the command. Let every person pertaining to garrison, man, woman, and child, be reported without delay to the surgeon as soon as taken ill.”

This communication was published to the command on parade, with excellent effect.

CASE 59.—Private Fred. Schmidt, M, was taken sick with yellow fever on the 10th of October. He was a drunkard, a prisoner, and was admitted from the guard-house at 6 P. M. This case was managed in the usual manner, by sinapisms, cups, calomel, quinia, &c., and the mouth became sore on the 14th; and from this date convalescence seemed to be established, the patient taking little medicine except a bitter infusion, the diet being nourishing, as the appetite was rather strong. Indeed, the man was considered out of all danger. On the morning of the 20th, there were strong yellow fever signs, black vomit finally commenced on the 25th, and the patient lingered, in an offensive condition, and more dead than alive, until the 2d of November. There was mystery in this relapse, which at first could not be understood; but the facts

finally came out. It is the custom among soldiers to "cheat the doctor," if possible; and the patient had managed, by means of his *confrères*, to smuggle some liquor and a free diet into the hospital, thus "cheating the doctor," and launching himself into eternity.

CASE 60.—Private Thomas Patterson, K, drunkard, was taken with yellow fever on the 11th of October. At 8 A. M., cal. 10 grs., opii $\frac{1}{2}$ gr.; 15 grs. quin. at 8, 12, and 4; ol. ricin. $\frac{1}{2}$ oz. at 12. Quin., in smaller doses, at night, and on the 12th, 13th, and 14th. Appeared better on the 12th, decidedly better on the 13th, was considered convalescent on the 14th, and decidedly so on the 15th. Took no medicine on the 15th and 16th, except some bitter infusion, and his diet consisted of beef-tea in the day and arrowroot at night. Took no medicine on the 17th and 18th.

18th—*night*. On making an evening visit to the hospital, found the patient had relapsed, or secondary fever had come on; there was intense pain in the frontal and orbital regions; eyes suffused, painful, tender, and the vessels were injected. Sinapisms and mustard pediluvia. Quin. 10 grs., cal. 3 grs., at 9, 1, and 5 A. M. to-morrow morning. 19th. Quin. 5 grs., cal. 1 gr., at 9, 12, 3, and 6; ol. ricin. 1 oz. *Night*. Quin. 5 grs., cal. 1 gr., at 9, 1, and 5; $\frac{1}{4}$ gr. morphia at 9. 20th. Symptoms continue much as at first; very yellow. Quin. 10 grs., cal. 1 gr., at 9, 1, and 6. *Night*. Quin. 5 grs., at 9, 1, and 5; cal. 3 grs., morphia $\frac{1}{4}$ gr., at 9. 21st. Ol. ricin. $\frac{1}{2}$ oz., quin. 5 grs., at 9, 12, 3, and 6. *Night*. Quin. 10 grs., cal. 2 grs., at 9, 1, and 5. 22d. Quin. 10 grs., cal. 1 gr., at 9, 1, and 5. *Night*. Quin., cal., $\bar{a}\bar{a}$ 5 grs., at 9; 10 grs. quin. at 1 and 5. 23d. Quin. 5 grs., at 9, 12, 3, and 6; and 5 grs. cal. at 6. *Night*. Quin. 5 grs., cal. 2 grs., at 9, 12, 3, and 6. Wine and water, or ale. 24th. Quin. 5 grs., cal. 2 grs., at 9, 12, 3, and 6. Brandy or wine, and beef-tea. *Night*. Quin. 3 grs., at 9, 1, and 5, and wine or brandy, as he prefers. 25th. Hitherto, no effect of the mercurial on the gums could be perceived, but this morning they were tender, with fetor of the breath. Quin., as yesterday, wine, chicken-tea. *Night*. Quin. 5 grs., at 9, 1, and 5; cal. 2 grs., at 9, to insure the effect. Brandy or wine. 26th. The patient was so ill that it was determined to continue the mercurial to the full extent of decided ptyalism. Quin. 5 grs., cal. 1 gr., at 9, 12, 3, and 6; $\frac{1}{4}$ gr. opium at 9. Brandy or wine, and a little beef-tea. *Night*. Wine; opium 5 grs., cal. 1 gr., at 9, 1, and 5, and $\frac{1}{2}$ gr. opium at 9. 27th. Mouth quite sore, and mercurials were omitted from this date. Gargles, wine and brandy, beef-tea, and 3 grs. quin. every third hour. With occasional variations, this plan was continued until the patient became thoroughly convalescent. Duty on the 27th of November. This was an exceedingly obstinate case, and perhaps the mercurial ought to have been pushed further in the first instance.

CASE 61.—Private Robert Ripley, E, a great drunkard, belonging to the guard of Fort Sumter, was admitted into hospital from that post on the 11th of October. He had been sick more than 24 hours, and would have been brought over on the 10th, but the high wind of the night before had damaged the boats so that it was impossible to get him into the hospital sooner. Eyes suffused and injected; face of a dusky yellow, his complexion being very swarthy; skin clammy. Mustard bath,¹ sinapisms and cups to the epigastrium and thorax, quin. 10 grs., cal. 5 grs., at 9, 1, and 5. After this date he was

¹ Baths and pediluvia have not been mentioned heretofore, because it is a standing hospital regulation that every patient shall be washed on admission, have his feet bathed, his clothes changed, and his hair combed and brushed—to be cut, if necessary. During the epidemic, the water for washing was always warmed, and the pediluvia contained mustard.

put on moderate doses of quinia, brandy, chicken and beef-tea, &c. Duty on the 8th of November. This was the only case of fever at Fort Sumter during the season, and the occurrence of this was at first alarming, for we had fears that the yellow fever was breaking out there; but, on the other hand, the patient was known to be very irregular in his habits, and to have spent much time on Sullivan's Island; and finally, at the close of the epidemic, this proved to be the only case of fever which occurred there. In truth, the only healthy spot in the vicinity of Charleston, free from all fever during the year, was Fort Sumter. There were at this post during the epidemic, one ordnance sergeant, six privates, two women, and four children, thirteen persons in all, and not one of them had fever except Ripley. This man had yellow fever, and was nursed at Fort Sumter more than 24 hours; some one must have been exposed in that time, for not one of these persons had ever been through the disease, yet no one contracted the fever. Further, I have reason to know, and do know, that the sergeant and six men were exposed, and not one of these seven persons received the contagion of yellow fever.

CASE 62.—Private Patrick Fitzpatrick, M, a great drunkard, was admitted into hospital on the 11th of October, at 3 o'clock P.M., with yellow fever. Cal. 5 grs., quin. 10 grs., at 3 and 7. *Night.* Same at 12, and 6 to-morrow morning. Large sinapisms and cups to epigastrium, blister to back of neck. 12th. Quin. 5 grs., at 9, 12, 3, and 6; same at night, at 9, 1, and 5; cal. 5 grs., sul. morph. $\frac{1}{4}$ gr., at 9. 13th. Quin. 5 grs., cal. 3 grs., at 9, 12, 3, and 6; sinapism, cups, and blister to epigastrium; enema. *Night.* Quin. 5 grs., at 9, 1, and 5; $\frac{1}{4}$ gr. morph. at 9. 14th. Quin. 5 grs., at 9, 12, 3, and 6; chicken-tea; same at night. 15th. Continue quin.; large blister again to epigastrium; ol. ricin. 1 oz. Quin. every 4 hours at night. 16th. Much better, and appears convalescent. Continue the quinia. 17th. Appears decidedly convalescent. Quin. 3 grs., with bitter infusion, every 4 hours. 18th. Still better. Take same as yesterday, and 1 oz. ol. ricin, as the bowels had not moved for 24 hours. In the afternoon, I found him much worse, and with decided symptoms of yellow fever. Oil had not operated; enema. Quin., cal., āā 10 grs., at 5 and 9, and 10 grs. quin. at 1 and 5, night. 19th. Cal., quin., āā 10 grs., at 8 and 12; ol. ricin. 1 oz., at 2 P.M., as the bowels are somewhat confined; 10 grs. quin. at 6 P.M. *Night.* Cal., quin., āā 5 grs., morphia $\frac{1}{4}$ gr., at 9; cal. 3 grs., quin. 5 grs., at 1 and 5. 20th. Worse; black vomit came on about the middle of the day. Died on the 23d.

This case was considered in a fair way of recovery on the 16th, 17th, and morning of the 18th; and, though there was no actual proof, it was strongly suspected and believed that food and liquor had been smuggled to him. Be this as it may, however, I have always regretted that the calomel had not been combined with every dose of quinia, uninterruptedly, from the very beginning on the 11th of October, until ptyalism was produced. There was the more reason for this course, as he was a notorious drunkard, and had been on a drunken frolic since the 1st of October, on his arrival from Castle Pinckney.

CASES 63 and 64.—Private Charles Thappler, K, sober and well-behaved in all respects, was taken with yellow fever on the 14th October, and was treated principally by quinia, with a small quantity of calomel at the commencement. He had the usual symptoms at the beginning of the disease, and in the progress of the case his face became quite yellow. He went to quarters on the 1st November, and on the 7th, 8th, and 9th said he was fit for duty,

and on the last day he was marked "duty to-morrow." On the morning of the 10th he was brought to the hospital, and appeared to be very sick; intense pain of the frontal and orbital regions; eyes tender, suffused, and the conjunctival vessels were injected; face of a dusky red. Cal. 5 grs., quin. 10 grs., at 9; $\frac{1}{2}$ oz. ol. ricin. at 11; 10 grs. quin. at 1 and 5. *Night.* Cal. 1 gr., quin. 10 grs., opii $\frac{1}{2}$ gr., at 9, 1, and 5. *11th.* Quite yellow. Cal. 1 gr., quin. 10 grs., opii $\frac{1}{2}$ gr., at 9, 1, and 5. *12th.* Mouth sore. The patient was now put on small doses of quinia, brandy, mist. ammon. et camph., for a time, beef-tea, &c. Duty December 17.

It may be thought singular by those who contend that yellow fever attacks a person but once in a lifetime, and by those who consider an attack of the disease as a preservative against future attacks, provided the person does not spend a winter at the North, to find that this patient is registered as having had the fever twice in about one month. Many will consider the second attack as a relapse, but this is not my opinion, for the patient was well. A more singular instance than this occurred at Pascagoula in 1848. One of the hospital matrons, who occasionally drank too much, had severe yellow fever, and in its progress every symptom occurred which may be expected in this disease, except black vomit, including yellow skin, hemorrhage from the mouth and fauces, and uterine hemorrhage. She recovered, went on duty, and did her usual work for more than two weeks, went out of camp in all directions, got intoxicated, and at the end of all this she was again taken with yellow fever, had black vomit, and died. I did not think her last attack a whit more severe than the first, until black vomit made its appearance.

CASE 65.—Private Thomas Johnson, M, drunkard, was brought from the guard-house, in which he had been confined by sentence of court-martial, with yellow fever, at 10 $\frac{1}{2}$ o'clock A. M., October 19. This man was a corporal when his company arrived at the post on the 1st of October, but he had been beastly drunk, and had been tried, broken, and confined. On going into hospital he had every symptom of malignant yellow fever; skin of a dusky yellow (he had swarthy complexion); and in addition, the nervous system seemed to be in a state bordering on delirium tremens. Cal., quin. $\bar{a}\bar{a}$ 10 grs., morph. $\frac{1}{4}$ gr., ol. ricin. at 1 o'clock; at 3, quin. 10 grs. *Night.* Quin. 5 grs.; cal. 1 gr. at 6, 9, 1, and 5. *20th.* Quin. 5 grs. at 9, 12, 3, and 6. *Night.* 10 grs. quin. at 9, and 1, reveille (about 6 A. M.). Cal. 5 grs., morph. $\frac{1}{4}$ gr. at 9. *21st.* Cups and large blister to epigastrium; quin. 5 grs., cal. 1 gr., at 9, 12, 3, and 6. *Night.* Quin. 10 grs., cal. 2 grs., at 9, 1, and 5, and brandy and water. *22d.* Quin. 10 grs. at 9, 1, and 5, and brandy; same at night, and 5 grs. cal. at 9. *23d.* Quin. 5 grs., every third hour; 5 grs. cal. at 9 P. M. *24th.* Quin. 5 grs., cal. 1 gr., every third hour, day and night, and brandy. The calomel and quinia were continued until the 28th, when black vomit came on, and he died on the 29th. Perhaps there was an oversight in this case, in not giving the mercurial with every dose of the quinia, both day and night, from the very commencement. This was determined upon in every case which might occur hereafter. But this case was a bad one from the beginning, and it may be said that there was so much the more reason to push the remedies, to which opinion I agree.

CASE 66.—Private John Sheridan, M, sober, was taken with yellow fever on the 19th of October, and reported to me at 6 P. M. Cal., quin., $\bar{a}\bar{a}$ 10

grs.; quin. 5 grs., cal. 1 gr., at 9, 1, and 5, at night. 20th. Quin. 10 grs., at 9, 1, and 6. *Night.* Quin. 5 grs., at 9, 1, and 5; and cal. 5 grs., opium $\frac{1}{2}$ gr., at 9. 21st. Ol. ricin., 5 grs. quin., every three hours. *Night.* Quin. 10 grs., cal. 5 grs., morph $\frac{1}{4}$ gr., at 9; quin. 5 grs., at 1, and 10 grs. at 5. 22d. Quin. 10 grs., at 9, 1, and 5, and 5 grs. at same hours of the night, with 1 gr. calomel. 23d. Quin. 5 grs. every third hour; and at night 5 grs. every fourth hour, with 5 grs. cal. at 9. Brandy at same time. 24th. Quin. 5 grs. fourth hour. *Night.* Quin. 5 grs., cal. 1 gr., third hour; brandy. 25th. Quinia continued day and night, with 1 gr. cal. every dose at night; wine. 26th. Quin. 5 grs., cal. 1 gr., every three hours, and wine. At night, quin. 3 grs., cal. 2 grs., opii $\frac{1}{2}$ gr., at 9, 12, 3, and 6, and brandy or wine. 27th. On visiting the hospital this morning I was gratified to find the patient complaining of a very sore mouth, and on the 28th there was severe ptyalism. It must be confessed that, until the morning of the 27th, the patient's life was despaired of, and that I began sincerely to regret not having given calomel with every dose of quinia from the first. This man made one among eight sick whom I set down as fatal cases on a certain night visit, but fortunately only two of them died, Johnson and Schmidt. So ill were some of the sick in the last part of October. From the time that ptyalism fairly set in, Sheridan was treated with small doses of quinia, brandy, chicken-broth, beef-tea, &c. Duty, November 27th.

CASE 67.—Private Thomas Boyd, K, a great drunkard, was brought from the guard-house, where he had been confined for bad conduct since the 19th day of August, by sentence of general court-martial, to hospital on the 26th of October, about 10 o'clock A. M., with severe yellow fever. He had intense pain of the frontal and orbital regions; the eyes were tender, suffused, and the vessels were highly injected; the face was of a dusky brown (he has dark complexion), and altogether, the case was so unpromising, that I considered the result as settled. Mustard bath, and sinapisms; cal., quin., \bar{aa} 10 grs., and repeat at 2 P. M.; quin. 10 grs., cal. 1 gr., at 6. *Night.* Medicine had operated; quin. 10 grs., cal. 2 grs., opii $\frac{1}{2}$ gr., at 9, 1, and 5. 27th. Cal. 5 grs., quin. 10 grs., at 9, 12, and 3; sinapisms, cups, and a large blister to epigastrium. *Night.* Quin. 5 grs., cal. 2 grs., opium $\frac{1}{2}$ gr., at 6, 9, 12, 3, 6, to-morrow morning. 28th. Very sick; skin of an intensely dark-yellow. 8 A. M., cal. 10 grs., opium $\frac{1}{2}$ gr.; quin. 10 grs., cal. 2 grs., opium $\frac{1}{2}$ gr., at 10, 2, and 4. The medicine at 8 and 10 o'clock was doubtless retained; but the two other doses were probably rejected, for the patient vomited several times in the course of the afternoon. Before 4 o'clock P. M., while I was standing by him, he threw up a quantity of fluid which appeared to be, in every respect, commencing black vomit, and the result of the case was considered as settled. He threw up two or three times after this, the contents of the stomach each time being of a darker hue, and more nearly resembling real black vomit, and I had no reason in the world to doubt that the real *vomito prieto* had begun. He was directed to have nothing but light drinks, as small quantities of iced mucilage, and towards sunset the stomach became more quiet. 9 P. M. Has not vomited since sundown. Although the case was regarded as necessarily fatal, it was determined to try one dose more of medicine; and 10 grs. cal., and $\frac{1}{2}$ gr. opium were administered. Directions were given for him to take nothing for three hours after, and then to take a little wine and water, brandy and water, or such other drink as he might prefer; and I remarked to the steward: "If he does keep the medicine down, it will be of no more use than so much sawdust." 29th. On going to the hospital this morning, my first inquiry was about Boyd. The steward said he was

better, and I replied that I did not believe it, for it was scarcely possible; the steward said that he must certainly be better, for his stomach had been quiet all night, and he had a very sore mouth, to which I replied, "I don't believe it, but let's go and see;" and we found him with a sore mouth indeed, no uncertain one, but a severe ptyalism had begun; face swollen, tongue enlarged, teeth and gums very sore, and a decided mercurial fetor of the breath. From this date, small doses of quinia, brandy, chicken-broth, beef-tea, gargles, enemata, &c., were used; but it was several days before we could be persuaded that the patient would be saved, as I had never seen one recover with even commencing black vomit. Duty, November 27th. This case was saved, in my opinion, by the energetic administration of calomel and quinia from the first.

Only two more cases will be given from the fort, although others might be prepared, and these two cases will show the decided treatment pursued near the close of the epidemic.

CASE 68.—Antonio Knoche, ordnance soldier, temperate, was taken with yellow fever on the 31st of October. He was sent from the Charleston Arsenal to be tried by court-martial, on the 6th day of September, and had been confined since that time by sentence of the court. Very yellow. Quin., cal., $\bar{a}\bar{a}$ 10 grs., at 9 and 1; quin. 10 grs., at 5. *Night.* Quin. 10 grs., cal. 2 grs., opium $\frac{1}{2}$ gr., at 9, 12, and 3; and 5 grs. quin. at reveille. *November 1.* Quin. 10 grs., cal. 2 grs., opii $\frac{1}{2}$ gr., at 9, 1, and 5. *Night.* Cal. 1 gr., quin. 5 grs., opium $\frac{1}{2}$ gr., at 9, 12, 3, 6. *2d.* Quin. 5 grs., cal. 1 gr., opium $\frac{1}{2}$ gr., at 9, 12, 3, and 6; same at night. *3d.* Quin. 10 grs., cal. 1 gr., at 9, 1, and 5, and $\frac{1}{2}$ gr. opii at 9 and 5; same at night, and $\frac{1}{2}$ gr. opium each dose. Brandy. *4th.* Quin. 5 grs., cal. 1 gr., every four hours, day and night, and $\frac{1}{2}$ gr. opium with each dose at night; brandy. *5th.* Same. *Night.* Cal. 1 gr., quin. 10 grs., opium $\frac{1}{2}$ gr., at 9, 1, and 5, and brandy; and in addition, cal. 5 grs., opium $\frac{1}{2}$ gr., at 9. *6th.* Mouth sore. From this date took quin., brandy, opium, beef-tea, chicken-broth, &c. Although it was difficult to get the system under mercurial influence, and a large quantity of calomel had to be given, convalescence was rapid, and he went to duty on the 16th of Nov.

CASE 69.—Sergeant James F. Cross, M, sober, was taken with yellow fever on the 4th of November, and reported to me at 11 $\frac{1}{2}$ o'clock A. M. The pain of the frontal regions and orbits was very severe; eyes tender, suffused, and vessels injected; severe pains of the back and limbs; face of a dusky yellow, his complexion being swarthy. Cal. 5 grs., quin. 10 grs., and repeat at 3 P. M.; cal. 1 gr., quin. 5 grs., at 6 P. M. *Night.* Cal. 2 grs., quin. 10 grs., opium $\frac{1}{2}$ gr., at 9, 1, and 5, and cal. 5 grs., opium $\frac{1}{2}$ gr. at 9, in addition. *5th.* Very yellow. Quin. 10 grs., cal. 2 grs., opium $\frac{1}{2}$ gr., at 9, 1, and 5. *Night.* Cal. 1 gr., quin. 10 grs., opium $\frac{1}{2}$ gr., at 9, 1, and 5, and 5 grs. cal. and $\frac{1}{2}$ gr. opium in addition at 9. *6th.* Some appearance of ptyalism; 5 grs. quin. every three hours; $\frac{1}{2}$ oz. ol. ricin; at night, 5 grs. quin. every 4 hours; cal. 3 grs., opium, $\frac{1}{2}$ gr., at 9 P. M. *7th.* Ptyalism. Small doses of quinia, hereafter, brandy, chicken-broth, beef-tea, &c. Convalescence was rapid. Duty, Nov. 28th.

We will now notice some interesting cases wholly unconnected with the garrison of Fort Moultrie.

CASE 70.—Mary McDonough, wife of Edward McDonough, about 25 years of age, one child, and is *enceinte*, residing in a large shell of an old house, open

to the wind and rain, said old house being known as the "Island House," was ill on the 29th and 30th of September, and had undoubted yellow fever on the 1st of October. There were the usual intense pain of the frontal and orbital regions, suffused and injected eyes, &c. Calomel 10 grs., at the first visit, and no more was given during the disease, her situation being the reason; but quinia was freely given, say 10 grs. every four hours at first, and afterwards in 5 gr. doses. Oct. 3d. Profuse epistaxis; fortunately, no tendency to uterine hemorrhage; skin quite yellow. From the 3d, the date of the epistaxis and yellow skin, small doses of quinia were given, and brandy, chicken-broth, &c. She went to town two or three times a week, but never stayed over night. Recovered.

CASE 71.—Margaret McDonnell, Irish, 18 or 20 years of age, living with the McDonoughs, was taken with yellow fever on the 7th of October. She had been in Charleston but twice for three months, and never stayed there at night. Was there last on the 2d of October. Catamenial discharge in the week previous, but completed, and no uterine hemorrhage during the fever. Epistaxis on the 9th, and subsequently considerable hemorrhage from the mouth and fauces. She was put on cal. and quin.; on the third day the mouth was sore, and she recovered.

CASE 72.—Edward McDonough was taken sick with malignant yellow fever on the 26th of October. The symptoms were severe at the commencement; the supra-orbital pain was intense; eyes tender, suffused, and the conjunctival vessels were highly injected; skin of a dusky yellow, his complexion being swarthy. He was put on regular doses of cal. and quin.; sinapisms; cups; blisters, &c. 28th. Epistaxis, and very yellow. 29th. Mouth very sore; small doses of quinia, brandy, mist. ammon. et camph., chicken-broth, beef-tea, &c. Recovered.

A contagionist will say that Mary McDonough took the fever in town, and that it proceeded from her to the girl and Edward McDonough, who had not been in town for weeks. But if the disease was contagious, why did not the other members of the family suffer? There were several boarders, no less than eight; and only one, who went to town daily, and was drunk daily, had any kind of fever, the remaining seven escaping. More than this—several persons I know visited Edward McDonough, who had the most malignant fever of all, in all stages of the disease, and not one of them, to my certain knowledge, contracted any kind of fever; they had no fever during the year.

CASE 73.—Wagner, a Frenchman, discharged soldier, residence in Middle Street, east of the fort, and near the hotel, was taken with remittent fever on the 2d of October. This was a light case, and I did not visit him more than three or four times. He was in Charleston about a month previous, worked all day there, but did not stay at night. Went fishing about a week before the attack, and again the day before, and both times he was exposed to the evening air in the "creek" between Sullivan's and Long Island.

CASE 74.—Evaline, free mulatto woman, living with the above-named Wagner, was taken with severe yellow fever on the 19th of October. Intense pain of the frontal and orbital regions; forehead hot and dry; eyes very tender, suffused, and vessels highly injected; severe pain of the back and limbs. On the 23d, profuse uterine hemorrhage came on, and the sclerotic coats were very yellow. The skin was also of a very dingy and singular

appearance. For a few days she was very ill; recovered. She had been in bad health during the summer, being *enceinte*, and had not been from Sullivan's Island for more than three months; had seen no sick person since July except Wagner, and she had not been from her house since her child was born, on the 8th of September, until the day before she was attacked with this fever, when she called on a coloured girl from Santee, in a house near by, who was sick with country fever. To my certain knowledge, her health was so poor that she could not go from home for several weeks previous to delivery, on the 8th of September; and after her confinement she was so ill and infirm as to render it impossible for her to leave home until almost up to the very day of being herself seized with the fever. Again, there was no person sick with yellow fever with whom she could possibly come in contact between the 8th day of September and the 19th of October; or, indeed, from the last of August to October 19, for she was unable to leave the yard; which settles the question in regard to contagion. It is my deliberate opinion that this case originated on Sullivan's Island.

CASE 75.—Miss A. F——, in the sixteenth year of her age, was taken with yellow fever in Charleston on the 18th day of October, and came to Sullivan's Island on the 19th; but I was not called to see her until past 3 o'clock P. M., after she had been sick about thirty hours. She had been in Charleston about five weeks, from Canada East, her friends thinking it best for her to come south on account of a troublesome cough. Catamenia had never appeared. Having been ill so long the case presented a most unpromising appearance, and so the friends were informed. The patient took calomel, quinia, and ol. ricini on the first day, and afterwards some calomel with the quinia, and the latter was given as fast as was considered judicious; but the case progressed in the most malignant manner to the 21st, when there was great irritability of the stomach; and it is uncertain how much medicine was retained afterwards. Sinapisms, cups, and a large blister to the epigastrium had been used. 22d. Every appearance of the first stage of black vomit, which continued to increase until I had no hesitation in pronouncing that the fatal precursor had taken place. To take such drinks during the night as are most agreeable, iced or without, and a little brandy in some shape, if possible. 23d. Early; it was reported to me that the patient had thrown up several times during the night, and that about 3 o'clock the vomit appeared in the most decided manner. Since that time the stomach had remained quiet. I could easily credit these reports, both from what I had seen the day and night before, and from the present appearance of the patient, for she appeared to be *in extremis*; pulse frequent and feeble; skin with the peculiar yellow fever sensation to the touch, and clammy; extremities cold; and the eyes, face, and neck intensely yellow. External applications; ices, and iced mucilages; brandy in small quantities in some shape; and mist. ammon. carb. ʒj every two hours. In truth, she was considered moribund. Hemorrhage from the nose and mouth came on in the early part of the day, continuing at intervals during the 24th and 25th; after the 25th there was but little hemorrhage. On the 24th, the stomach remaining quiet, and the mist. ammon. appearing to agree better with that organ than anything else, the patient was directed to take a drachm of it every hour, day and night, unless asleep; and as there was troublesome cough and restlessness, ¼ gr. morphia was given every night from this date. Brandy, in some form, was recommended, if she could be prevailed on to take it; and a little chicken tea was ventured upon. Mist. camp. was afterwards combined with the ammonia mixture. The young lady finally recovered, most unexpectedly to

myself, and the severity of the fever may be inferred from the fact that she lost her hair subsequently.

This case and Boyd (Case 67) are the only ones I have ever known to recover after black vomit had come on, ever so slightly, in all my experience. The Charleston physicians report a number of recoveries after the occurrence of this usually fatal symptom; more, I believe, than in former epidemics. We have all read of these recoveries, and no one had reason to doubt the fact of their sometimes occurring, but I never expected to see an instance. These two patients were very different; Boyd was a very great drunkard, and the original organization was worn out and could not be trusted; this young girl had led an innocent life, and though there was tendency to pulmonary disease, much might be trusted to nature. In both of these cases, however, there was only incipient black vomit, the inner coat of the stomach remaining intact, for we can scarcely conceive of recovery taking place after confirmed black vomit, when the mucous coat is in a state of disorganization.

During the fever of this young lady, there were four white females who were pre-eminently exposed, being in the room and about the patient in all stages of the disease, and not one of them contracted it. I am not certain whether one of them, an elderly lady, had suffered an attack of this fever or not, but I am positively certain that the other three never had the disease, and two of them had been in the low country, of the South not more than a year or two. And let me remark in this place, that in all cases where persons are mentioned as not contracting yellow fever, though they had been exposed to it, it is intended to assert that none of these persons so exposed have ever had the disease. Besides these females, there were seven male boarders in the house who did not enter the patient's room, not one of whom had yellow fever, and only one was at all ill. This man had slight remittent at the time the young lady came from town, and I recommended infusion of *Serpentaria Virginiana*, and nothing more was necessary. I saw the man but once, so light was his fever. These were the only cases of fever in the house during the year; and how could all these persons, especially those three females in attendance, escape the contagion of so malignant a case?

CASE 76.—T. C——, brother of J. C—— (see Case 47), came from Charleston sick with yellow fever on the 14th of October. Complexion dark, now of a dusky appearance, and on the 15th the skin was of a dark yellow, and the sclerotic coat of the eye was yellow also. The treatment of this case consisted mainly in giving moderate doses of quinia at first, with which a grain or two of calomel was often combined, the tongue being loaded; afterwards mist ammon., brandy, chicken tea, &c.; and in due time he recovered, though greatly reduced.

In this house of J. C. (see Case 47) there were two cases of yellow fever during the epidemic; and in the family there were two young ladies and an Irish servant girl, who were exposed in Mrs. C.'s case, and Mr. C. was exposed in both cases; there were six male boarders, all of whom were exposed in the

case of T. C.; and there were also three children in the family—making 7 men, 3 women, and 3 children, or a total of 13 persons in all. Not one of them had fever during the season. Not one of the 10 persons, seven men and three women, ought to have escaped, if yellow fever is contagious.

CASE 77.—Mrs. Harris, wife of Rev. Matt. Harris, Post Chaplain, was taken with yellow fever in the night of the 3d of November, and early the next morning I saw her. She had the usual symptoms; intense pain of the supra-orbital regions, suffused eyes, &c. On the 5th, there was epistaxis. Recovered. The residence of Mr. Harris was within one-quarter of a mile of the steamboat landing, the first door west of the Episcopal church, and about midway between Middle Street and the back beach. This lady had not been to Charleston since July, nor had she seen a fever patient during the whole summer.

CASE 78.—Mary Wagner, widow, residing in the family of Mr. Harris during the season, was taken with remittent in the livery of yellow fever, on the 10th of November. She had severe supra-orbital pain; eyes tender, suffused, and changed in appearance; dusky appearance of the face, though a blonde; severe pain of the back and limbs. Was taken with a severe chill. She had not been from Sullivan's Island for six weeks, nor had she seen any fever patient.

CASE 79.—Mr. Harris was taken with simple remittent fever, without a single sign of yellow fever, on the 16th of November. This was a case of undisguised remittent fever, of the simplest form. It was ushered in with a chill, and there was an evident tendency to a cold, hot, and sweating stage, every afternoon during its progress. The only untoward occurrence was an accidental ptialism; 10 grs. cal. was given on the first visit, followed in a short time by ol. ricini, which acted admirably; and, three or four days afterwards, 5 grs. mas. hydrarg. was given in combination with other eccoprotics, which acted sufficiently in a brief period. These were the only mercurials exhibited. The patient had not been in Charleston for three weeks, but he had seen yellow fever, of the most malignant character, repeatedly, during the epidemic.

If Mrs. Harris had seen a single case of fever, it might be said that she contracted yellow fever by exposure to contagion. But she saw no person with fever during the epidemic; and the same may be said of Mary Wagner, who saw no sick person except Mrs. Harris. Mr. Harris, however, did see yellow fever, of the most malignant character, repeatedly, during the epidemic, as I have reason to know; and if his had been the first case in the family, no matter how mild and how simple the remittent, it would be said that he introduced the contagion from abroad. The natural order of events, according to the doctrine of contagion, was inverted, for those who might bid defiance to the disease had it worst, and the one who was repeatedly exposed had only a mild remittent, at the close of the season, the case of Mr. Harris being the last fever which occurred in my practice in 1852. Mrs. Harris's case indubitably originated on Sullivan's Island, for we can account for it in no other way. If yellow fever is contagious, three white persons (two nieces and an Irish girl, none of them children) ought to have taken it, for they were all exposed.

CASE 80.—Mrs. O., residing on the back beach, northeast of the fort, was taken with remittent fever on the 15th of November. In a few days this changed into an obstinate quotidian intermittent, over which quinia appeared to have no control, and which yielded to Fowler's arsenical solution. On the 7th of January, 1853, she was taken with tertian intermittent, which lasted until the 23d. On the 14th of February she was attacked with a tertian, which lasted until the month of July succeeding, without interruption, when it gradually broke up. Many remedies were tried without effect, and the disease seemed to yield only to change of season. This patient ought to have gone North, or to Aiken, or to Buncombe, for change is essential in chronic intermittent. She had not remained in Charleston over night since the 4th of July, 1852, and had not been there during the day for more than three weeks previous to the 15th of November. Resided on Sullivan's Island during the whole summer.

We have done with the cases, and if they appear too numerous, it must be recollected that they were introduced in order to show the nature of the treatment, to show the origin of the fever, and to show that it was not contagious in the slightest degree.

Treatment of Yellow Fever.—The general plan may be seen from the cases; adjuvants being almost wholly omitted from the reported treatment. Calomel and quinia were given in all cases from the beginning, and the former was never omitted from each dose in the last part of October, when the disease assumed a more malignant character than before; and it may be remarked in this place, that if a mistake was made in treatment during the whole epidemic, it was in the cases of Fitzpatrick and Johnson (Cases 62 and 67), in which calomel ought to have been pushed with every dose of quinia to downright ptyalism. It is my opinion that each of these men might possibly have been saved, though notoriously great drunkards, and bad subjects, by this course of treatment, for no other method could have done it; but perhaps I have more reason to rejoice that the mortality was no greater. Fitzpatrick was strongly suspected, also, of having "played old soldier." Compare these two cases with some others, particularly cases 68 and 71. Bilious fever was treated on the same principles; the lighter the case, the more care was taken to avoid ptyalism.

It affords me great pleasure to bear testimony in favour of this plan of treatment during the epidemic, which was the same as that pursued by myself at Vera Cruz, in the summer of 1847; and this is the more cheerfully done, because it is not at present the fashionable treatment, like oil and lemon juice, snakeroot and salts, verberna, &c. &c. From the thousand infallible specifics advertised in the medical journals and newspapers for the cure of yellow fever, it may be inferred that every plan of treatment is somewhat uncertain, and that we have to contend with a malignant disease. The more *certain cures* we see advertised for any disease, the more uncertain is the treatment; and we see, notwithstanding infallible treatment, that yellow fever patients do die, as at New Orleans, Havana, Bermuda, &c. It may be said that my patients escaped in spite of the doctor, and this could be said if I had lost 15 or 20 of

them, in 48 cases of severe and malignant yellow fever, instead of 4 in 48; and, in reality, the physician ought not to be held responsible for more than 2 cases out of the 4 fatal ones (see Cases 58 and 59). There was, however, a mortality of 4 in more than 100 cases of fever at Fort Moultrie. If mercurials are so injurious in yellow fever as is contended by many, whether by dissolving the blood, by creating great nervous irritation, by exciting inflammation of the mucous coat of the stomach and duodenum, or for any other reason, most of my patients ought to have died; but did they die? We will see when we come to the statistics of the epidemic.

Venesection was not practised in a single instance; cups were freely employed, but leeches were not used in any case, cups answering every purpose. Anodynes were in universal use, particularly at night. Ice was freely used in a few special cases, but it was not generally required. Sinapisms and epispastics were freely applied. Brandy, in some shape agreeable to the patient, was universally used, more so, perhaps, than in any epidemic I have witnessed; and, pretty early in the disease, with the best effect, in the cases of temperate females as well as with old topers. Mist. ammon. carb. was employed in the advanced stages of some of the most malignant cases, with excellent effect. Ale and wine were sometimes given when the brandy did not agree with the stomach. So soon as the first stage of the disease had somewhat passed, chicken and beef-tea were given in small quantities. It is not necessary to go more into detail.

Symptoms have not been detailed. It is easy to perceive, from almost every case, what are considered diagnostic symptoms, and the rest have been almost wholly omitted. Pain of the back and limbs has not been much noticed, for it is by no means pathognomonic, occurring in every southern fever. It is mentioned in this place for the purpose of alluding to the disposition, generally prevalent at the commencement of the yellow fever, to call all fevers *dengue*, after its predecessor of 1850, when these pains were so common; but they were just as common and severe in this fever. They are common in all southern fevers. The pulse was slow, frequent, full, weak, &c., in all stages of the different cases. The tongue is no criterion. It was nearly natural in appearance, flabby, white, yellow and loaded, brown and moist, brown and dry, black, had red edges, &c., in different cases. The organ presented almost every variety of appearance.

Little will be said concerning the nature of yellow fever. When the disease first occurred to me it was regarded as *sui generis*—as different from all other southern fevers, but it must be confessed that this opinion has been considerably modified; and at the present time it is believed that intermittent, remittent, continued, congestive, and yellow fevers are nearly related, if not modifications of the same fever, all being southern bilious fevers, the nervous system in some and the blood in others being pre-eminently affected. Whether the difference in these varieties of southern fever, in different seasons and in the same season, depends on a simple difference of intensity in the predisposing and exciting causes with one and the same *materies morbi*; whether

different causes exist at the same time, developing the different forms of fever; or whether there is a blending and conversion of types, as is maintained by Dr. Dickson—all remains to be determined. It is easy to diagnosticate a mild, open, remittent fever from a malignant yellow fever; but the different rounds between the extremities of the ladder are so numerous as to render the central one, the dividing line, difficult to distinguish. This was particularly so in the fevers of 1852, and some cases were registered as remittent fever, in the month of September, which might have been, with equal propriety, named and reported yellow fever. As the fevers became more malignant in the month of October, there was no sort of difficulty in determining their true character.

STATISTICS OF THE EPIDEMIC.

I. *Table showing the Class of Persons attached to the Garrison of Fort Moultrie, with their Febrile Diseases and Mortality.*

DESIGNATION.	Strength.	No. cases of yellow fever.	No cases of other fevers.	Total number of all fevers.	NO. SICK PER 1000 WITH			DEATHS FROM		MORTALITY PER CENT. FROM		Deaths per 1000.
					Yellow fever.	Other fevers.	All fevers.	Yellow fever.	All other fevers.	Yellow fever.	All fevers.	
Officers	9	...	4	4	...	444	444			
Enlisted men	112	33	27	60	295	241	535	4	...	12.12	6.66	35
White male adults, not military	9	...	3	3	...	333	333			
White adult females	29	8	15	23	276	517	793			
White children	25	...	11	11	...	440	440			
Coloured male adults	2			
Coloured adult females . . .	7	...	2	2	...	285	285			
Coloured children	7	...	2	2	...	285	285			
	200	41	64	105	205	320	525	4	...	9.75	3.80	20

II. *Table showing the Exposure, Fevers, &c., of different Classes of Persons during the Epidemic.*

DESIGNATION.	Strength.	Cases of yellow fever.	Other fevers.	Total number cases of fever.	Drill duty.	Guard duty.	Fatigue, duty, or previous exposure.	Provision for respiration.
Officers	9	...	4	4		Light	Light	Very good.
Enlisted men	112	33	27	60		Severe	Severe	Good.
Officers' ladies	7	1	4	5		...	None	Very good.
Soldiers' wives	18	7	9	16		...	Severe	Good.
White female servants	4	...	2	2		...	Light	Good.
White males, not soldiers	9	...	3	3		...	Moderate or light	Good.
White children	25	...	11	11		...	None	Good.
Coloured males	2	Light	Moderately good.
Coloured females	7	...	2	2		...	Moderate	Moderately good.
Coloured children	7	...	2	2		...	None	Moderately good.
Total	200	41	64	105				

III. Table showing the Habit of Intemperance in different Classes of Persons belonging to the Garrison, with their Febrile Diseases, &c. &c.

DESIGNATION.	Strength.	Sober.	Drinkers.	Hard drinkers.	Drunkards.	SOBER PERSONS.		DRINKERS.		HARD DRINKERS.		DRUNKARDS.		DEATHS.		Deaths per 1000.
						No. cases yellow fever.	Other fevers.	Yellow fever.	Other fevers.	Yellow fever.	Other fevers.	Yellow fever.	Other fevers.	Yellow fever.	Drunkards.	
Enlisted men of Company E	21	12	2	3	4	1	4	...	2	...	1	1	4			
Do. Company K	45	20	6	12	17	5	6	...	2	2	1	5	5			
Do. Company M	43	18	3	8	14	7	2	...	9	1	4	4	93
Do. other corps	3	1	...	1	1	1	1			
Soldiers' wives	18	18	7	9			
White female servants	4	4	2			
White males, not soldiers	9	5	1	2	1	...	2	1			
White children	25	25	11			
Coloured males	2	2			
Coloured females	7	7	2			
Coloured children	7	7	2			
Total	184	117	12	16	39	21	38	...	4	4	3	15	11	4	4	22

IV. Table of Comparison between Companies K and M, from the 1st of October to the end of the Epidemic.

	Sober men.	Drunkards and drinkers.	Total.	SOBER MEN.		DRINKERS & DRUNKARDS.		Cases of yellow fever per 1000.	Cases of other fevers per 1000.	Deaths.	Deaths per 1000.	REMARKS.
				Cases of yellow fever.	Cases of other fevers.	Yellow fever.	Other fevers.					
Enlisted men of Company K	20	25	45	5	...	6	1	244	22	In every table, and in all cases, it is to be recollected that Company M was not present in September.
Enlisted men of Company M	18	25	43	7	...	11	1	419	23	4	93	

V. Table of different Classes of Persons, not Military, pertaining to the Garrison, with their Febrile Diseases.

	Number of persons.	No. cases of yellow fever.	No. sick per 1000 with yellow fever.	No. cases remittent fever.	No. sick per 1000 with remittent fever.	No. cases in all. Total fevers.	No. sick per 1000 with all fevers.	Deaths.
Officers' ladies	7	1	143	4	571	5	714	None.
Soldiers' wives	18	7	389	9	500	16	889	
White female servants	4	2	500	2	500	
White males	9	3	333	3	333	
White children	25	11	440	11	440	
Coloured adult males	2	
Coloured adult females	7	2	286	2	286	
Coloured children	7	2	286	2	286	
Total	79	8	101	33	418	41	519	

VI. *Table of Mortality in Yellow Fever in all the Cases Treated.*

	Number of cases.	Deaths.	Mortality per cent.
Enlisted men	33	4	12.12
Women of garrison	8
Not of garrison—citizens . .	7
Total	48	4	8.33

The first table requires no explanation. The second, third, and fifth tables give the different classes of persons attached to garrison more distinctly than the first, the previous exposure, provision for respiration, and their character for sobriety, with their febrile diseases and mortality. Under the head of officers' ladies are included visitors, who remained through the epidemic in precisely the same condition as the class in which they are included. Among soldiers' wives are included several women who assisted as laundresses, who married soldiers then, or shortly after, and whose situation in every respect, duty, fatigue, sobriety, &c., was the same as the class into which they are incorporated. This class of persons was temperate. They were exposed to great fatigue during the heavy rains, and the cases of fever were no less than 889 per 1,000, but there was not a single death. The mortality among enlisted men was 35 per 1,000; and in Company M, the only company which lost men, the mortality was 93 per 1,000. The provision for respiration was good for both classes, the fatigue and exposure were great in both classes, the drunkenness was great among the men, and there was none of it among the women; and although the sickness per 1,000 among the enlisted men was 535, and among the camp-women 889, the mortality was nothing in one class, and in the other 35 per 1,000, or 6.66 per cent. of all fevers. The fevers of this year, both remittent and yellow, were universally more easy to manage among females than in men. The third table shows the fevers among drunkards, with whom is classed one habitual opium-taker. From this table it appears that Company E had rather more cases of all sorts of fever than Company K, or 619 per 1,000, while K had 578 per 1,000; but the number of yellow fever cases was much less in E, amounting to only 95 per 1,000, while the number sick with yellow fever in K was 267 per 1,000; and in Company M, the yellow fever cases were 419 per 1,000. The number of soldiers' wives sick per 1,000 with yellow fever was 389; other fevers, 500; all fevers, 889, as seen in the fifth table, but there was not a single death. To what can this difference be ascribed? To what is the exemption of Company E from yellow fever, as compared with Company K, to be attributed, unless to the comparative freedom from hard drinkers and old drunkards? The two companies had been side by side during the whole year, and their barrack-rooms, duties, and exposures, had been alike in every respect. The following table exhibits the prevalence of fever among sober men and drunkards in the respective companies:—

Company.	Strength.	SOBER MEN.								HARD DRINKERS AND DRUNKARDS.							
		Number.	Yellow fever.	Other fevers.	All fevers.	Sick per 1000 with			Deaths.	Number.	Yellow fever.	Other fevers.	All fevers.	Sick per 1000 with			Deaths.
						Yellow fever.	Other fevers.	All fevers.						Yellow fever.	Other fevers.	All fevers.	
E . .	21	12	1	4	5	83	333	416	...	7	1	5	6	143	714	857	...
K . .	45	20	5	6	11	250	300	550	...	19	7	6	13	368	316	684	...
M . .	43	18	7	...	7	388	...	388	...	22	11	1	12	500	45	545	4

Company M was not present in September, and there was little fever other than yellow, in the month of October. The September cases are included in Companies E and K, and are almost entirely remittent—all but one.

There were three men of other corps present during the whole epidemic, in whom yellow fever is apparently in an inverse ratio to the above statement; but this is not in reality the fact, for a sober man who had yellow fever was undergoing punishment (hard labour, confinement, &c.) by sentence of general court-martial; the hard drinker, who had mild remittent fever, was on light duty, and was but little exposed; and the one who escaped all fever, a thief, a liar, and a drunkard, also undergoing punishment for desertion by sentence of general court-martial, was an exception to all rules, being completely hardened, both morally and physically.

We have high authority for asserting that intemperance is a strong predisposing cause of fever in hot climates. Dr. Carpenter:—

“Its efficacy has been generally attributed to the general disorder of the nutrient process, and to the weakening of the vital powers, which it tends to induce; but to us it appears that it possesses a more direct and special action. One of the best-established among the consequences of the introduction of alcohol into the blood, is its rapid oxidation, whereby it is itself eliminated from the circulating current; but, in thus greedily appropriating to itself the oxygen which the respiratory process supplies, it *prevents* the oxidation of other substances, of which it is one of the special objects of that process to get rid; thus tending to induce the same condition of the blood, as that which is consequent upon obstructed respiration. And the peculiar potency of this cause in hot climates, where the oxidating process, as measured by the production of carbonic acid, does not take place at above half the rate at which it is carried on in a colder atmosphere, is a strong confirmation of this view.

The sixth table gives the number and mortality of the yellow fever cases, and at the risk of repetition, is intended to show the result of the practice pursued. A comparison of the cases with this table shows that the main remedy, one in universal use, the combination of calomel and quinia, was not so inefficacious, nor so prejudicial, as has been represented by many physicians. If the combination had been inefficacious, there must have been a large mortality; but it is scarcely probable that two such powerful articles of the *Materia Medica*, which show their effects on the system so obviously, should prove inert, and their influence in this epidemic, either for good or evil, is undoubted. If prejudicial in their action, the mortality must have been frightful, for both articles were administered so freely that scarcely a chance was left for recovery. But there was no such mortality, and the treatment

was as successful as any mode of practice could be expected to prove in any circumstances. Much has been said against this mode of treating the disease, but what plan of treatment has ever proved infallible? A clerical gentleman very kindly remarked to me one day, that he did not know of a more disgusting sight than to go into the wards of an hospital filled with sick undergoing ptyalism; I replied it was disagreeable, but to me it was not half so disagreeable a sight as to enter the wards of an hospital filled with patients having black-vomit. The occurrence of ptyalism in this yellow fever was never an unpleasant sight to me; on the contrary, it was always hailed as the harbinger of recovery. If a simple mode of treating yellow fever can be devised, by which it may always be cured, *cito, tute, jucunde*, no one will more cheerfully follow it than myself, to the exclusion of calomel, quinia, cupping, blistering, and all other unpleasant remedies.

Our limits will permit of but a single remark on the *modus operandi* of quinia. It is regarded by a very great majority of the profession as a sedative, but since the free use of it in the severe and malignant fevers of Florida, in 1839, '40, and '41, I have regarded it as having a peculiar stimulant action on the nervous system; nor have I since seen reason to change the opinion. The salts of quinia seem to have a peculiar affinity, if we may so speak, for nervous matter, and hence their beneficial action in many diseases. Dr. Stevens believed that the sulphate has an effect on the blood in malignant fevers, though not to the extent of some of the other alkaline salts.

It may be said that this epidemic was of a mild character. It was not so malignant as that of St. Augustine, Florida, in 1841; nor was it so severe as the yellow fever at Pascagoula, Mississippi, in 1848, though more prevalent; but it appeared to be as grave a fever as that in Vera Cruz in 1847, though the mortality, from circumstances, was immensely greater in that town than on Sullivan's Island and at Fort Moultrie. A glance at the cases will show that the epidemic was not of a mild character. On a certain night visit in October, ten patients were considered in a very hazardous situation, but fortunately only two of them were lost; the others recovered under the free use of calomel and quinia, which ought not to have been the case if the remedies were pernicious. The following table shows the number of all cases during the epidemic months, with the exception of three fever cases in August.

MONTHS.	DESIGNATION.	Strength.	Remittent fever.	Yellow fever.	Cholera infantum.	Cholera morbus	Dysentery acute.	All other diseases.	Total No. of cases.	Died.	Cause of death.
September	Officers and enlisted men	71	24	1	...	1	...	3	29	...	Encephalitis.
"	Women, children, servants, &c.	61	15	2	...	2	2	4	25	1	
October	Officers and enlisted men	121	3	29	...	1	1	4	38	3	Yellow fever.
"	Women, children, servants, &c.	79	12	5	2	...	3	2	24	...	
November	Officers and enlisted men	107	1	3	2	13	19	1	Yellow fever.
"	Women, children, servants, &c.	74	6	1	1	...	2	2	12	...	
Total number of sick . . .			61	41	3	4	10	28	147	5	

Every case of cholera infantum, cholera morbus, and dysentery given in the preceding table, occurred in persons who had not been from the island for weeks, and some of them had never been from the island in their lives. Every case originated on Sullivan's Island. Of the remittent fever almost every case originated here; for after the public notification of July 24, and the subsequent post regulation, not a man was permitted to stay in Charleston over night, and few wished to go there; nor did the women and children who had these diseases go to town at all. It has been already shown that the yellow fever originated on Sullivan's Island, having proved in the preceding pages that several cases of genuine disease—the very first cases too—had their origin here, among whom, in their order, are Sergeant McNair, Mary Ann Brassard, Margaret McNair, Deborah Doherty, Evaline, and Mrs. Harris, all of whose cases indubitably originated on this island, neither Charleston nor Castle Pinckney having had any agency in their production. About the origin of the disease there can be no question, for the cases of Sergeant McNair, Mary Ann Brassard, and Margaret McNair settle it, without having recourse, as might be done legitimately, to an abundance of other fever cases (See Cases 1, 5, 6, 7, 9, 10, 13, 14, 15, 18, 19, &c.) during the epidemic.

The foregoing tables do not perfectly agree with the quarterly reports of sick in a few particulars, the mean strength not being the same as the full number of persons. (To be continued.)

ART. III.—*On the Movements of the Glottis in Respiration.* By JOHN C. DALTON, JR., M. D. New York. (With two wood-cuts.)

EVERY one must have observed, in examining the respiratory passages after death, how great a disproportion exists between the opening of the glottis and the caliber of the trachea below. The glottis itself presents the appearance of a narrow chink, while the passage for the inspired air widens in the lower part of the larynx, and in the trachea constitutes a spacious tube, nearly cylindrical in shape, and over half an inch in diameter. In an adult male subject, in whom the respiratory passages were healthy, the space included between the vocal chords had an area of from 0.15 to 0.17 square inch; while the caliber of the trachea, in the middle of its length, was 0.45 square inch. It is evident, therefore, that for the purposes of respiration, either the glottis is too small or the trachea unnecessarily large; at least, if we suppose the condition of the larynx after death and during life to remain the same.

There are, in Longet's work on physiology, now in course of publication at Paris, in the article on the pneumogastric nerve, several allusions to a dilatation of the glottis during life, by which its capacity is increased in inspiration whenever more air than usual is required. Longet, however, appears to

regard this dilatation of the glottis as uniform and permanent during ordinary respiration, while there is an alternate enlargement and diminution of its caliber only when the respiratory movements become forced or hurried. His words are (vol. ii. p. 314): "Lorsque la respiration est calme, on ne peut constater rien autre chose qu'un écartement permanent de la glotte ou des narines; quand elle devient gênée, ces orifices se dilatent d'abord outre mesure, puis reviennent bientôt à leurs dimensions normales." The author speaks of examining the condition of the glottis, and the effect upon it of dividing the superior and inferior laryngeal nerves by simply "drawing the larynx forward." I have myself never been able to obtain any satisfactory view of the glottis in living animals by this means. In the dog, even when the animal has been completely etherized, and the larynx drawn forward as much as possible by laying hold of the epiglottis with hooked forceps, I have not succeeded in getting anything more than a very indistinct view of the entrance to the larynx, and the vocal chords were always too deeply situated to be seen at all. I had recourse, therefore, during the past winter, to the following experiment, which I have repeated some half a dozen times since. The animal is to be etherized, and laid on its right side. The left common carotid is then to be tied, and the dissection continued so as to expose the œsophagus and the lower part of the pharynx. If an incision be now made into this canal, extending from just above the hyoid bone some two or three inches down the œsophagus, the larynx can be readily turned over from behind forward so as to bring into full view its posterior surface, the glottis, and the vocal chords. The observation can be continued for an indefinite time, by occasionally placing a sponge, moistened with ether, over the entrance to the larynx, and so keeping the animal in a condition of insensibility. These experiments have demonstrated that *there is, during normal respiration, a constant and regular movement of the vocal chords, by which the size of the glottis is alternately enlarged and diminished, synchronous with the inspiratory and expiratory movements of the chest.* These motions are of the same character as the general automatic movements of respiration, of which they in fact form a part. It is at the same time, and by the same nervous influence, that the chest expands to inhale the air, while the glottis opens to admit it. When the chest collapses, the glottis returns to its original size, and the air is expelled through it from below.

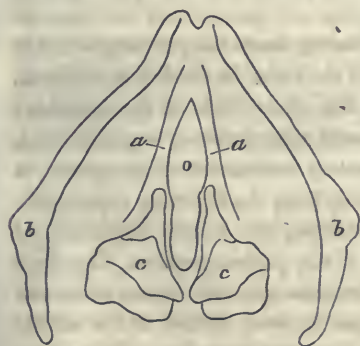
These motions, which may be called properly enough the "respiratory movements of the glottis," are increased in force when the respiration becomes excited, the glottis opening more widely as the chest expands more fully. The variation in its size is also more strongly marked when the animal whines or cries. Then the glottis not only collapses during expiration, but is strongly contracted, and is actually narrower than in a state of rest, the chords being closely approximated so as to produce a vocal sound. But it is not necessary to the continuance of the movements that there should be any production of sound or any disturbance of the respiration.

Notwithstanding the gravity of the operation necessary to expose the la-

ryn timer, when the animal is thoroughly etherized the respiration is often perfectly as quiet and regular as in the natural condition; yet the movements of the glottis continue to be performed. At one time, I attempted to stop them by pushing the etherization to excess; and they, in fact, ceased after prolonged application of the sponge, but only when all the respiratory movements were suspended together. When the motions of the chest and diaphragm began again, the movements of the glottis recommenced at the same time. Another fact, which I have twice verified, tends to show that this dilatation of the glottis is not, as Longet supposes, an extraordinary movement caused by the demand for an unusual supply of air to the lungs, but a part of the ordinary respiratory process. If a large opening be made in the trachea below the larynx, the movements of the glottis go on as before, notwithstanding an abundant supply of air is admitted by the artificial orifice.

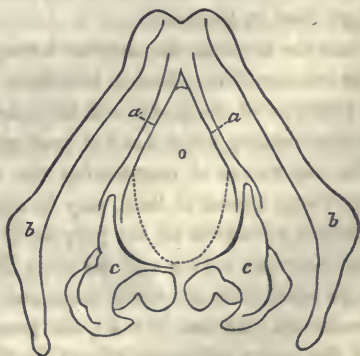
The separation of the vocal chords during inspiration, and the consequent enlargement of the glottis, is produced by the action of the posterior crico-arytenoid muscles. By their contraction the arytenoid cartilages are rotated outward and downward, and at the same time drawn backward, so that the opening of the glottis is lengthened as well as widened. The movement may be readily produced after death on the fresh larynx of the calf by irritating the first terminal branch of the inferior laryngeal nerves. It will then be seen that the glottis opens, and at the same time the arytenoid cartilages are rotated outward. It can also be imitated on the human larynx by exposing the crico-arytenei postici, and drawing upon these muscles in the direction of their fibres. By this means the size of the glottis may be increased from 0.15 to 0.27 square inch. As the vocal chords are composed mostly of elastic tissue, it is easy to understand how they can be at the same time elongated and separated laterally.

Fig. 1.



Human larynx, viewed from above, in its ordinary *post-mortem* condition. *O*, opening of the glottis; *a*, vocal chords; *b*, thyroid cartilage; *c*, arytenoid cartilages.

Fig. 2.



The same, with the glottis opened, by separation of the arytenoid cartilages.

As the respiratory movements of the glottis are produced by the action of the posterior crico-arytenoid muscles, they are, of course, entirely under the control of the recurrent or inferior laryngeal nerves; and the section of these nerves instantly puts a stop to them, however vigorous they may have been previously. The glottis can then be neither expanded nor contracted, but remains collapsed, and even partially closes at every forcible inspiration. This tendency to occlusion of the glottis after section of the recurrences, which has been noticed by several experimenters, is a consequence of the paralysis of the laryngeal muscles. The vocal chords, having no longer any active movement of their own, form a double valve; and are forced together by the column of inspired air. The difficulty of inspiration which results is much more strongly marked in young than in adult animals. If the recurrent nerves be divided upon a pup two or three weeks old, the inspiration becomes at once excessively difficult, and has a peculiar choking or *sucking* character, which immediately indicates the larynx as the seat of the difficulty. In older animals, the section of these nerves, as Longet has already observed, produces less disturbance of the respiration, owing to some anatomical differences in the structure of the larynx. According to him, the anterior processes of the arytenoid cartilages, which form the borders of the glottis in its posterior part, are longer in proportion to the vocal chords in the adult than in the young animal; so that in the adult, after paralysis of the laryngeal muscles, the glottis will still be kept pervious, in a considerable portion of its extent, by the unyielding arytenoid cartilages; while, in the young animal, the flexible vocal chords will fall together, and close the opening almost entirely. I have not been able to convince myself of this difference in the proportionate size of the cartilages at different ages; though it is very certain that in old animals the glottis still remains somewhat permeable after section of the recurrences, while in the young it closes much more completely. The difference appears rather to be dependent on a difference in the position of the arytenoid cartilages than in their size.

As the fibres of the inferior laryngeal nerves leave the pneumogastrics in the interior of the chest, it is evident that if the latter be divided in the neck, a paralysis of the glottis will be added to the other effects of the operation; a circumstance which has not been sufficiently appreciated by experimenters. The importance of maintaining the respiratory movements of the glottis, in the young animal at least, may be understood from the following experiment, which I undertook for the purpose of ascertaining the relative importance of the recurrent nerves and the other fibres of the pneumogastrics: Two pups were taken from the same litter, and of the same size and vigour, about two weeks old. In one (No. 1), the pneumogastrics were divided in the middle of the neck; and in the other (No. 2), at the same time, the inferior laryngeals. For the first few seconds after the operation there was but little difference in the condition of the two animals. There was the same struggle for breath, the same gasping and *sucking* inspiration, and the same frothing at the mouth.

Very soon, however, in No. 1, the respirations became much reduced in frequency (10, 8, and 5 per minute), as usual after section of the pneumogastrics, while in No. 2 they continued frequent as well as laborious, and the general signs of agitation and distress were kept up for one to two hours. The animal, however, after that time became exhausted, cool, and partially insensible like the other. They both died between 30 and 40 hours after the operation. The peculiar congestion and solidification of the lungs, considered as characteristic of section of the pneumogastrics, existed to a similar extent in both; and the only appreciable difference between the two bodies was that in No. 2, the blood was fluid, and the abdominal organs congested, while in No. 1, the blood was coagulated, and the abdominal organs natural. This experiment affords additional evidence of the two following facts, which have been already more or less generally acknowledged by experimenters:—

First.—After section of the pneumogastrics, death is produced by congestion of the lungs.

Second.—This congestion is not a direct effect of the division of the nerves, but is caused by the imperfect admission of air into the chest.

ART. IV.—*Extracts from the Records of the Boston Society for Medical Improvement.* By WM. W. MORLAND, M. D., Secretary.

February 13. Imperforate Rectum.—Dr. PARKMAN reported the case. The patient 52 hours old; anus perfectly formed; obstruction of rectum nearly two inches within the anus. Dr. P. waited 18 hours, until the septum became distended by collected meconium, and then punctured the pouch with a trocar; injecting afterwards, and washing out the bowel. This process was repeated, but the opening not remaining free, a director was passed into the anus and through the opening made by the trocar, and the sphincter ani and the septum were divided, from before backwards, by a free incision; the finger could then be passed, and went into a large cavity. After the operation, the child took the breast readily, and is now doing well, three months after the operation. Dr. P. directed an attendant to pass a well-oiled finger within the opened intestine daily.

Needles penetrating the Knee-Joint.—Dr. J. MASON WARREN related three cases. The first was a child of scrofulous habit, 5 or 6 years old, who, while kneeling on the floor, had a needle penetrate and break off in the knee-joint. It was of large size, larger than the ordinary darning-needle. Dr. W. was called out of town late in the evening to see this child in consultation.

On examination, a small aperture could be distinguished below and to the inside of the patella. The leg was flexed on the thigh, and fixed in that position, so that it could not be extended. Dr. W. thought that the needle had penetrated between the two condyles of the femur, and was fixed there, and that it was broken off in the joint, as nothing could be felt of it exter-

nally. He advised, however, that a dissection should be made as far as the capsule, but to abstain from going further unless it could then be detected.

This was done, but nothing found. The question then arose whether the limb should be left in the position in which it was fixed. Dr. W. advised strong flexion and extension to be made, so that in case the body were lodged in the way he supposed, it might either be dislodged or else plough up for itself a cavity in the cartilage on the head of the tibia. By these measures the motions of the limb were restored. The child was kept perfectly quiet for a few weeks, until all inflammatory symptoms had subsided, after which he walked about without inconvenience.

This patient died of phthisis some years subsequently to the accident, and on examination of the knee-joint, the following was the appearance: The needle, as had been supposed, was firmly lodged between the condyles of the femur; it was somewhat corroded; and, from the motions of the joint, it had worn and maintained for itself a passage, so as not to interfere with flexion or extension. The joint itself was otherwise healthy.

The second case was that of a child 6 years old. In the summer of 1853, a needle, which was sticking in the window-seat, got into the knee-joint. It was immediately withdrawn, and the child, not suffering any pain, was allowed to use the limb. A few days after, severe inflammation came on in the wound, and a fungus shot out. At this period, Dr. W. was called to see the patient. The joint was found to be in a very tender and inflamed condition. On flexion, a quantity of pus ran from the wound, which was surrounded by a fungus of the size of a five-cent piece. Entire rest, with a splint, was enjoined, and the fungus was touched with caustic, causing at each application an increase of inflammatory trouble. After four or five weeks of treatment, the case finally terminated favourably. The needle had entered just on the inner side of the ligament of the patella.

The third instance was observed in a child of 5 years. Kneeling down on the floor in front of a bureau, to get something from underneath it, a needle, which had been engaged in the carpet, ran into the joint, and broke off therein. When seen by Dr. W. with her physician, Dr. Ball, the limb was painful on motion, somewhat swollen, and the child could not walk. By making strong flexion, a hard substance could be felt below the patella, on the inside of the joint, giving to the touch the sensation of some large body like a nail.

The patient being etherized, a dissection was made through the skin and fat until what appeared to be the capsule of the joint was reached. Within this, the foreign substance was felt firmly imbedded. The nails of the two fore-fingers were now pressed against it on each side, causing it to project through the capsule, when it was seized by the forceps. It was now found quite difficult to extract, and this was only done after a number of efforts, and by working it laterally, thus disengaging it from the bone. A splint was directed, with applications of cold water, and care in diet; her physician promising to give information if any symptoms requiring attention should present themselves.

Unusually persistent Hemorrhage.—Dr. STRONG reported the case. J. G., 43 years of age, a carpenter by trade, for several years had used spirits freely, but for three or four past years had been temperate; for at least twenty years has been subject to epistaxis, especially during the summers, as long as warm weather continued; he usually bled twice every day, at noon and at night; the amount of blood lost was often large, and the bleeding was arrested with difficulty. For about the same length of time that this hemorrhage has con-

tinued, the patient has been subject to cough, accompanied by copious expectoration; and, within a year or two, there came on slight, occasional hæmoptysis, which was easily stopped by draughts of salt and water; recovering from these attacks, he would return to his occupation as usual. Within the above-mentioned twenty years, he has had several severe fits of illness, which were called "bilious" attacks; his skin has been more or less yellow for the whole period; for about 12 years he has been dyspeptic; his food often oppressed him; he was an unusually small eater, his favourite food being fresh meat without salt; half a pigeon sufficed him for an entire day. His last illness was of twenty-one weeks' duration, reckoning from his first calling medical aid; but for more than a year previously, he had not his usual health. His last attack was catarrhal, with frequent cough and copious expectoration. After about eight weeks he so far recovered as, on the 7th of December, to go out into the open air for a walk, the midday being sunny and pleasant. Soon after returning from his walk, he began to bleed from the lungs, and Dr. S. was summoned. Before his arrival, however, the hemorrhage had ceased; about a pint of blood, by estimate, having been lost. A somewhat superficial examination of the lungs (Dr. S. fearing to excite a return of the bleeding) detected no signs of a cavity or of any marked disease of the lungs. The most usual symptoms were shifting pains about the chest and shoulders, with a sensation of congestion, more permanent in the præcordial region, attended by stricture, as if a cord were tightly drawn around him; and this latter feeling was at times excessive, especially immediately before an access of bleeding. He could, at such times, hardly raise himself erect, but leaned forward, with an effort to respire. On examination, the liver was found to occupy an unusually large space, and was uncommonly prominent and firm to the touch at the pit of the stomach; tender, and even sore, upon pressure being made. From this time, by his own report, there were twenty recurrences of the hæmoptysis and seventeen of the epistaxis; these usually, but not always, *alternating*. Pulse between 60 and 70 per minute; full and hard previous to the hemorrhages, and did not lose this character, even immediately after them, until within a few days of his death. When about to bleed from the lungs (which he could foretell with great accuracy) he had increased sense of stricture about the base of the chest, with more pain, and that always concentrated in the side of the thorax from which he apprehended the flow of blood. There was usually much febrile action and great heat in both the back and front chest; the bleeding continued until the local and general symptoms subsided; relief being very marked; appetite, lost just previous to hemorrhagic attacks, would in the intervals return. These symptoms and their relief thus alternated without any special difference. At one time, while the patient was complaining of severe pain in the chest, Dr. S., on auscultation over the spot, heard a distinct crepitous rale, which, after the subsequent hemorrhage, had disappeared. Seemingly a transient inflammation was established, which at a certain stage, was uniformly relieved by the natural bleedings. Epistaxis was preceded by pain in the head and nose, with a feeling of fulness and of heat in the nostril from which the blood was about to flow; in these bleedings, the first discharge was of a dark colour, but soon it became florid red, with increased sensation of heat. From the lungs, the first flow of blood was rather dark, but in a short time it was of a bright vermilion; occasionally (and more particularly in the first bleedings), the blood was diffuent, and remained fluid; but in the latter attacks it coagulated firmly. There was but one discharge of blood *per anum*; this he himself referred to the fact of having felt much of it run into his throat during epis-

taxis, and to his swallowing the same. The blood from the lungs was of a bright arterial hue, and, mixed with a frothy mucus, came away after coughing, and did not immediately coagulate. The last recurrences of bleeding were on the 10th inst., when it lasted sixteen hours; on the 12th, about twelve hours; and one a few days subsequently, a small quantity. Death took place on the 23d of February.

The patient undoubtedly bled from some general constitutional cause; the vital force being depressed in connection with, or perhaps wholly dependent upon, disease of the liver and a diffusion of the bile through the circulation.

The treatment was various; an effort was made to relieve the system from the bilious derangement, but unavailingly; astringents in great variety were also employed without the least apparent benefit; strict diet, with a like result; leeches, blisters, issues were all tried without the least good effect. The bleedings would recur with nearly the same intervals, whatever was done. After very copious hemorrhage, the interval was somewhat longer between two attacks. The patient's family estimate the amount of blood lost at more than two gallons; there were about forty recurrences of hemorrhage, and the quantity lost was rarely, if ever, less than a gill at one time, usually being much more. Remedial measures being found of no avail, they were finally abandoned, and a generous diet was allowed. Towards the close of his life, the mind wandered; a comatose state succeeded, and persisted for about two days, when death took place.

Autopsy, five hours after death.—Rigidity slight. At apex of right lung, posteriorly, was a small cavity, as large as a shagbark, containing pus, and in its immediate neighbourhood were a few small, opaque, tuberculous masses. In the same lobe, anteriorly, there was a mass of dense, white, or grayish-white tissue, occupying about the space of a cubic inch, and having very much the appearance of diffused, gray, semi-transparent tubercles. The remainder of this lung, as well as the left lung, was considerably congested, especially in the posterior portions. There were slight adhesions of both lungs in front, and exceedingly firm adhesions of both lungs behind. Two or three ounces of serum in each side of the chest. Heart normal; left side contained considerable liquid blood, which soon coagulated. About four ounces of clear serum in pericardium; no adhesions. Liver large, dense, and firm; studded and filled with small nodules of yellowish-white colour, probably tuberculous. Gall-bladder and bile natural. Spleen quite soft and friable; stomach, kidneys, and other organs healthy. There was some clear serum found in the peritoneal cavity, but no injection of vessels, and no adhesions. The blood which escaped from the heart, as mentioned above, as well as that which escaped in removing the lungs, was at first unusually liquid, but soon coagulated. No appearance of paleness in any organ, as if from loss of blood.

Dislocation of the Humerus—Easy Manual Reduction without the Use of Ether.—Dr. CABOT called the attention of the Society to a method of reduction in cases of dislocation of the head of the humerus into the axilla, viz., by standing above the patient, and fixing the scapula with one hand, or by a foot placed on the shoulder, and pulling the humerus directly upwards; the antagonism of the deltoid muscle is thus avoided, and the bone usually slips instantly into the socket. The case which prompted these remarks was that of a stout farmer, upon whom very violent efforts at reduction had been unsuccessfully made under medical supervision, in the more ordinary manner, even to the extent of excoriating the arm. The dislocation occurred on a Saturday, and was reduced, instantly, by the above method, by Dr. C., on Sunday, at

the Hospital. Dr. Cabot remarked that he had been for some time in the habit of employing this method, and with uniform ease and success, but never before in a case in which other efforts at reduction had been made; he had, consequently, supposed that his cases had been remarkably easy and favourable ones. The result, in the above instance, had induced him to refer to the method employed.

Dr. J. M. WARREN said that he had often tried this method, but had not always been successful; the want of success arising from the great pain caused by the attempt to elevate the arm above the shoulder-joint further than a right angle. In one instance, having been unable to reduce the dislocation by the ordinary plan, with the heel in the axilla, and having sent out for ether, while waiting for it, it had occurred to him to try the plan above referred to. [The patient was made to kneel upon the floor, the operator standing in a chair, and making extension upwards, so as to nearly lift him from the ground. The pain thus produced caused some fainting, and the bone was at once restored to its place. It was certainly a means always to be thought of when the surgeon was without assistance.]

Dr. PARKMAN asked if such dislocations were not *always* easily reducible by means of etherization. He had thus found them in his own practice. [This was acknowledged; but it was remarked that ether, not being always at command, the surgeon usually attempts reduction without waiting for it to be brought; and if any one mode be easier, or more likely of success than others, it is desirable to know it.]

Dr. COALE referred to a case which he had treated, and in which much force had previously been unavailingly used to reduce a displaced humerus; the mode advocated by Dr. Cabot, and which was first advised by Mr. CHARLES WHITE, of Manchester, England, about the middle of the last century, was wholly successful, and very easily so. [The patient, in Dr. Coale's case, dying thirty days after, an opportunity was afforded for examination of the joint. For a full description of this interesting case, see *Catalogue of the Cabinet*, Specimen 172, p. 39. Mr. White's method has been revived by M. Malgaigne and Mr. Syme; the patient is, by them, placed in a *recumbent* posture; the surgeon sits behind him (for good representations of the positions of surgeon and patient, see Dr. Cruikshank's *Vide Mecum* and Sir A. Cooper's *Fractures and Dislocations*); the scapula being firmly fixed, "the arm is raised from the side and drawn straight up by the head, till the bone is thus elevated into its socket." (Dr. Cruikshank.) Mr. Fergusson (*Practical Surgery*) points out the modifications of Mr. White's method, as practised by Malgaigne and Syme. Mr. White suspended the patient, by the injured arm, from the ceiling.—SECRETARY.]

Use of Lemon Juice, and of other Remedies, in Rheumatism.—Dr. PUTNAM asked if any of the members of the Society had found *lemon-juice* to be of any service in rheumatic affections. He had observed some beneficial effect, as he believed, in the case of a child, but had not often tried it.

Dr. STORER mentioned the case of a woman, with rheumatism, at the Massachusetts General Hospital, who took, at one period, fourteen tablespoonfuls of the juice of lemons, daily, and for several days; there was no purging, and no abdominal pain; little, if any marked effect upon the disease; in many instances, no effect at all is observed; in some, it has been thought noticeable.

Dr. J. B. S. JACKSON referred to two or three cases. No effect.

Dr. C. E. WARE gave to one patient one-half a tumbler of lemon-juice, daily, for ten days. No effect; no inconvenience.

Dr. SHATTUCK had observed decided benefit from the use of lemon-juice in

three or four cases of rheumatism. One patient, who had had two attacks, found great advantage in taking the juice; colchicum was, in his case, of far less avail. In certain cases, Dr. S. had seen no effect whatever from the remedy.

Dr. BOWDITCH had observed no effect from the lemon-juice. Whatever be the remedy, if emesis and catharsis be excited, relief is generally afforded.

Dr. J. B. S. JACKSON spoke of the well-known uncertainty of remedies in this disease.

Dr. COALE referred to the bad quality of much of the colchicum on sale, and to its variable strength.

Dr. BETHUNE, when affected with rheumatism, had found he could take no more than fifteen or twenty drops of the wine of colchicum without purging being induced, and which always aggravated the disease, which he considered, in his own case, to be subacute. Dr. B. said that a more accurate and discriminating classification of rheumatic cases is desirable; some cases yield readily to colchicum; others do not, in the least. He had found quinia of service.

Dr. STRONG believed that, to do any good by purgation, the latter must be very thorough, and to the extent of *fully clearing* the bowels, not merely irritating their mucous membrane.

Dr. PUTNAM agreed with Dr. Strong as to the efficacy of purgation in rheumatism, and recommended the combination of rhubarb with the colchicum; the action of the latter is better and more thorough. [At the next subsequent meeting, Dr. Putnam mentioned a case in which, under the use of purgatives and colchicum combined, violent vomiting and purging were induced, and the disease was nearly subdued in twenty-four hours. Sulphate of quinia was given, in the dose of from 15 to 20 grains, daily, for four days, with marked benefit. Dr. WATSON mentions the sudden and complete yielding of the disease when powerful purgation and emesis are caused by colchicum. (*Practice of Medicine.*) He adds that, unless the affection disappear after such violent action on the alimentary canal, it will be useless to push the colchicum further.

—SECRETARY.]

Dislocation of the Thumb forwards.—Reported by Dr. MINOT.

A lad, in running, fell, and was found to have dislocated the left thumb. On examination, the thumb was shortened and strongly extended, so that the second joint was brought near to the metacarpal bone, and projected obliquely upwards. The head of the bone could be felt in the ball of the thumb, in front, and a little to the outside of the metacarpal bone; the bones of the thumb were, consequently, a little oblique, compared with the direction of the metacarpal bone. The second phalanx of the thumb was flexed upon the first.

The patient having been etherized, reduction was easily effected by employing extension, with a slight rotary motion.

Dr. Minot was not aware that this form of dislocation was considered rare until he met with a statement by Nélaton (*Eléments de Pathologie Chirurgicale*, t. ii. p. 423), that but three examples of it are on record, to which M. Nélaton adds a fourth.

Opiates in Peritonitis and in other Inflammatory Affections.—Dr. J. B. S. JACKSON met with a physician, while travelling, who mentioned having had great success in the treatment of peritonitis by opium, freely used. Dr. J. said that he was inclined to believe such a treatment would be very efficacious

in many inflammatory cases; he instanced pleurisy and rheumatism, and, moreover, would have great confidence in such a treatment of *non-serous*, as well as of *serous* inflammations.

Dr. HOMANS spoke of the beneficial action on the skin produced by opium in many cases; in abdominal tenderness, with diarrhoea, small, often repeated doses are very serviceable.

Dr. STORER thought the free use of opium in cases such as Dr. Jackson had mentioned, would have a tendency to constrict and dry the skin.

Dr. C. E. WARE relies upon opium in pneumonia. He thinks that, when largely given, it does not constrict the skin, but induces diaphoresis as readily and fully as does Dover's powder. He has not bled for many years in pneumonia.

Dr. STRONG often gives opium to the point of incipient narcotism in acutely inflammatory cases. He gives the pure opium as well as Dover's powder.

Double, Self-adjusting Stethoscope.—Dr. BOWDITCH introduced to the notice of the Society a new stethoscope recently invented by Dr. Camman, of New York city. Dr. B. said that heretofore he had no belief in any one form of stethoscope being much better than another. Dr. Williams, of London, had long since suggested a form, constructed on scientific principles, which, in Dr. B.'s opinion, was a *shade* better than any other, until Dr. Camman's was presented to him. Between the value of Dr. Williams's and of Dr. Camman's no comparison could be made. The instrument constructed under the direction of the latter gentleman, *intensifies*, to an extraordinary degree, every sound heard in auscultation. For instance: the puerile respiration of a child seems almost like the rushing of a whirlwind. One scarcely believes his own senses at the first inspiratory act that is heard. In cases where the healthy respiratory murmur is very quiet, perhaps scarcely recognizable by the naked ear, or when examined with Dr. Williams's stethoscope, it becomes quite manifest while using Dr. Camman's. So with morbid sounds. Dr. B., although he had very recently received the new instrument, and was consequently but little accustomed to its use, had been able to discover râles and rubbing sound, only recognizable after the closest attention by the ear alone, and that too after their presence had been ascertained by means of Dr. Camman's apparatus. The reasons for this intensity of sound appeared to be chiefly two: 1. Both ears of the observer are acted upon at once. 2. The ear-pieces of the instrument, fitting tightly into the meatus of both ears, all *external* sounds are more thoroughly cut off, and the mind of the auscultator is thus forcibly drawn to the phenomena taking place within the thorax. By this instrument, moreover, the much mooted question, whether the column of air, or the material composing the stethoscope, conveys the sound, is decided in favour of the column of air. The proof of this consists in the following facts: 1. Dr. C.'s stethoscope consists of no less than *five* different media, viz. ebony, an elastic tube lined with metallic wire, metallic tubes, and finally, ivory ear-tubes. According to all acoustic theories, such a variety of media should not transmit sound as well as a more homogeneous substance. 2. Dr. Camman, by experiment, found that when the base of his stethoscope (*i. e.* the bell-shaped portion that rests upon the chest) was made solid, *all* sound was lost. Dr. B. concluded by recommending the new instrument to the candid examination of the members of the Society.

Tumour of the Breast—Dr. CABOT.—Mrs. M., 40 years of age, first noticed a hardness and swelling in the left breast about five months since; it increased

rapidly; at first with pain, but latterly there has been none. Dr. Cabot removed the breast on the 11th inst.

On the 14th, there were cough and some streaks of blood in the sputa.
R. Pulv. ipecac., Pulv. opii, āā gr. ss.

17th. Some suppuration under flaps of wound.

19th. All sutures came away.

20th. Discharge from axillary corner of wound is quite considerable.

March 1. Dress with spirit and water; patient may sit up.

3d. Granulations flabby, dress with resin cerate.

7th. Granulations more healthy.

9th. Discharged, nearly well.

Microscopic Examination of the Tumour. By Dr. JOHN BACON, Jr.—Sections from different parts of the tumour exhibit under the microscope a dense cellular tissue, with a small proportion of cells and free nuclei; having the characters of cancer. In some parts small granular cells, which may be epithelium or secretory cells from portions of the mammary gland not invaded by the disease; oil-globules and molecular granulations occur as usual.

Tumour of the Upper Maxillary Bone—Excision.—Dr. J. MASON WARREN exhibited a specimen in which the whole maxillary sinus was occupied by a tumour of fibro-plastic appearance. Towards the angle of the jaw the tumour had made its way out of the sinus by an opening, and insinuated itself under the zygomatic process, being firmly attached to the external part of the jaw at that point. The history of the case is as follows:—

The patient is 21 years old, and has just graduated from college. Many years ago he had the last molar tooth in the left upper jaw extracted. Nine months since, after a fortnight's pain in the face, a small tumour appeared at the edge of the jaw, whence the tooth had been removed, which ulcerated and extended. In July, an operation was performed to remove the posterior edge of the jaw, with the disease, but the wound never healed. Lately, the front part of the maxillary sinus had been projected forwards, making quite a prominence on the cheek. The eye was slightly protruded, and the sight somewhat impaired. Within two months, a large ulceration, or perhaps it ought rather to be called a tumour, occupied the posterior part or angle of the upper jaw; its edges projecting so much as to interfere with complete closure of the jaws, and requiring to be occasionally trimmed off, which was done by the patient himself, but not without some bleeding. The discharge from the tumour was not very copious; hemorrhage took place from it from time to time. The finger being carried into the mouth, encountered behind the ulceration a rounded tumour, lying close upon the coronoid process of the lower jaw, and leading to the supposition that the tumour had made its way out behind, from the maxillary cavity. The microscopical examination was of a doubtful character, but rather tending to establish the malignancy of the growth.

The patient being informed of the probable nature of the disease, and that it might have already made its way out of the maxillary sinus into the cheek, decided, as his case otherwise appeared hopeless, to have the operation performed, which was done Jan. 28, in the following manner, etherization having been first employed.

An incision was made from midway between the orbit and the ear to the angle of the mouth, with the concavity backwards, the better to expose the tumour, which tended in that direction. The flaps were rapidly dissected up, the large vessels tied, and a freezing mixture applied to the surface so as to stop the bleeding from the small vessels; the ether being used at the same

time, and the patient again brought fully under its influence. The first incisor tooth was extracted, and the soft palate, or rather mucous membrane covering the hard palate, cut across, where the superior maxillary joins with the palate bone. The bones were now quickly divided; first, the zygoma; next, the external angular process of the malar bone; third, the nasal process of the superior maxillary; and, finally, the junction of the two maxillaries, by the cutting forceps, one blade of which was introduced into the mouth, the other into the nostrils. The whole bone was now seized with strong hooked forceps and slightly depressed, so as to expose the superior maxillary nerve in the bottom of the orbit, which was divided by the scissors; and, by a few more strokes of the knife, the whole mass was removed. Three or four vessels were tied, but the hemorrhage was not great.

An examination of the tumour showed it to be of a fibro-plastic character. It occupied the whole maxillary cavity, had made its way out behind, and turned up on its posterior wall under the zygoma; but it adhered so firmly to the maxillary bone as to come out with it enveloped in a cyst.

The edges of the wound were approximated by sutures, and cold-water dressings applied. In a month the patient had so far recovered as to go home. Double vision was experienced for some days after the operation, but the control of this faculty was soon regained. The loss of sensibility in the integuments, consequent on division of the nerves, was partially restored, and is constantly improving.

February 27. Exfoliation from the right Lower Jaw after Extraction of a Molar Tooth.—Dr. MORLAND showed a piece of the lower jaw, an inch and three quarters in length, by three quarters of an inch in breadth at the widest part, and which comprised several entire alveoli. The patient from whom the specimen was taken is a very tall man, of strong constitution, 45 years of age. A decayed and ulcerated molar tooth was extracted from the right lower jaw after very violent efforts, by a dentist, on the 15th of December, 1853; the crown being broken off, after using two instruments, and the roots pried out by main force afterwards. Pain and swelling, which had, to some extent, previously existed, continued and became aggravated after the above operation, and extended to the neck. The patient, finding two sound teeth loose in the vicinity of the former, took them out himself easily; and, on the 2d of January, perceiving a portion of bone loose in the site of the extracted tooth, he used a penknife as a lever, and turned out the specimen shown. Pain and swelling somewhat abated afterwards, but a numbness "of the lip and of three front teeth," as the patient describes it, followed—with shooting, neuralgic pains, particularly when the head was inclined to the right side. An aggravation of the pain and swelling occurred January 24, the latter increasing until it extended from the eye to the collar-bone, accompanied by soreness of the throat, loss of power of articulation, and great difficulty in opening the mouth. This state of things was relieved after profuse discharge of pus by the mouth, seven or eight days from the recrudescence of the trouble. Shortly after, pus in large quantity was evacuated externally from just beneath the right lower jaw. Convalescence was gradual after this; the patient has been, for a period of nearly three months, almost wholly incapacitated from attending to his business, and for nearly that time has suffered pain and excessive inconvenience in deglutition, articulation, &c. His countenance has a pale, sallow hue, and plainly indicates that his system has felt the effects of the injury very severely. Several smaller pieces of bone came away during the above period. Dr. M., considering the amount of injury

and disturbance unusual from so common an operation (although much violence was used), inquired particularly as to the patient's occupation, and whether there were anything to which he was exposed likely to affect the bones. He is occupied in a chemical laboratory; he states that he has never been exposed to the fumes of phosphorus in a free state; has had much to do with the various *phosphates*; has dealt largely in muriatic acid, but is unaware of any ill effects therefrom; he is quite confident, however, that the being so much in an atmosphere of hydrochloric acid vapour has caused excessive *leanness* in his case, and believes it always does so; patient has manufactured iodine, largely; has often been troubled by its irritant effects on the air-passages—not otherwise, that he is aware of; has handled pyroligneous acid in large quantity.

May 1, 1854.—Patient reports that the numbness (termed by himself "*paralysis*," and consequently it must be a *marked* sense of benumbing) of the lip and chin still continues, and that there is also pain in the jaw, at times severe; the whiskers refuse to grow upon the right side, over the seat of injury, which, as the patient remarks, gives him a rather "sinister look."

Cancerous Disease of the Rectum and Vagina.—Dr. MINOT reported the case. Mrs. G——, æt. 48, laundress, widow, having borne four children, had always enjoyed good health until June, 1853, when she began to have, without known cause, frequent, small, bloody, painful dejections, accompanied by much tenesmus and dysuria. She had perceived a tumor in the abdomen for some months previously.

On the 20th October, when she applied for relief, she was pale, sallow, and feeble, with a clean tongue and a pulse of 94. She had been losing, according to her own statement, half a teacupful of blood by stool daily. The discharges from the bowels were scanty and clay-coloured, and there was a frequent desire to pass urine, and difficulty in voiding it. On examination *per rectum*, the caliber of the intestine was almost blocked up by a large, smooth, irregular mass, apparently covered by mucous membrane. *Per vaginam*, a similar state of things existed, the canal being obstructed by smooth, irregular masses of cartilaginous hardness, which projected into its caliber. The os uteri could not be felt with certainty. The diseased mass was not movable, and gave no pain when pressed by the finger. On palpation of the abdomen, a smooth, hard, round tumour of the size of an orange was felt in the left iliac fossa. It was movable, and shifted its position with the motions of the patient.

Until her death, which took place Feb. 19, 1854, Mrs. G. had a constant, profuse, offensive discharge from the vagina, which was generally dirty-coloured, sometimes bloody. There was also frequent desire to empty the bladder, and difficulty in doing so. She had little or no pain except during her last few days, though she occasionally complained of much uneasiness or distress in the abdomen. The bowels were costive early in December, the discharges from the bowels began to come *per vaginam*, and, on examination, a large communication was found between the vagina and rectum. She menstruated regularly, her last period beginning January 28. During the last fortnight she vomited everything taken into the stomach, though nothing was found in the appearance of the organ after death to account for this symptom beyond injection of the cardiac extremity, and an extremely corrugated state of the mucous membrane. The liver was large, fawn-coloured, fatty, and contained much fluid blood. The gall-bladder was greatly distended with bile.

Dr. J. B. S. JACKSON, who examined the diseased pelvic viscera, gave the following account of the appearances :—

On dissection, there was found deep ulceration of the rectum, and of a well-marked malignant character, commencing about an inch from the anus, extending upwards two inches or more, and involving the whole circumference of the intestine. The cellular tissue was affected so that the parts were very firmly bound down to the sacrum. The disease extended also to the upper part of the vagina, and there was a direct opening into it from the rectum, effected by the process of ulceration, and sufficiently large to admit two fingers. The os uteri was somewhat ulcerated, but the uterus itself was not much diseased. The whole posterior half of the bladder, however, was very much thickened and indurated, having generally a scirrhus appearance, without any encephaloid deposit, which last was quite marked in some parts of the rectum; the inner surface was red, roughened, and evidently inflamed, though nowhere ulcerated; otherwise, the bladder was perfectly healthy.

Tumour connected with the Cerebellum.—Dr. J. B. S. JACKSON reported the case, of which he had lately made the dissection. The mass was about the size of an English walnut, perfectly defined, firm to the feel, partly made up of three or four small cysts, and partly of a solid whitish substance. Externally, it was somewhat lobulated, traversed superficially by large thin vessels; and the whole appearance, externally and internally, was such as to suggest the idea of malignancy, though nothing was discovered by the microscope to favour it. The tumour was connected with the under surface of the right lobe of the cerebellum, and appeared to have been formed in the pia mater. There was a very large serous effusion in the lateral ventricles, as usual in such cases; and the fifth ventricle was observed to be particularly dilated. The brain itself was healthy. The patient was a middle-aged man, and by profession a clergyman. Deafness in the right ear was observed two years ago; and the tumour may then have been of some size, though no other symptoms occurred until the following autumn. Then there came on occasional paroxysms of headache and vomiting; and this last was a very marked symptom throughout most of the disease. Last summer the gait became unsteady, and there was towards the last much loss of power, though it was never complete. The intellect also became dull, and since last October, when he gave up his professional duties, he was mostly, if not entirely, confined to the house.

Dr. SARGENT, of Worcester, who was present at the meeting of which the above report was made, spoke of a patient whom he had attended, a lady, 52 years of age, who had loss of memory, some vertigo, slowness of speech with miscalling of words, opisthotonos, and, finally, paraplegia and complete dementia. Dr. S. supposed that he should find, at the *post-mortem* examination, softening of the brain and some affection of the spinal marrow. The patient was ill for from six to eight weeks; on necroscopic inspection, Dr. S. found the substance of the cerebrum the *hardest* he had ever seen, and the spinal marrow was perfectly healthy.

Dr. S. remarked further of this woman, that, although she was only 52 years of age, and had scarcely ceased to menstruate, she had the whole appearance of extreme old age. She had also recently experienced a great deal of domestic trouble.

Ulceration of the Knee-Joint—Amputation.—Dr. CABOT showed the parts removed, and gave a history of the patient, a girl, 16 years of age, who entered the Hospital, April 26, 1853, with disease of the knee-joint of four years' duration. She first observed slight enlargement of the right knee without

known cause; it has never incapacitated her for working or for walking; there has never been pain in the joint except when she was overworked. At the time of her entry at the Hospital, the motion of the joint was limited; the head of the tibia much enlarged; circumference of the joint at upper part of patella fourteen and a half inches, the same at the middle, fourteen inches at the lower part. Blisters, tincture of iodine, and water-dressings were applied until the 29th of May, when there were indications of abscess forming in the popliteal space; swelling, with fluctuation, &c. Poultice.

June 11. Abscess opened last night, and is now discharging pus and clots of blood. Poultice continued.

July 6. Abscess injected with tincture of iodine; a knee-cap was used to support the joint and prevent motion; liniments; issues over the condyles of the femur; tonics internally, &c.

Nov. 20. Patient etherized and the sinus examined; it was found to extend around to the front side of the knee, where there was a large cavity; an incision was made over the site of said cavity and pus was discharged. Flaxseed poultice; much pain; opiates were applied locally and given internally, also stimulants.

23d. Abscess beneath knee-joint opened. Diarrhœa supervening on the 29th, was met by a tonic and astringent combination. Cod-liver oil was afterwards administered.

February 18, 1854. The limb was amputated by Dr. Cabot.

Dr. J. M. WARREN, referring to this case, spoke of its earlier aspect, &c.; there was, at first, only a small bunch upon the knee, which, when opened, discharged serum; an abscess, as above described, subsequently formed; the diagnosis was at first difficult; no dead bone was detected; the muscles were found, on removal of the limb, in a state of fatty degeneration.

Dr. J. B. S. JACKSON spoke of the soft, fleshy, membranous, red formation sometimes observed on denuded bones; he asked if this were not often described as a pulpy degeneration of the synovial or investing membrane? May it not, rather, be frequently only the granulations of inflamed bone?

March 13. *Intermittent Headache.*—Dr. C. E. WARE reported two cases of this affection, relieved by opium. The first was in a young woman who was suffering from amenorrhœa following typhoid fever. The headache had continued very severe for more than a fortnight, during which time she had tried quinia, iron, and arsenic in full doses. She had been leeches and blistered; nothing seemed to make the slightest impression upon the disease. The headache commenced in the morning and continued till evening.

She began to take opium in two grain doses the sixteenth day of the disease. Soon after, she became easy, and then continued the pills through the day often enough to keep her fully under the influence of the opium, and to insure freedom from pain. She was obliged to continue the pills more or less frequently for two or three days after. She never, however, after the first pill, had any serious return of the headache. The irregularity in her catamenia continued. She experienced no unpleasant effect from the opium.

In the other case, the affection occurred in an old lady, 87 years of age. She was a vigorous woman for her years, and was well, except for the headache. She tried for several days quinia and iron, without the least benefit. She took opium in similar doses to those of the first case; was relieved by the first pill, but was obliged to keep under the influence of the opium for a day or two. She had no serious return of the headache.

¹ *Tuberculosis, etc.*—Dr. BOWDITCH was called, *February 26, 1854*, to see B. M., male, *æt.* five months. Some months since, the boy fell from his cradle and struck upon his head. No serious trouble remarked, and no *manifest* connection of the fall with present disease. For two months previous to present illness, he had had a pustular eruption on various parts of the body, and had “drooled” much, as if troubled by dentition. Both of these affections disappeared on the occurrence of the present attack. For a fortnight before Dr. B. saw him he had not been quite well, as his mother thought, although he had had no evident illness, except that, about the middle of that time, the child, while nursing, seemed suddenly distressed; at these accessions, he would throw his head backward, and seemed faint and gasping, with the eyes rolled up. These lasted only a moment, and immediately he would recover and seem as well as before. They occurred, however, only a few times, and, as no other symptom appeared, little was thought of them except by the anxious mother. On 22d or 23d, he occasionally moaned as if in pain, and a slight torticollis to the right side was noticed. When Dr. B. saw him, he had then turn of the neck permanently, but in a slight degree. No swelling, tenderness, or redness apparent, but the child seemed to suffer when any manipulations were made on the neck. Little appetite, no vomiting. Respiration regular, but with a constant moan. Pulse not slow; skin of moderate temperature. Calomel gr. i. ordered; and Dover’s powder $\frac{1}{4}$ gr. at night and morning.

27th. Restless night. At visit, sleeping quietly. Pulse 130. In addition to the Dover’s powder, 10 drops of paregoric had been given three times.

28th. Easier night. One powder only taken. At visit, brighter, but had refused to nurse, and had been fed with breast milk which he took with avidity. Very irritable; no strabismus; had had four attacks similar to these above described. Apparently a little tenderness about right ear. Pulse regular, quick; respiration easy; no vomiting. A leech below right ear.

Patient remained in the same state, and next day Dr. Channing saw him in consultation and advised—Potass. hydriodat. gr. iv.; in simple syrup \mathfrak{z} ij; \mathfrak{z} ss to be taken once in six hours. Cold water to the head if much suffering. Paregoric, if severe suffering at any time.

March 3d. Patient has been more quiet. Had frequently moved right arm and rubbed hand upon same side of the head and right eye; the accessions of increased suffering less severe, at times low moaning; neck less stiff and right limbs which had at times been restless, were less so. Right fist firmly contracted; thumb bent inward on the palm. Frequent gaping. No real vomiting, but some retching; eructations of flatus after food; no dejection. Urine, in quantity rather more, and less ammoniacal in smell than it had previously been. Pulse rapid and small.

On 2d, Dover’s powder and calomel had been ordered occasionally. Continue them and iodide of potassium as directed. Ol. ricin. \mathfrak{z} j.

The disease steadily advanced. Night of 4th, quiet. 5th. Restless under a domestic remedy and without opiates; subsequently, not very uncomfortable nights till 10th, when he died in convulsions. Previously, he was quite irritable at times, and screaming as if in pain. On the 4th, the eyes tended to the left. There was frequent motion of the right extremities, a partial loss of power in the left ones. The pupils, which were at one time contracted, on 4th were larger. On 7th, strong strabismus. On 9th, deep stupor. On 4th, the breathing was deep, rather slow, and uneven. Occasionally a slight hack, but no severe cough was heard at any time. No other rational, and no distinctly morbid physical, signs referable to the lungs. Pulse on 4th was 128, on 7th, 100. Urine was augmented on 6th. Some tendency to occasional

retching, and a day or so before death vomited a few times. During this period, the same medical treatment was continued except on 6th; 1 gr. of potass. nitrat. was ordered at intervals.

On 10th, the report was, that, after lying for the previous thirty-six hours in more or less stupor, accompanied by evident prominence of the anterior fontanelle and constant strabismus, but with a less firmly contracted fist, he died in convulsions. At 6 A. M. of 10th, *autopsy*, twenty-six hours after death; meninges of the brain very much distended, owing to a large quantity of thin, rather turbid, fluid in ventricles, which, under the microscope, contained pus and exudation corpuscles, and apparently detritus of the cerebral substance. Numerous minute (tubercular) granulations in the anterior commissure, and over the base of the brain. Recent lymph around the optic nerves. The interior of the left hemisphere was broken down by the scalpel throughout a large part of its extent, and a soft white detritus, save in one part, involved the corpus striatum and optic thalamus, and wall of the ventricle. Here the cut surface had a hard, tubercular aspect. Bronchial glands, tuberculous; minute semitransparent granulations were found filling the lungs. They were likewise numerous in the liver, less so in the spleen; a few in the kidneys and intestines; and, finally, two or three, unequivocal in their character, were found on the left ventricle of the heart, just under the pericardium. The voluntary muscles, so far as examined, contained none.

The very insidious nature of the attack, the torticollis, and the universal distribution of the tubercles, Dr. B. regarded as the peculiar points in the case.

Blighted Ovum.—Dr. W. E. TOWNSEND exhibited a blighted ovum of about three months' growth. This was cut open. The umbilical vesicle was very distinctly seen, and the walls of the structure were very thick. Dr. T. reported the following history of the case: "Was called on Wednesday last to see a lady, who stated that she was that day taken with a slight flowing, unaccompanied by pain; that she supposed herself to be between three and four months pregnant; that on Saturday night, two and a half weeks previously, she had slipped down two or three steps at the Fitchburg Railroad station-house, and jarred herself severely, so that, whilst riding home in an omnibus, she suffered greatly with pain in her back; the next day, however, she experienced no inconvenience from her fall, but went to church as usual. From that time, the morning sickness and occasional faintness, which had been prominent symptoms of pregnancy with her, disappeared, and she had felt entirely well till the present occurrence. Horizontal posture, and other appropriate remedies, were tried without avail; and, on Sunday afternoon, after suffering severely, she miscarried, three weeks and a day after her accident."

Spleen enlarged; not the Result of Fever.—Dr. JACKSON exhibited the specimen and reported the case, which he thought interesting from the circumstance of so many of the same kind having occurred here; the organ, in such cases, weighing several pounds, and being rather firm to the feel, whilst the patient had never had intermittent fever, and, in several of the cases, had never been in any part of the country where this form of fever prevails. The weight, in this instance, was between four and five pounds, the density was very considerable, and the enlargement had existed for some months at least. The patient was a middle-aged man, and died of a pulmonary affection of short duration.

Iritis terminating in Staphyloma.—Dr. R. W. HOOPER.—Three cases of inflammation of the iris, terminating in staphyloma of the eye, have been seen at the Massachusetts Charitable Eye and Ear Infirmary in the last two years. They were in Irish patients, one male and two females, between the ages of eighteen and twenty-four.

In the first two cases the eyeball was removed on account of the severe suffering attending the disease, and its doubtful character. As these two cases terminated favourably, and after the interval of two years the patients remained well, the operation in the third case was limited to a removal of the anterior half of the eye, to which the disease seemed principally confined.

In these cases, the first departure from the usual appearance was in the iris becoming of a bright yellow colour, and receding from the cornea; the anterior chamber then became turbid, and the sclerotica yielded near the margin of the cornea, and irregular dark gray masses protruded.

Great pain attended this stage of the disease, not relieved by puncturing the globe; and the operation for removing the eye in the first two cases, its anterior half only in the last case, was followed by entire relief. The patients, at the time of the operation, were under the influence of sulphuric ether.

Since the above was written, the last patient died with disease of the brain at the Massachusetts General Hospital. No return of disease of the eye after the operation. An *autopsy* was refused.

Gleet, its Treatment, &c.—Dr. COALE read the following remarks: A paper by Mr. John L. Milton, in the *Medical Times and Gazette* for May, 1853, on the treatment of gleet, has suggested some remarks on this disease. Mr. Milton divides gleet into three kinds: 1. Dependent upon structural changes or incipient stricture. 2. Gleet not dependent on these changes. 3. Gleet arising from disorder of other structures—the prostate gland, testicle, &c. For practical purposes, I would make a different division—retaining, however, the last, and particularizing other gleet, as to whether it be purely a chronic affection, or whether with it there be more recent gonorrhœa. In the third species of gleet—that from affection of other parts of the genito-urinary apparatus—the treatment must evidently be directed to the affected organ rather than to the lining of the urethra. In the other varieties, the difference of treatment I make is this: when more recent gonorrhœa accompanies it, inducing an acute or semi-acute inflammation, the astringent injections, so useful in a purely chronic case, are hurtful. In these, then, I substitute warm water, or warm mucilaginous injections, and am more particular about the diet until all the recent inflammation is wholly subdued; this effected, resort may be had to astringent injections with benefit, and should be continued until every trace of the disease is obliterated. In using the injections, the effect of habit must be remembered, and the active agent of the injection changed as soon as its power seems to decline. Mr. Milton attaches much importance to the use of blisters in gleet, and argues for them as if they were a new remedy, of which the effect might be doubted. I some time since found out their efficacy in this disease, and have used them freely; indeed, they constitute an important part of the treatment. In very severe cases, I use the blistering tissue, applying it to the inside of the thigh, or to the pubis, or even to the root of the penis itself; but in most cases this is unnecessary, and instead, I use the strong tincture of iodine. The patient is directed to paint a strip on the under side of the penis from the root to the glans, bounding one edge of the strip by the median line. This painting is renewed sufficiently often to get up a vesication. When this surface becomes too sore, it is permitted

to heal, and another strip is painted on the other side of the median line. In this way, alternate blisters can be kept up for any length of time. As to internal remedies, the difficulty of getting a patient to take any of the disagreeable mixtures prescribed, long enough to do any good, has occurred to all who have had to deal with the disease. Some tasteless and convenient remedy is wanted; this, I think, I have contrived: Bals. copaiva and pounded cubebs are made into a mass together, and divided into pills. These can be coated with gelatin. They permit the dose to be regulated very conveniently; from three to four, taken four times a day, generally being the amount given.

Popliteal Aneurism—Dr. J. MASON WARREN reported the case of a blacksmith, 30 years old, to whom Dr. W. was called by a distinguished physician in a neighbouring city. In September, 1853, this man perceived a small pulsating tumour in the upper and back part of the left leg. This increased slowly to the size of a hen's egg. The knee was bent, and he was obliged to walk with a crutch. The foot was swollen, its motions partially lost, and it was excessively painful. The patient being informed of the dangers of the operation by ligature of the artery, the possibility of paralysis or sloughing of the limb consequent upon it, and of the alternative of the treatment by compression, decided on the former; as he was out of town, could not well leave his family, and it was impossible to have the compression satisfactorily conducted at home.

The femoral artery was therefore tied at the middle of the thigh, and the pulsation of the tumour was at once arrested. The patient was directed to be kept in bed, artificial warmth to be applied if necessary, and, in case of great reaction—the patient being very muscular—blood to be taken from the arm. For a week or two after the operation, the numbness of the foot was much increased, though the pain was relieved. The ligature separated in sixteen days. The patient was seen by Dr. W. some months after, when he was able to walk, having recovered the use of the limb. A small, hard tumour existed at the place of the former aneurism, having an apparent pulsation, which, however, after repeated examinations, seemed to result, and to be communicated from, an enlarged collateral vessel. He was directed to keep still, avoid animal food, and to do nothing that might excite the arterial action.

By recent reports he is quite well.

Herpes Preputialis; Case and Treatment—Dr. BETHUNE.—A gentleman, from 20 to 25 years of age, presented himself in the early part of February, with this disease. It first appeared after a single exposure last May. He had been under the care of several physicians, who were well-educated men, and had been severely treated. Among other things had had mercury to salivation, and several times had been pronounced cured. The disease has returned five times. The present attack had existed two weeks, and was marked by several vesicles, or rather the remains of them, situated on the inside of the prepuce, near the glans. He was directed simply a saline laxative every third day; lint dipped in a solution of acetate of lead placed between the prepuce and glans, and strict diet. On the week following, the compound rhubarb pill was substituted for the salts, and, soon after, the strength of the lead solution increased. March 12, he appeared quite well, having been two weeks longer than ever before without a return of the disease. A solution of tannin was ordered to replace the lead—and vegetable diet. In the early part of April, he was obliged to fulfil a matrimonial engagement which had been thrice postponed. This was followed by a slight return of the disease.

March 27. Bezoar.—Dr. GOULD exhibited a very beautiful specimen of bezoar, presented to the Society by Mr. Emery Souther, apothecary, of this city. It was taken from the stomach of a deer killed at the mouth of the Mississippi River. *Dimensions:* $2\frac{1}{4}$, $1\frac{1}{4}$, $1\frac{1}{4}$ inches. *Weight:* 2 oz., 2 drs.

The thanks of the Society were voted to Mr. Souther.

Dr. BACON, who had examined the concretion at Dr. Gould's request, stated that it consists of layers of diphosphate of lime, with organic matter, deposited around an angular nucleus which appeared to be a fragment of pottery.

[In the report of Dr. Bacon's analysis of a "factitious bezoar," (Dec. 12, 1853, see No. of this *Journal* for April last, p. 346,) the following should have been added: "The organic matter is very soluble in water, and appears to be a vegetable extract. It contains neither ellagic nor lithofellic acid, and there is no doubt that the supposed bezoar is factitious."]

The following paper was prepared by Dr. W. J. BURNETT, and was read to the Society by Dr. BETHUNE. Remarks relative to the proposed operation are appended:—

Recommendation of an Artificial Cornea as a Substitute for the Transplantation of the Cornea. By JOH. NEP. NUSSBAUM, Assistant Physician in the General Hospital at München.* The object of this article is, as its title indicates, to show the advantages, or rather the full success attending the introduction of an artificial cornea composed of glass, in those cases where there is more or less blindness from opacity of the cornea.

It is not necessary to give a translation of the entire article, and I shall quote only those passages which contain the gist of the subject.

After alluding to the many contingencies for failure attending the bold practice of Himly, of transplantation of the cornea, even by the most skilful operators upon the eye, he says he was led to make experiments relative to what substances would be least offensive as foreign bodies in the healthy tissues; in view of using such for an artificial cornea. After experiments upon his own body, he found that, of many solid substances, glass produced the least irritation, and in some instances scarce any at all. With this fact obtained, he formed a circular cornea of glass, perforated by a hole two-thirds its width. This he introduced into the eyes of dogs, having previously removed a corresponding portion of the cornea. But it was attended with *no* success; for, aside from the extreme difficulty of removing a portion of the cornea *exactly* the size of the artificial body introduced, there was much disturbance following the introduction of so large a body in so delicate a tissue, such as suppuration, &c., with a loss of the eye in the end.

Thus foiled, he says it all at once flashed upon his mind that an orifice, of the size of a pin-hole, is sufficient to admit a good image of an object, if the eye is placed directly near it; as, for instance, in looking through such a small hole in a piece of pasteboard. With this valuable hint, he made a new trial, forming the artificial cornea after a new model, and of a much smaller size. Its general form was much like that of a shirt-stud, there being a main shaft with a rim on each end; but, instead of being round or circular, both shaft and rims were compressed laterally, being, therefore, of an oblong instead of a circular form. The artificial cornea, thus formed and shaped, was not much

* Die Cornea artificialis als Substitut für die Transplantatio Corneæ empfohlen. Von Joh. Nep. Nussbaum. z. Z. Assistenz Arzte im allgemeinen Krankenhause zu München. From Siebold and Kölliker's *Zeitschrift für wissenschaftliche Zoologie*. V. December, 1853, p. 179.

larger than the head of a large pin, and perforated by a hole of an oblong shape and of a correspondingly minute size, as will be seen in the annexed figures.

Fig. 1.



Fig. 2.

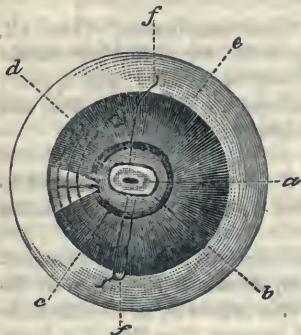


Fig. 1. Glass body, natural size. a. Front view. b. Lateral view.

Fig. 2. Eye of a puppy, several weeks after the operation, with glass body in cornea, magnified $3\frac{1}{2}$ times. a. Glass body. b. White opaque circle around glass. c. Remainder of pupil. d. Iris. e. Sclerotic. f. Bloodvessels on cornea.

With this new model for a cornea, he proceeded to operate upon the eyes of some puppies. Instead of making a circular incision, as in the first experiments, a simple slit only was here required. In this slit of the cornea, the new body was introduced exactly as a shirt-stud is put in a shirt.

The following is his description of the operation:—

“For the operation are required, a cataract knife, a pair of small anatomical forceps, and, for the emergency, Cooper’s scissors. In the absence of a good assistant, there is needed a lid-holder of Kelley Snowden, and when the eye is very restless, a sharp hook. For the patient, I choose the reclining, and for the operator the sitting position. The pupil being dilated by a strong solution of the extract of belladonna, I narcotize the eye until the bulb remains quiet; then, opening the lids by means of a lid-holder, I place the cataract knife, which I hold as a pen, at right angles on the surface of the cornea, at about one-eighth of an inch from its external border, with the knife’s edge directed towards the inner (not the outer) canthus,

whereby both borders of the wound are made of equal thickness. I then plunge the knife into the cornea, until it reaches the anterior chamber; then holding the instrument at a somewhat obtuse angle, I carry it inwards, making an incision one-eighth of an inch in length. The knife is then withdrawn from the wound by carrying it backwards.

“As the wound is small, it gapes open but little, and the aqueous humour flows out very slowly; but, quick as possible, I seize the glass cornea with the pincers, and insert it in the incision, as a button in a button-hole. All this insertion must be done very quickly, for upon the time occupied depends the reaction and disturbance which are to follow. In conclusion, I remove the lid-holder, and glue up both eyes.

“The quantity of aqueous humor that escapes during the operation, is in exact ratio with the disturbance and trouble that follow. When little escapes, the iris is little irritated, and the lens but slightly disturbed. In some instances, I was fortunate enough to lose only two drops of the aqueous humour; these cases healed very quickly, and I was convinced that neither iris nor lens had been at all disturbed. In those cases where the incision was too large, and did not hold the glass, the operation proved a failure, and I sealed up the eye, allowed the wound to heal, and afterwards operated again with better success.”

I need not here give the after-treatment, as laid down by the author. It must vary, of course, according to the patient, and the care with which the operation is performed, and will suggest itself, *pro re nata*, to every intelligent oculist.

In regard to some of the sequelæ, he says: “In all cases, there appeared, on the first day, a universal conjunctivitis, and a ceratitis, with some disturb-

ance of the cornea; in several cases, an onyx. The former disappear quickly; and the abscess of the cornea heals usually in eight to fourteen days, when the glass, inclosed in an exudation, ceases to be objectionable to the cornea. Iritis I have observed only when the operation was so conducted that much aqueous humour was lost and the lens impinged upon.

"In regard to the appearances in general, my patients seemed as free from pain after the eighth day as before, and the general aspect of the cornea appeared much less disturbed than in those cases where a portion of the cornea was removed (as in transplantation of the cornea). The appearance of the eye is not particularly bad; around the glass there is a small, white, opaque circle, to which extend, from the border of the cornea, one or two small bloodvessels. The eye has no irritability, and no photophobia even to glaring light."

As to the intimate changes which ensue in the tissue of the cornea, from this operation, the author says: "The sections which I have made during the various stages of healing, presented anatomical changes corresponding to the different sequelæ. The perfectly healed cornea I have often observed microscopically. I found the fibres in the vicinity of the glass always more or less troubled, and slightly lengthened; close to the glass, they presented a wave-like aspect, and their usual parallelism was wholly wanting."

Such is a brief abstract of Nussbaum's account of his new operation. As yet, his experiments have been only upon the lower animals, and the success he has had with dogs makes him confident of a like result in man.

The artificial cornea must, of course, be made with great care, and its size and various proportions varied according to the eye to receive it. The author recommends the rock-crystal as the best material out of which this body is to be formed.

These experiments, from their success, have excited no little attention in the locality of their occurrence, and Von Siebold, a name too well known in science to require mention here, and one of the editors of the journal in which this article was published, carefully examined some of the dogs operated upon. He was surprised to find so little disturbance from the foreign body. Indeed, he says, the animal suffered no inconvenience, the secretions of the eye were not diminished or increased, and the animal winked, or otherwise used the organ, as in the natural state. He considered the subject of sufficient importance to have a lithographic plate made of one of these animals, with details of the parts. At all events, the subject is not lacking in ingenuity; and it having been shown that a perforated glass body, of small size, can be introduced in the cornea, and there remain harmlessly—this, certainly, is an important fact learned; but the application of this operation upon man can alone determine the visual relations of this body, and the greater or less degree which its orifice can transmit rays of light for the formation of an image on the retina. Certain it is, however, that the optical principle here adopted is sound in theory.

Dr. BETHUNE, though he agreed with Dr. Burnett in the opinion he expressed of the great ingenuity shown in this form of artificial cornea, yet thought its practical utility extremely doubtful. The aperture which forms the pupil is made so small (probably to retain the aqueous humour by capillary attraction) that a very perfect adjustment must be made and retained, or vision will be prevented. So small an opening would be liable to become clogged either with foreign substances, or with the secretions of the eye itself, and in getting rid of this obstruction by passing it into the anterior chamber, there would be danger of exciting destructive irritation of the organ. After much difficulty, Mr. Griffiths, a glassblower of Roxbury, has succeeded in making for Dr.

Bethune a very perfect and beautiful glass cornea, and he will test it, if a suitable case presents itself.

Dr. DURKEE considered the operation described in the communication from Dr. Burnett as a specimen of very ingenious ophthalmic surgery. He had, however, great doubts as to its practicability on the human subject. The operation might *possibly* be successful, considered merely in a surgical point of view, and yet be an entirely useless one to the patient. It seemed to him that the laws of optics, in regard to the refraction of the rays of light in its passage through the cornea, the aqueous humour, the crystalline lens, and the vitreous humour, each having different powers of refraction, and each having a different density and different shape from the other, would be disturbed in a manner that would prevent the formation of a distinct image of objects on the retina. Dr. D. was not, therefore, inclined to coincide with the opinion of Dr. Burnett, as expressed in the closing sentence of his manuscript. The rays of light, it seems, are to be admitted through a very minute aperture. This aperture will act in the same way as a small perforation does through a card. And how is this? The rays must cross each other in their transit through this opening. They must, consequently, enter the aqueous humour greatly divergent; and thus the very first step in the series of refractions is reversed. How is it possible for these divergent rays to be changed in their direction, so as to pass through the crystalline lens in the normal manner? The rays of light, as they pass from the cornea through the aqueous humour in the sound eye, are rendered more convergent than while passing through the cornea. This increase in their convergence through the aqueous fluid is necessary in order that the rays may reach the lens in a proper manner. But Dr. Durkee could not understand how it was possible for this order of refractions to be secured after the surgical operation in question had been performed. For the rays traverse no cornea, and the shape of the aqueous humour must be altered, and the light be confused. The different chambers may be illuminated, to a greater or less degree, through the small opening in the glass stud; but that a distinct image could be spread out upon the expanded surface of the retina, appeared to him a matter of great doubt. In the account furnished by Dr. Burnett, it is not stated that the animals operated upon were enabled to see. Ophthalmic surgery has accomplished wonders, it is true; but in this instance the principles of optics will be so much disturbed that the intended result of the operation will almost certainly be defeated. Such, at least, is Dr. D.'s opinion.

Dr. D. thought the chances for distinct vision would be greater, if the glass were solid instead of being tubular. The glass would then be a substitute for the portion of cornea that had been removed by the operation. It would present an unchangeable uniformity for the admission and transit of the rays of light. But it is scarcely to be supposed that the aqueous fluid occupying the tube—and there being no way for confining this fluid, or for regulating its shape, or for preventing the constant movement of its particles—can possibly preserve any fixed form. It will be in continual agitation except during sleep. If we attempt to look at an object in water while the water is in a state of agitation, we find that vision is disturbed; we see the object in broken fragments and assuming all manner of distortions; and in the case before us, the aqueous humour will be constantly oozing out through the opening in the glass, and irrigating the surface of the eye. How is it possible, under such circumstances, for the rays of light to be transmitted towards the crystalline lens in the proper, regular order, without which there can be

no correct vision? The practicability of the operation, whether considered as a matter of surgery, or in its relations to the principles of optics, remains to be proved. Dr. D. demonstrated his ideas upon this subject by drawings upon a blackboard.

Dr. BETHUNE exhibited to the Society specimens of the glass cornea, which with great trouble he had procured. The same workman will doubtless now be able to furnish others, after this model, with greater ease, and promptly, should there be found a use for them.

Dr. WILLIAMS made the following remarks relative to the operation :—

The *Union Médicale*, of Paris, in some of its numbers for January, 1854, alludes to the operation proposed by Dr. Nussbaum. The proposition is condemned by the editor in strong terms, and characterized as “worthy of a German brain.” A subsequent number contains two letters—one from a German physician, who repels the imputation against his countrymen, as entirely too sweeping, but coincides with the editor in his estimate of the value of the experiments made upon animals, and deprecates their repetition upon the human eye—the other letter is from Dr. Déval, of Paris, of some authority in ophthalmic surgery; and contains proof that the plan proposed by Dr. Nussbaum has in it nothing of novelty, as not only had the insertion of an artificial cornea been proposed and practised upon animals, but the artificial substitute had been made of similar form to that now advocated. Dr. Nussbaum, he shows, has merely revived an operative procedure which had become obsolete. He refers to the discussions to which the subject had given rise in Germany, and states that the experiments on the human eye have been condemned by Dr. Pauli, of Landau, and other distinguished authorities; and further, that Dr. Nussbaum had recently performed his operation on a patient, with no other result than a total failure.

[MALGAIGNE writes as follows in his *Médecine Opératoire*, p. 406 (4th ed. 1843): “Pellier was the first who proposed excision of the natural cornea, and the fitting of a glass cornea within its circumference. This extraordinary (*bizarre*) idea has never yet been applied to the human eye, and doubtless never will; it has not even been tried upon animals.” It has only been shown that the glass stud can be inserted and worn in the eye of an animal, and not that the animal could see therewith. The eye of such an animal, previous to the operation, is supposed to be in a healthy state; the human eye must be, or have been, in a diseased and abnormal state, to require any such attempt at giving a new cornea;—would not this fact influence the result? If even it be proved that the animals thus treated can see, no such success could *therefore* be predicated for man.—SECRETARY.]

Abscess of the Lungs without previous Symptoms or any evident Cause.—Dr. COALE reported the case of A. G., æt. 26, a carpenter, who considered himself in perfect health until Wednesday evening, February 8, when he was taken with a violent “cramp colic,” referable to the right iliac or hypochondriac region. His physician partially relieved him, by opening his bowels. Costiveness again occurred, and the pain returned, so that on Saturday night Dr. C. was sent for in consequence of dissatisfaction with the physician in attendance. Refusing to visit at that time, Dr. C. did not see him till Tuesday, 14th. Found great tenderness midway between right costal cartilages and crest of ilium of same side. Bowels costive. Tongue furred. Pulse 80. Purged gently and blistered the spot, which perfectly relieved symptoms, so that on Wednesday, 22d, eight days after, Dr. C. paid him the last visit he deemed necessary for a week. On the next Monday, February 27, he came to report himself at Dr.

C.'s office. He was costive, but otherwise well, and some aperient pills were ordered. On Saturday, March 4, Dr. C. was again sent for. The patient had been improving in general health and strength until Tuesday afternoon, when he was taken with a sudden tickling in the throat, and on coughing a gush of pus took place from his mouth, amounting in the course of two or three hours, as the family said, to the better part of a pint, half of the quantity coming at the first gush. When Dr. C. saw him he was sitting up, and very comfortable while in that position. Pulse 80. No unusual heat of skin. No pain. Cough at times, with expectoration of a half mouthful of fetid pus. Lying down instantly brought on cough. He took this position at Dr. C.'s request, and was soon compelled to rise, when he covered the bottom of a handbasin with pus immediately, it coming up very freely; R. Elix. opii et vin. ipecac., as occasion required. Being deprived of the use of his left hand, Dr. C. could not examine the chest, but this was done the next day very thoroughly by Dr. OLIVER. Dulness all around on the right side from outer edge of cartilages to sixth rib. There were sonorous, sibilant, and gurgling râles on that side, over the dull part; and some of these continued up to the clavicle. Under these circumstances, the previous good health and well being for the ten days before the gush took place; the sudden gush of pure pus; the dulness of the inner part of the chest; the mixed râles; we could not doubt the existence of an abscess of the lungs. No particular treatment was used, as none seemed necessary. Nature had a certain piece of work to do, and seemed fully able to do it. Dr. C. visited him until Monday, March 13th, the general symptoms not amounting to anything worthy of note during this period, except the necessity of sleeping in his chair, which apparently brought on a temporary swelling of his legs. Wednesday, 15th, he went out. Thursday, 16th, he called at Dr. C.'s house; no excitement of pulse, good appetite and digestion; slept well; coughed very little; expectoration very little. Saturday, 18th, again reported himself as doing very well. Saturday, 25th, very well in all general and apparent circumstances. Dulness as high as eighth rib. A feeble respiratory sound before this. No râles. Face of a good healthy colour. Strength good. In short, no marks whatever of the invalid about him, except the physical signs just mentioned.

To conclude: 1. The case was evidently one of pulmonary abscess. 2. It came on without any previous ascertainable cause, either external or in the system of the patient. 3. It was not preceded by any appreciable symptoms. 4. It got well without any treatment being necessary.

Dr. Coale added that these peculiarities, so far as he had been able to ascertain, make it a unique case, particularly the first and second points.

External Use of the Tincture of Aconite. Three Cases.—Dr. STORER observed that, after the discussion which took place a few meetings since upon the effect of aconite, he had determined to present to the Society several cases in which he had administered it at the Hospital. He had, however, been unable to analyze the cases, and would, therefore, offer but these three cases of about sixteen which he had treated; and they among the most favourable.

M. E., æt. 45, entered Hospital, June 4, 1850;—six months previous to entrance was attacked with pain in lumbar region and left lower extremity, most severe in former, confining him to his bed for two months. After the application of a blister to back, two months ago, pain had nearly disappeared and has been very slight since, but that in leg at once increased, the limb being always cold and at times numb, with some diminution of sensibility; though lameness appears to have been caused by pain rather than by loss of

muscular power; walks with a cane, but with considerable difficulty on account of pain. Some tenderness on pressure along outside of left thigh as well as in gluteal region.

8th. Dry cupping about region of sciatic nerve on left side.

9th. *Acupuncture* to left hip succeeded, and the following liniment twice daily to be applied to hip: R. Olei olivæ ʒiiss; acidi. sulph. ʒiiss; olei terebinth. ʒss. M.

12th. *Cantharidal Collodion* applied to left hip.

13th. Much relieved since blistering.

24th. Pain about the same of late, though he walks much better than on entrance. Reapply blister. 29th. Pain about the same.

July 1. I took charge of the Hospital and found patient walking not only with a cane, but with great difficulty, with *two*—or rather crutches, although I find no record making mention of this fact.

2d. Complaining of pain and numbness in thigh. Applied to thigh tinct. aconiti sat. ʒj, twice daily. 3d. Less pain.

4th. Continues to receive relief after each application of aconite.

5th. Soreness in knee diminished.

6th. Knee continues relieved. Still has a dull pain in hip.

12th. Still improving. Apply aconite three times daily.

19th. Had a shooting pain last night, not only through hip, but down leg to ankle. Returned this morning. Relieved by application of aconite.

20th. Last evening and this morning pain returned in hip extending through whole length of limb to ankles. Aconite increased to ʒii.

21st. Pain returned in night of a character similar to that of the previous day, and continued for about two hours; relieved, he says, by application of the remedy.

23d. Paroxysm of pain through leg last night which continued about half an hour; relieved by application. No return of pain this morning.

30th. Doing well.

August 1. Complains of no pain in thigh; reports "as well as ever." Discharged well.

June 30, 1853. J. H. E., aged 40, works in a clothes-drying machine. About a year ago was attacked with pain and numbness and prickly sensation in soles of feet, which afterwards extended to hands. At times, at intervals of a month, had bilious attacks with vomiting, lasting a day or two. Gave up work two months ago. A month since was attacked with sudden pain in shoulder running down to the heart. Was delirious with pain in the first part of attack. Unable to sleep without an anodyne. Has lost flesh very rapidly. Has not walked for a month. Has no power over lower extremities below the knees, but sitting in a chair can raise his feet upon the bed by the flexors of the thigh. Has not lost sensation in feet, but experiences great pain in ankles and soles of feet. Arms powerless below elbows; sensation somewhat impaired, though a "prickly pain," as he expresses it, is produced by rubbing their posterior surface. P. M. Complains of very acute pain in feet upon pressure. Constantly groaning. Let him have of R. Morphine. sulphatis gr. ii; aquæ ʒj; a drachm, and repeat every hour till relief. Apply to soles of feet—R. Tinct. aconiti saturat. ʒj.

July 1. Took morphia twice. Aconite was applied once. Very soon experienced some relief. Complains now of no pain. Speaks only of a sense of numbness in upper and lower extremities. Let him be rubbed night and morning with—R. Tinct. capsici; tinct. cantharid. fab. āā ʒj. M. Repeat morphia if necessary; if pain return in feet, reapply aconite.

2d. Thinks he has more use of hands. Reports himself as much more comfortable.

3d. Slight return of uneasiness yesterday, P. M., in right foot. Aconite was applied, and he soon experienced some relief.

4th. Has suffered since yesterday more from increased numbness in hands and feet, and burning sensation in calves of legs. By misunderstanding of nurse, aconite was not applied. Bowels constipated. R. Pil. aloes et colocyntid. gr. x.

5th. Return of inconvenience in feet yesterday afternoon; not, however, severe, and readily relieved by aconite. Had a comfortable night from morphia. Bowels open.

8th. Since 5th, has been quite comfortable; does not feel as well to-day; has suffered no severe pain, but still complains of uneasiness in feet and hands. Omit the wash now used on arms and legs, and substitute tinct. aconit. sat.

12th. Had yesterday a *salt-water bath*. Doing well.

14th. No return of suffering in feet. Thinks he has some motion in them. Has omitted morphia the last three nights.

15th. Decided improvement in motion of hand, but makes no complaint.

16th. Still improving; has no pain. Substitute for aconite, *salt-water*, *warm*, night and morning.

20th. Thinks limbs are less flexible, let them be bathed morning and night with tinct. sapon. et opii.

22d. Uneasiness in right foot not diminished for last three days. Apply unguent. veratriæ gr. viii. to the drachm.

25th. Sensation in hands now nearly natural; motions in all directions; complains only of weakness. Motions of feet constantly improving.

I. B., æt. 38, engineer, entered Hospital October 12, 1853. Some four months since, while at the West, after taking cold, was attacked with violent pain, and heat and swelling in feet, especially the left. Had about the same time an attack of bilious diarrhœa, and was obliged to keep his bed for two months. About two months since came from Ohio, and now makes no complaint but of pain in feet, which are somewhat swollen; the cuticle is peeling off as if bruised. Feet not so tender but that he can walk about. Says he has often excruciating pain by night, keeping him awake; has been in the habit of taking morphia every night to procure sleep. Pain is constant in top of foot, and at times also shoots up his leg and down to the toe. Apply to feet, tinct. aconit. sat. ʒj ter die; fluid extract of valerian ʒj, at bedtime.

14th. Had a disturbed night from pain; now bears pressure much better than yesterday.

15th. Suffering in feet diminishing.

19th. Free from pain since yesterday's visit.

20th. Had shooting pains at intervals during night, which he attributes to omission of morphia at bedtime.

21st. Says for last three or four days has been relieved during the day, but pains return at night, continuing till morning. Let him have sulph. quinia, grs. ii ter die.

23d. Continues to improve.

26th. Had a very comfortable night, better than any since entrance; took no morphia.

31st. Complains of sensation of soreness rather than pain in right foot. Substitute for tinct. aconite, tinct. saponis et opii ʒj.

Nov. 4. Thinks he has improved rather less since omitting aconite. Apply olei tereb., tinct. sapon. et opii, tinct. aconit., equal parts.

6th. Less pain than any morning since entrance.

19th. Able to walk a mile or more without difficulty; has no continuous pain, but occasionally it darts along ball of foot and to great toe—not up the leg. Bears pressure everywhere without pain, but a slight touch to toe causes a feeling of tenderness. General health perfect.

21st. Discharged well.

Dr. PUTNAM suggested that instead of a specific quantity being ordered to be applied, it seemed a better way to direct the tincture to be rubbed in until tingling was produced.

Dr. STORER thought that too large a quantity might be absorbed in this way.

Dr. PUTNAM had never seen persistent ill effects from its external use; but he had heard of an instance of permanent facial paralysis following its employment in the way mentioned by Dr. Storer for neuralgia. It sometimes produces distressing temporary effects. Dr. P. added, by way of sequel to a case formerly reported by him, in which a drachm of the saturated tincture had been swallowed by mistake (see No. of this *Journal* for July, 1853, p. 69), that the patient, after recovery, found herself completely and permanently cured of the neuralgia, for which the external use of the remedy had been ordered.

Dr. BETHUNE thought that the principal difficulty in the employment of the tincture of aconite was, that the part became, in time, insensible to its effects. He mentioned cases in which this occurred.

Protrusion of the Nictitating Membrane in a Horse, and Tumour connected therewith.—Dr. BETHUNE was consulted a few weeks since by a gentleman who owned a valuable horse, in reference to a protrusion which had occurred between the lids of one of his eyes. On examination, it was found to be a fold of the nictitating membrane. It had existed above three weeks, and the owner thought was connected with an attack of the distemper, which appeared severely last spring, and from the effects of which the horse had never entirely recovered. The eyes, he thought, had been inclined to run from that time. After waiting a week longer, as the disease appeared to increase, it was determined to remove it. Assisted by Dr. Wood, the veterinary surgeon of this city, the horse was cast and secured. A further examination now showed a tumour beneath the conjunctiva of the lid, by which the nictitating membrane was pushed out from between the lids. This was removed, and was found to be of the size of a walnut, elongated and irregularly rounded. Its external appearance and the microscope both proved its fibro-cellular and non-malignant character. A portion of the exposed haw was removed with it, and a week or ten days after, the animal appeared quite well. As far as he could learn, tumours in this position in horses are quite rare. In this case, it probably had no connection with the previous catarrhal affection.

Poisoning by Prussic Acid.—Dr. H. G. CLARK reported a case of suicide by prussic acid. A German, after playing whist and drinking beer at a public house, went to bed. Soon afterwards he was heard groaning, and, the room being entered, he was found insensible, and died in fifteen minutes, and in three quarters of an hour after entering his room. The organs of the body were all found to be perfectly healthy, except that the brain was slightly congested. No odour of prussic acid was perceptible in the cavities, though carefully sought for. The stomach was given to Dr. Bacon for examination.

Dr. BACON stated that the stomach, which had not been opened at the

autopsy, was examined on the third day after the patient's decease. Its exterior presented nothing remarkable, except that the course of the large venous trunks, especially about the cardiac end, were marked by dark blue lines; on opening it, the mucous membrane, at the greater curvature, was found somewhat congested, and of a dark red colour, which passed gradually into a light pink at the pyloric end. The contents were about six ounces of a gray, pulaceous mass, with fat and fragments of food; they were acid to test-paper, and had a peculiar acid odour, which did not suggest that of prussic acid, though, after the detection of this poison, a distant resemblance was recognized in the odour.

Prussic acid was discovered by the appropriate tests, applied to the vapour evolved from the contents without heat. A portion of the contents distilled by a water-bath, yielded a clear fluid which had a decided odour of prussic acid, mixed with that of butyric acid, and in which prussic acid was readily detected. Perhaps a drachm of the medicinal prussic acid was present in the contents of the stomach; how large a quantity had been taken could not be ascertained. No other poison was found.

Adherent Placenta.—Case reported by Dr. STORER. Dr. S. was called, on the evening of the 27th ult., to attend Mrs. S. in labour with her first child. Patient aged 21 years; very stout and healthy, weighing 180 pounds. After suffering to a greater or less extent for forty-eight hours, she was delivered of a still child. The second stage of labour continued only about a couple of hours. After waiting some time for the expulsion of the placenta, Dr. S. passed his hand into the uterus, and found the after-birth firmly adherent, throughout its entire extent to the parietes of the organ. He made a slight effort to remove it, but found it impracticable. At the expiration of an hour he determined to attempt its extraction, and, expecting much hemorrhage to supervene, and that a considerable length of time would necessarily be employed in the process of detachment—if, indeed, it could be done—he administered half of a drachm of ergot. After it had been taken, the remembrance of two similar cases which had occurred in his practice several years since, both of which had terminated fatally, the one in opisthotonos, and the other in inflammation and disorganization of the fundus of uterus,* determined him to do nothing further without assistance. He accordingly sent for Dr. Channing, who made a very careful examination, and ascertained that a very slight detachment of the placenta, of perhaps an inch in extent, had taken place towards the anterior portion of the fundus, produced undoubtedly by the powerful uterine contraction caused by the ergot. As great suffering was produced by the most careful manipulations, Dr. C. administered the sulphuric ether by inhalation, while Dr. S. reattempted the extraction. With much difficulty he was enabled, by commencing at the detached portion, gradually to separate the placenta by peeling it, or rather tearing it, from the uterus; and eventually, after exertion continued for about twenty minutes, almost the entire portion was removed; small detached particles being alone left. The uterine contractions were so severe that but little hemorrhage ensued, and she was left a couple of hours after the birth of her child, comfortable. For several days she was watched with unusual interest, but not an untoward symptom has occurred since the night of her accouchement, a month since.

April 10. Compressed Aspect and Condition of the Lung after Bronchitis.—Dr. J. B. S. JACKSON referred to an instance of this appearance

* Both of these cases were reported to the Society at the time of their occurrence.

noticed by him in the lower lobe of the left lung; a compressed and flaccid condition, as if the organ had been beneath pleuritic effusion, of which latter there were no traces. Dr. J. remarked that Dr. James Jackson had inquired of him, respecting this case, whether the *bronchia* were examined, referring to the views of Dr. Gairdner (of Edinburgh), relative to this state of the lung, and that it is often caused by the exclusion of the air from the vesicles, and their collapse consequent upon the blocking up of the bronchial tubes by tough and adhesive mucus. Dr. J. said that this patient had been subject to bronchitis; he had not, however, observed any mucus obstructing the air-tubes, except one, on section of which there was some noticed. The upper lobe of the left lung had become hypertrophied, to supply the lack of action consequent on the compressed condition of the lower lobe; the upper lobe was estimated by Dr. J. to be one-half larger than its ordinary size. He referred, in this connection, to an instance of entire destruction of the left lung in infancy, and in the subject of which the right lung became nearly as large as *both* usually are. In the case which forms the basis of these remarks, there was emphysema of the anterior portion of the lungs, and also some pneumonia (Dr. Gairdner mentions the occurrence of emphysema in his cases); in the middle of the right lung there were appearances as if of tubercular disease, but, in fact, this was discovered not to be the case; there were but few tubercles in any portion of the lungs.

Dr. BOWDITCH remarked that often, lately, when about to puncture the chest in cases apparently offering most of the signs—and particularly the flatness on percussion—of pleuritic effusion, he had thought of Dr. Gairdner's opinions, having found, in many instances, no fluid upon effecting paracentesis.

Dr. BETHUNE asked if some dulness be not often caused, in *pleuritic* cases, by the existence of false membrane?

Dr. BOWDITCH replied affirmatively, but added that this cause would not account for *all* the dulness on percussion which is sometimes found.

Dr. MINOT inquired if mensuration of the chest would not generally show a relative difference in the volume and in the shape of its two sides, in cases of pleuritic effusion?

Dr. BOWDITCH: Not necessarily.

Dr. C. E. WARE referred to instances of bronchial respiration existing in pleuritic cases, and asked if it does not always, or, at least, most frequently exist?

Dr. BOWDITCH thought it often concomitant. He remarked that it is not always the length of time the effusion has existed, that is the cause of the compressed condition of the lung, for he had operated on a case in which a pleuritic effusion had existed several months, and yet the lung expanded freely and very soon; whereas, in another case, of a few weeks' duration, the condensed condition of the lung was manifest for months after the patient was comparatively well.

Empyema.—Dr. C. E. WARE reported the case of a man, 56 years of age, of a strong and vigorous frame, who had had a slight cough, and occasional pain, at first in his right side, afterwards in his left, for about three weeks previous to the time when Dr. Ware was called to him, Feb. 22. He had had his usual appetite, and attended to his usual duties. In the afternoon of Feb. 22, he was attacked with a much more violent pain than he had experienced before, in his left side, accompanied by considerable dyspnoea, but by no chill, or any marked febrile symptoms. The next morning, there was dulness on percussion at the base of the left lung, joined with deficient

respiration, a bronchial expiration, and resonance of the voice. The next morning, Feb. 23, there was, as high as the angle of the scapula, on the left back, bronchophony, and bronchial respiration, with a crepitus around it. Cough, and expectoration of a viscid, tenacious, transparent mucus, in considerable quantity, without colour, and without blood. Pulse 96. On the 24th, the bronchial sounds were less pure, and were mixed with fine crepitous râles. On the 25th, the bronchial respiration had entirely disappeared, and subcrepitous râles were heard over the whole left back up to the spine of the scapula. The dyspnœa and cough had very much subsided. The pulse 96. On the 28th, the pulse was 72, and he was in all respects convalescent; there was still some dulness on percussion, and subcrepitous râles in left back.

March 2d. There being no aggravation or return of any other symptoms, Dr. Ware discovered an obscure and distant bronchial expiration at the angle of the left scapula. On the 4th, pulse and tongue natural, good appetite, natural sleep. At the angle of the left scapula, bronchial expiration without râles, bronchophony, and dull percussion. On the 5th, he was on his bed, with pulse 108. The 7th, perfectly flat on percussion over the left back, with a clear œgophony; slight cough, with no expectoration. To the 10th, the effusion continued to increase. There was more cough and dyspnœa. The pulse was 120. The expectoration was viscid transparent mucus, very small in quantity. Through the day, the dyspnœa continued to increase very rapidly until 9 o'clock P.M., when the patient was sitting up, unable to lie down; skin cold and damp; pulse 128, very feeble; respiration gasping; countenance sunken; voice whispering. On percussion, there was entire flatness over whole left chest. The heart was pushed entirely to the right of the median line.

At 9 o'clock, the left chest was tapped a little below and outside of the angle of the scapula, and with a suction-pump two quarts of thin and very fetid pus were drawn off. At half-past 10, about three quarters of an hour after the operation, the expression and colour of the countenance had become natural; the skin warm and dry; the respiration easy, in a reclining position. There was a good resonance on percussion over the whole chest, together with respiratory sound and crepitus; the pulse 120. At 5 P.M. of the 11th, the pulse was 112, full. There was a subcrepitous râle over the whole left back, and the sounds of the heart had returned to their normal location. There was not now, nor had there been at any time, the slightest fetor of the breath. On the 12th, the general condition was the same; the pulse 108. When lying on his back, under the left clavicle, for about two inches down, there was very strong tympanitic resonance on percussion, with loud, copious gurgling; but no resonance of voice, nor pectoriloquy. Immediately, on sitting up, there was dulness on percussion, and subcrepitous râles over the same space. From this time his pulse continued to increase in frequency, and it was evident that the fluid was again accumulating. His general symptoms were all aggravated, and he died very suddenly, at 2 A.M. on the 16th.

Autopsy.—Very little emaciation. On puncturing the pleura, there was an escape of gas. On removing the sternum, there was discovered, on the left side, a cavity, at the upper part, about half full of thin fetid pus, separated, by a septum of adherent lung, from another cavity, at the base, of about the same size, containing about the same quantity of pus—about a quart in the whole. These cavities being in the pleura, the lung, at the apex and at the base, was compressed, and both cavities were lined by a thick coating of lymph. Air forced into the trachea expanded the lung, but showed no opening into the pleural cavity. There was no sign anywhere of gangrene. The lung,

although condensed by compression, appeared healthy and firm. It was rather dark coloured at the surface, but less so at the root, where it was less compressed. At one point, near the mediastinum, there was a purulent deposit in the substance of the lung, about a teaspoonful in amount. In the mediastinal cellular tissue, there was extensive purulent infiltration. There was no purulent deposit discovered in any other organs of the body. The right lung and pleura were perfectly healthy. There was no trace of tubercle in either lung. There was a slight effusion into the pericardium, and the surface of the heart and pericardium were coated with a very rough deposit of lymph. Nothing abnormal in the heart. Nothing was discovered after death to explain the extreme fetor of the fluid in the pleural cavity. There was no communication with the air; there was no gangrene. There were no tubercles. The effusion was not of long duration. It supervened very gradually upon the subsiding pneumonia. The pneumonia was not attended by very active symptoms, and was short in its duration, yielding promptly to leeching and simple opium treatment. The sudden death was probably due to the complication of pericarditis with the plenrisy. Dr. Ware thought that this must have occurred after the operation. Had it existed earlier, he thought, that after the chest was evacuated, and before it filled again, he should have discovered it by the physical signs in his repeated examinations of that region. The occurrence of gurgling with tympanitic percussion, at the upper part of the left chest, was an interesting physical sign to notice. No one who heard it would hesitate a moment to say, unless he was cautioned by the history of the case, that there was a large cavity in the lung. The lung was perfectly healthy, except for compression. The sounds were owing to the lung expanding in a cavity containing air and fluid.

The operation gave very great and immediate relief. Indeed, death was imminent, and it saved the patient's life for the time. He would have been operated upon again, had he lived another day. It was obvious, however, from the appearances at the autopsy, that the operation would have been unsuccessful and useless. The chest had been divided by the adhesion of the lung since the first operation, so as to form two cavities, each containing a large quantity of fluid. The lung was adherent just where there was every indication externally for the performance of the operation. It was so compressed by the fluid where it was in contact with the walls of the chest, that it returned no more sound than when there was fluid. The lung would, therefore, in all probability, have been perforated; and not the cavity.

Dr. BETHUNE asked if it were possible that air could have entered the chest, when the latter was tapped for the purpose of drawing off the contained fluid?

Dr. WARE thought *not*; he referred the production of gas within the chest to decomposition of the pus.

Dr. BOWDITCH was quite certain that no air could have gained admission at the operation; he had taken sufficient precautions to prevent such an accident.

Dr. J. B. S. JACKSON, referring to the adhering band of false membrane, which was stated to have divided the thoracic cavity into two parts, said that it is not uncommon to find such a structure, and also a pint or two of foul matter *below* the band, while the membranes are in a better condition above, and sometimes quite smooth and polished.

Dr. BOWDITCH thought that it was quite rare to find so very *fetid* pus in the chest. He regretted that he had not injected iodine and iodide of potassium into the thoracic cavity. He thinks that, in similar circumstances, he should decidedly use this means. He agreed with Dr. Ware in his remarks that a *second* operation would have been of no service to the patient.

Premature Birth.—Dr. STORER had put a woman to bed, since the last meeting of the Society, with a premature child; she lacked two weeks of being six months pregnant; in other words, she was five and a half months advanced. The child weighed just one pound; measured $12\frac{1}{2}$ inches, entire length; $6\frac{1}{2}$ inches to the umbilicus; $8\frac{1}{2}$ inches across the occiput, and lived fifteen hours.

Erysipelas after Vaccination.—The following remarks were elicited by the report of a case of the above nature, at the meeting of the Society, holden March 27; and which was related by Dr. STORER, in reply to a query by Dr. J. M. WARREN, whether any such cases had been observed, he having lately heard of a case following revaccination.

Dr. STORER's report is that he lately saw a child in whom erysipelas was developed after vaccination. The matter introduced was taken from a perfectly healthy child; the day after vaccination, erysipelas appeared about the punctures and gradually extended over the whole arm, then attacked the other arm, passed down, covering the entire back, and disappeared after extending to the ankles. The child was about nine months old, was kept constantly at the breast, and at the expiration of ten weeks was entirely well. Dr. S. had applied the tincture of iodine with benefit, though without arresting the disease, as he has usually been able to do with this remedy.

Dr. DURKEE inquired of Dr. STORER, if, in the above case, the application of the tincture of iodine prevented the formation of vesicles upon the integument?

Dr. Storer replied that there were no vesicles formed, and he spoke highly in favour of the tincture of iodine as a local application in erysipelas; in every case, it almost instantly arrests the progress of the disease, and he had not found, usually, that the latter extended beyond the portion of the skin covered by the application.

Dr. D. remarked, that for several years he had used iodine as a topical remedy in erysipelas. He considered the simple tincture inferior to the ethereal preparation. He had used the latter, having a strength of thirty grains to the ounce, and applied it freely to the affected surface. It did not produce as much pain as the ordinary tincture. The ether evaporates instantly, and, if the patient complains of the severity of the pain, the application of pure ether would dilute the iodine at once, and thus diminish the suffering. He knew of no other topical remedy that would prevent the formation of the phlyctenæ, and arrest the erysipelatous inflammation at this stage; but the iodine will do this if liberally applied, so as to turn the skin nearly black. Merely to produce a brown discoloration is not sufficient. A physician in full practice in a neighbouring town recently stated to Dr. Durkee, that he had used the ethereal tincture in quite a number of cases of erysipelas, and he regarded it as decidedly preferable to any other outward application.

Dr. D. considers the vascular portion of the skin to be the primary seat of the disease in question. In simple erysipelas, the superficial capillaries alone are implicated; and, during the first or congestive stage of the inflammation, the iodine can be applied advantageously, and will usually arrest its further progress. But there is quite a different state of things in the second variety; that is, in phlegmonous erysipelas. Here the morbid action, which commenced in the superficial vessels, extends to the deeper vascular tissue of the chorion and the subjacent cellular membrane. And the inflammation, instead of producing a serous effusion in the form of vesicles upon the surface, yields a purulent matter, which is infiltrated into the subcutaneous cellular and muscular tissues.

Dr. PUTNAM asked for results of the application of tincture of iodine in the erysipelas following surgical operations and accidents.

Dr. CABOT had found it not satisfactory when applied upon *surfaces*, but serviceable when thrown into *sinuses*; traumatic erysipelas, he remarked, affects tissues more deeply than does that of the surface; hence, local applications are likely to be less effectual. In a case of erysipelas supervening upon ecchyma, in an old woman, Dr. C. had found the tincture of iodine ineffectual in arresting the progress of the disease.

Dr. C. E. WARE had never witnessed any efficient curative action from the local application of tincture of iodine in erysipelas.

Dr. DURKEE insisted again, and strongly, upon the *very free* use it was necessary to make of the tincture; he often pours twenty or thirty drops upon the affected part, spreading it instantly with a camel-hair pencil.

Dr. PARKMAN asked if any *constitutional* treatment was tried in Dr. Storer's case?

Dr. Storer said there was none; the patient being so young, it was not judged advisable to attempt any, even had it been deemed practicable.*

Dr. Parkman thought that many cases of erysipelas were referable to constitutional causes, and should be treated accordingly.*

Dr. STRONG believed that depression of the vital powers and debilitating influences exercised upon the system, together with depravation of the blood, were circumstances preparative and *quasi* causative of erysipelas in nearly, or quite, all cases; consequently, the manifestation of erysipelas is a measure, as it were, of the amount of derangement or depravation of the system.

The connection of erysipelas with *puerperal peritonitis* being alluded to;

Dr. C. E. WARE said, that although the terrible consequences of the accident when it did occur, made it the duty of every physician to be very guarded how he passed from cases of erysipelas to cases of midwifery; yet that the accident was a very rare one, and, probably, liable to occur only under some peculiar circumstances, or only from certain forms of erysipelas. It frequently happens that for intervals of months at a time erysipelas is epidemic amongst us, and no physician, in large practice, is, during that interval, without more or less cases of erysipelas upon his hands. He never heard of any physician abandoning his practice of midwifery on this account. And he had heard of only one or two instances within the last fifteen years, known to the rest of the profession, in which physicians were supposed to have communicated the disease.

Dr. PUTNAM remarked, that he had been much surprised to find Dr. HODGE, in a recent lecture, scouting the *very idea* of any such a connection.

[At a recent meeting of the "Medico-Chirurgical Society," of Richmond, a discussion upon the effect of local applications in *erysipelas* elicited various opinions. Among others, Dr. OTIS, one of the editors of the *Virginia Medical and Surgical Journal* (see number for April, 1854), "expressed his astonishment at the importance attributed to the local treatment of erysipelas by the physicians who had spoken." None of the local applications (among which he refers to *tincture of iodine*), he maintains, are supported by any precise evidence derived from any considerable number of cases in which they diminished the duration of the disease; most of them have the disadvantage of concealing the seat of the disease. It would appear that no one has tried the

* [M. GRISOLLE, who is opposed to all *external* medication in erysipelas, remarks, also, that none of the applications made use of, even the actual cautery, have ever availed in the erysipelas in the *newly-born infant*.—He does not mention tincture of iodine among the other applications.—*Pathologie Interne*, vol. i. p. 538.—SECRETARY.]

tincture of iodine in the thorough manner recommended by Dr. Durkee; it is, at any rate, not probable that they have *poured* it upon the part affected, and rendered the surface nearly "black" with it. Now, if Dr. D. and others, assert nearly universal success in arresting the disease by this method, we may be said to have *commenced*, at least, the accumulation of "precise evidence" in favour of the iodine, and particularly of the ethereal tincture.

Dr. Otis regards abortive treatment in "periodic erysipelas," as particularly reprehensible; the application of *collodion*, however, he had observed to be attended, apparently, with advantage. If the tincture of iodine or any other local application be proved sufficient to arrest the progress of erysipelas, what "disadvantage" can there be in the mere concealment of the affected part by such application?

For the tincture of iodine, however, we contend that it does *not* "conceal" the processes going on beneath it, even when applied in the liberal manner in which Dr. Durkee uses the ethereal tincture.

It should be distinctly stated that physicians here rely mainly, and from the first, upon *constitutional* remedies; tonics, and especially quinia, with wine and other stimulants, are freely administered. The advantages of certain local applications can hardly be denied in the face of strong testimony in their favour, and if any of them will safely and efficiently check the progress of the disease and *hold* it in check, *they* are to be preferred, of course, to mere palliatives, and are of exceeding value combined with constitutional means.

Dr. Wood (*U. S. Dispensatory*, 6th ed., 1845) thus distinctly recognizes the power of the tincture of iodine in *arresting* the erysipelatous inflammation: "It (tincture of iodine) is much used in this state (*i. e.* undiluted) in erysipelas, &c., and often with very happy effects. But its application requires some caution; we are in the habit rather of surrounding the inflamed surface with a border of the tincture, embracing a portion of both the sound and the diseased skin, so as to prevent the progress of the inflammation, than of attempting a complete cure by covering the whole surface affected."—(*Op. cit.* p. 1175.)—SECRETARY.]

Imperfect Physical and Mental Developments, in a Child born in Breech-presentation, and with difficulty resuscitated from its asphyxiated condition.—Dr. MINOT had met with the above case, which was similar to others reported by Drs. Gould, Storer, and Alley. The child, at the age of six years, could not walk alone, and had only partial control over his arms. There was strabismus; articulation was imperfect, and the mental faculties were very feebly developed.

April 24. Encysted Gelatinous Tumour of the Neck.—Removed by Dr. NATHANIEL MILLER, of Providence, R. I., April 8, and sent to Dr. B. S. SHAW, of Boston, who, through the Secretary, communicated the following account: The tumour was deep-seated beneath the sterno-mastoid muscle, and moving upwards and downwards in the act of deglutition; about three inches in longest diameter, oval, but not quite so thick as broad. It was diagnosed as an encysted tumour containing a fluid of some kind, but, on cutting down upon it, it was found to have a glandular and solid appearance. The tumour was closely attached to the trachea, and after having dissected out rather more than half, the hook slipped in and revealed its nature. A part of the sac having been cut away, the remainder was cauterized and allowed to suppurate. The patient was an English woman, æt. 32, married.

The membrane of the cyst was quite thick and firm, measuring about a quarter of an inch in diameter, and a purely fibrous structure; no lining epithelium seen. The contents had exactly the appearance of calf's-foot jelly, or of a dense jelly with the colour of brown sherry wine. It was perfectly transparent. In its structure it was amorphous, filled with the finest granulations, and with a few old and dried blood-globules imbedded in it. It was insoluble in boiling water, but soluble in alkalies, and from these solutions again precipitated by acids, showing its nature to be albuminous and not gelatinous. A small quantity of fluid substance of the same colour, escaping with the jelly, was found to coagulate by means of heat and acids.

This tumour seems to be of the same nature as a gelatinous encysted tumour of the breast exhibited to the Society last November, except that, in that case, the jelly was of a more fluid kind, and presented a variety of colours, probably owing to blood in different stages of decomposition.

The specimen was exhibited to the Society.

Dr. J. B. S. JACKSON asked if it were connected at all, originally, with the thyroid body? Such a connection was supposable and not unlikely.

[Dr. Shaw cannot learn from the original communication that there was any connection of the tumour with the thyroid gland. It would seem likely that Dr. Miller would have mentioned the fact had such been the case.—SECRETARY.]

Peritonitis.—Dr. SHATTUCK reported three cases, with the following pre-fatory remarks: It has been noticed that uncommon and unusual cases are apt to be observed in close sequence of time and place. Three cases of peritonitis have been lately observed, two in the same bed, and one in an adjoining bed, of one of the small wards of the Massachusetts General Hospital.

A tall, well-formed man, 53 years of age, was admitted into the Hospital on the 21st of March, 1854. He belonged to a rheumatic family, had suffered for years from rheumatic pains of the shoulders, had had two attacks of sciatica, and in his occupation as a peddler had been exposed to alternations of temperature. He was complaining of weakness, stiffness, and pain of the lower extremities, from which he had been suffering for nearly a year. The pain commenced in the sole of the left foot, and extended up the legs as high as the lumbar vertebræ. He walked about the streets with a cane till the ground was covered with snow, when he confined himself to the house. He could get about by taking hold of the furniture, till within a week or two, when he has needed the assistance of some person. Constipation; within a few months, occasional dysuria, and retention of urine; and within a few weeks incontinence. Nothing remarkable to the eye or touch about legs or back. Tickling soles of feet followed by disagreeable sensation and spasmodic movements of legs, which are sometimes drawn up of nights. This condition did not vary much for a few days. Constipation not easily overcome; uncomfortable sensations about bowels, back, and legs, rather than pain; increased difficulty of moving, so that he required assistance to be turned in bed. On the 1st of April, he complained of abdominal pain. On the 4th, the abdomen was full, resonant, and rigid; the scrotum was red, oedematous. There were two inguinal hernias, that of the right side of fifteen years' standing; it was reduced with but little difficulty. The urine passed in bed; the pulse 108, small, feeble; restlessness and discomfort; a large enema, followed by two free dejections. He took a grain of opium every three hours; failed rapidly; and died at 6 o'clock on the morning of the 5th.

At the *autopsy*, several ounces of pus were found in the peritoneal cavity.

Injection of the serous coat of the intestines, as well as of the membrane lining the walls of the abdomen. No adhesions of the intestines; that part which had protruded through the inguinal canal could not be made out. The hernial sac empty; its lining membrane red, with shreds of false membrane; the scrotum, of the size of both fists, oedematous.

Here we have an attack of peritonitis, without known cause, in a patient the subject of rheumatic attacks, and with partial and increasing paralysis of the lower extremities of a year's standing.

An Irish labourer, who gave his age as 43, but who looked to be at least ten years older, and yet as if he had had a good constitution, was received into the Hospital on the 18th of April, 1854. He had lived in Boston or its neighbourhood for twenty-two years; his health had been good, but he had come from the country a fortnight before, on a visit, and had been indulging himself in eating and drinking. He had some pain in the stomach a week before his entrance, but kept about, and on the 14th, four days before his entrance, after eating freely, he was seized with severe abdominal pain, no vomiting; constipation; and he took salts on the 15th, and went out. On the 16th, pain in right hypochondriac region, with chills, pain in back and abdomen, and he was obliged to go to bed, and to stay there. On the evening of the 18th, he presented the aspect of a man labouring under severe acute disease; the countenance dusky, petechial eruption on limbs and trunk, abdomen red from sinapisms, moderately full, the patient not complaining much on pressure. Flatness over lower right back; bronchial respiration; no râle. Pain in right hypochondriac region. Tongue brown, dry; nausea; occasional bilious vomiting. He died early on the morning of the 20th.

Autopsy.—The pericardium not adherent, containing six or eight ounces of reddish fluid, with flakes of yellow lymph. Surface of heart dotted over with soft lymph, that of right auricle had a striated and ecchymosed appearance. Two quarts of serum, with lymph, in right pleural cavity; two lower lobes of right lung carnified. *Abdomen.*—The intestines glued together by recent lymph. Pus and lymph diffused over the surface of the parietes and the viscera, particularly about the duodenum and liver. Duodenum much enlarged; small round perforation at the bottom of an old ulcer, close to the pyloric orifice of the stomach.

Intemperate eating and drinking seem to have been the determining causes of the rupture of the duodenum at the bottom of an old ulcer; and probably this took place in a very limited extent six days before death. The symptoms were not very severe, and he kept about for two days, and then the pain and vomiting returned, and he rapidly sank. It is worthy of notice that under these circumstances pleurisy and pericarditis should have supervened. The diagnosis at the time of entrance was not made out. He then complained of pain in the hypochondriac region, but the abdomen was not painful on pressure, nor was it tympanitic. He was much prostrated, and it was quite evident that he was sinking under very severe acute disease, the nature and extent of which were not to be ascertained in one so weak and suffering so much. In the third case the result was favourable, so that there has been no opportunity of confirming the diagnosis by inspection.

A Swede, 27 years of age, of a healthy family and of good general health, had a febrile affection, with diarrhoea, commencing about the 20th of December, and was confined to his bed for a month, and has not been able to work since, for more than a few days at a time, though he had tried to do so two or three times. On the evening of April 4, abdominal pain, weakness; and he came to the Hospital on the 6th. On the 7th, he was found sitting up;

cheeks thin, with an hectic flush; expression of weakness and pain, complaining of languor, restlessness, occasional nausea and vomiting. The pulse 120, small; the abdomen tympanitic, full, quite tender on pressure; constipation. Nausea and bilious vomiting continued during the day; he was very restless at night; the abdominal pain relieved by hot fomentations. He was ordered to take, on the 8th, two grains of opium every four hours, and at night the same quantity every three hours. The nausea, and vomiting, and restlessness, and pain disappeared; he slept but little till towards the morning of the 9th, and was sleeping soundly at the time of the visit, and sweating profusely. He drank freely of rennet-whey and lime-water, and took two grains of opium every three hours. He reported himself as very comfortable on the 10th, and asked for food; the pulse 118; the abdomen full, tense, tympanitic, not tender on pressure. On the 11th, he was found sitting up, dressed, and he begged for food. The abdomen was still tense and tympanitic; no dejection since his entrance. He took two grains of opium every three hours on the 11th and 12th, when he reported having been to the water-closet, and having had two free dejections. On the 15th, he had taken two quarts each of arrow-root and of rennet-whey. On the 17th, the pulse 88; the abdomen soft, not painful on pressure. He was ordered to take the opium pills once in twelve hours, and on the 19th to take them only at night; and on the 21st, they were omitted; on the 22d, he was allowed to take bread. On the 24th, he was put upon house diet, and allowed to go out, and has since been gaining strength and flesh; the bowels regular; no pain; the digestion easy.

Had this man typhoid fever in December and January? Was there any perforation in connection with an old ulceration? How far had the treatment anything to do with the successful issue? These are some of the questions open to members of the Society.

Abscess, containing Hair, on the Nates.—Dr. J. MASON WARREN had met with three cases of abscess of the nates connected with a fistulous opening over the coccyx, containing hair. The last case is a type of the two others. The patient, a young man, 20 years old, had for some time suffered from an irritation on one of the nates. Finally, an abscess formed and broke, followed shortly by one on the opposite side, both being the sources of great discomfort, the first having become fistulous. On separating the nates, Dr. W. at once discovered, about an inch below, in the median line and over the coccyx, a small aperture, about large enough to admit a probe, looking like a pit in the skin, and lined with epidermis. A probe, being introduced, penetrated to the depth of an inch and a half.

From his experience in the two former cases, Dr. W. was at once able to say that it led to a cavity containing hair, which was probably the origin of the abscess in the neighbourhood. An incision being made into this canal, it was found to terminate in a suppurating cavity, in which, lying quite loose, was a small bundle of hair. Radiating from this cavity were two canals leading to the abscesses in the nates mentioned above.

It would seem probable that originally the hair was contained in a cyst, which, from the irritation caused by sitting, had suppurated, and the pus had burrowed in different directions.

Dr. JACKSON referred to a specimen, in the Society's Cabinet, of a lock or pellet of hair taken from near the umbilicus.

Ovarian Tumour weighing Twenty-six Pounds.—The case occurred in the practice of Dr. GEO. HAYWARD, JR., and was reported by Dr. ALLEY.

The patient, a woman, forty-one years of age, of fair complexion, robust,

and active, mother of four well-formed children; nothing abnormal in their births. Had enjoyed good health until a year ago last February, when she began to feel some uneasiness in her abdomen, without swelling. Her catamenia ceased at that time, and continued suppressed until July, when a discharge of blood occurred, which continued at intervals for ten days. Her abdomen began to increase in size, and occasioned suspicions of pregnancy, which a careful examination, however, failed to establish. The discharge of blood continued at intervals, and the swelling of the abdomen increased till March, 1854, at which time the abdomen was punctured, and a pailful of fluid was drawn off, of so great density that it flowed with great difficulty through the canula, and it was necessary to clear out the tube occasionally to make it flow at all. Not long after, she died. At the *autopsy*, upon laying open the cavity of the abdomen, a large ovarian tumour was seen extending completely across the abdomen, crowding the viscera of that cavity into a very small space.

The tumour weighed twenty-six pounds, with the contained fluid. It was divided into three distinct cavities or cysts, with walls of unequal thickness; in one portion, of bouy hardness, and in another, breaking from the mere weight of the tumour. One of these cavities contained a serous, sanguinolent fluid, which flowed readily. The other two were filled with a ropy, tenacious semi-fluid, of such density that it could be cut across with a knife while flowing from the cavity. The tumour was attached to the uterus by a single pedicle, which, being severed, left the uterus remaining of normal size, and with some thickening of the cervix.

Use of Butyric Acid Ether for flavouring Ice-Cream; with a Sample of the flavouring, and of the flavoured article.—Dr. ALLEY read the following account, furnished by Dr. A. A. HAYES.

The sample of ice cream and the flavouring essence used abundantly in preparing it, now exhibited, were intended to form part of the refreshments offered at a festival in Beverly, Mass. Some of the attendants, having tasted the ice-cream soon after it was made, and before it was sent to the place of meeting, were rendered quite sick within three hours after eating it, and the apprehensions felt by their friends, joined to their own fears of poisoning, produced much excitement in the town.

These substances were placed in my hands for chemical examination. The flavouring substance, having the odour of the pine-apple, proved to be butyric acid ether, dissolved in spirits. It is well known that butyric acid is a product of fermentation, in which animal organized matter breaks up and reunites the elements of other organic matter, such as sugar. It is usually found in rancid butter, and in decomposing flesh and other animal matters at that stage when the exhalations are supposed to be infectious. This acid, united to the base of ordinary ether, constitutes the flavouring ingredient resorted to in this case, and it was free from any mineral poison. The ice-cream presented the usual character of a nicely prepared article, in which mostly cream was used instead of milk, and it was entirely free from any mineral body, of unusual occurrence. When subjected to chemical trials, the first odour of the butyric ether having passed off, butyric acid was detected, and the cream seemed to have passed, *through the presence of the butyric ether*, to an advanced state of rancidity. Although the low temperature at which ice-cream is kept might be supposed an effectual guard against any chemical change in which butyric ether may be considered as playing the part of a ferment, yet it is also well known that sugar is produced in milk and in organized vegetable matter in

the act of freezing. Sugar thus formed, together with that added to the cream, may react with the elements of butyric ether, at a low temperature, to produce butyric acid, or incipient compounds may result, which at common temperatures are converted into butyric acid.

A number of flavouring extracts, equally objectionable, are largely consumed by confectioners as substitutes for the volatile oils, and cases of severe sickness and alarm are multiplied every season from the practice.

In reference to this subject, Dr. JOHN BACON, JR., remarked as follows: The production of poisonous effects by the artificial flavouring extracts which are now coming into general use is a subject of practical importance, even if no more serious results should follow their use than sickness and vomiting, as occurred in the case above reported by Dr. Hayes. In that instance, it is possible, though not probable, that a spontaneously poisonous condition of the cream might have occasioned the illness, as such cases are on record. The artificial extracts were first prominently brought forward at the London exhibition, and were reported upon favourably by the chemists on the jury (Dr. Hofmann and De La Rue), as entirely safe substitutes for the volatile oils prepared from plants. It appears to have been assumed that the artificial products were identical in chemical composition and in properties with the natural ones which they resemble in flavour, in which case there could be no objection to substituting less expensive modes of preparation. In some cases they are certainly not identical; and, where any doubt exists, they should be used with great caution until positively ascertained to be harmless in their action on the system.

Dr. J. M. WARREN said he had been called to a family in which seven persons had been more or less poisoned by custards largely flavoured with "extract of vanilla," so termed; on analysis, nothing of a poisonous nature could be detected; yet, undoubtedly, the symptoms were referable to the said extract, which a cook, new to the family, had liberally used.

Dr. W. E. TOWNSEND referred to cases of illness caused, to all appearance, solely, by the eating of what are termed the "*acidulated drops*," and, particularly, of those termed "*banana drops*;"—he believed that one death, at least, in this city, during the last summer, was to be ascribed to their use; and certainly several instances of apparent poisoning, declared by severe sickness at the stomach, &c. &c.

[The editor of the *Annual of Scientific Discovery*, for 1853, remarks as follows in relation to the "*acidulated fruit-drops*:"—

"These have been denounced as poisonous by some persons, on the ground that fusel oil is known to produce deleterious effects; and, as a natural consequence, the confectionery referred to has been discarded. There is, however, no foundation for such statements or belief; and if the confectionery, flavoured with these extracts, has in any case produced injurious effects, it is undoubtedly to be referred to an injudicious consumption of it, and not to any inherent deleterious property."—*Op. cit.* p. 228.

This is certainly very positive language, and suited rather to reassure the somewhat daunted consumers of the said confectionery; with all due deference to so high chemical authority, however, it may be safely asked if there are not, undoubtedly, every day, instances of quite as "injudicious," and even more plentiful consumption of many *other* confections without any *such* effects as have been frequently observed immediately after using the kind under consideration. If this be admitted, do we not naturally look for *some peculiarity* in the confection after whose ingestion these effects are so often noticed? What, then, is the agent thus (seemingly, at least,) active? If such effects

be common, will not the "drops," and such like articles, be avoided at any rate? In perusing the entire article, upon "Perfumery, and the Artificial Extracts of Fruit," (*op. sup. cit.*), one can hardly escape being impressed with the feeling that the processes by which they are obtained, and their use, have been too readily sanctioned by the distinguished chemists whose names have been mentioned, and that more certain knowledge of their effects (occasional, at least,) upon the human system should be had before recommending them so fully.—SECRETARY.]

May 8. Fatal Peritonitis from Perforation of the Duodenum.—Dr. JOSEPH SARGENT, of Worcester, associate member of the Society, sent the specimen, which was exhibited by the Secretary, who read the following account of the case, furnished by Dr. S., who remarks that he thinks it a very rare one, and that it is the only one he had seen. The patient was a farmer, 35 years of age, and of large muscular development; he died April 28. He had, for months, complained of a feeling which he termed a feeling of "goneness" at the epigastrium, and had been troubled with flatulent eructations and occasional vomitings of green liquids, and had "*distressed turns*" at night, the distress being in the region of the transverse colon, and "*working off*" (as he expressed it) with diarrhœa. With all this, he had done rather less work than usual, but rode fourteen miles to Worcester, on a lumber-wagon, April 26, after having, on the previous day (only two days before his death), laid stone wall in the morning, and was sick in the afternoon, so as to vomit freely, which relieved him. He, however, ate no supper; was restless during the night, and rose early the next morning (27th), "feeling like death," and walked his room; ate but little breakfast; started upon his lumber-wagon for Worcester at 7 o'clock A. M., and arrived about 10 o'clock; felt very uneasy while riding, so that at last he got off the wagon and walked. At about 11 o'clock A. M. (April 27), while standing in a store in Worcester, he was seized with sudden distress in the right hypochondrium, so severe that he threw himself upon the floor, and lay there an hour or two, during which time he took something hot, with brandy, from an itinerant physician. Dr. S. saw him at 4 P. M., and found him complaining of severe pain in the region of the transverse colon, and also of pain in his shoulders. His abdomen had the hardness often observed in tetanic patients, and was not tumid, tympanitic, nor tender on pressure. With the view of inducing some relaxation of the abdominal muscles, and of facilitating an examination, Dr. S. allowed him to inhale a little chloroform, without procuring much relief to the pain or obtaining muscular relaxation. A teaspoonful of black drop was then given; and subsequently some calomel and ipecac caused emesis, and seemed to give slight relief. He took opium, however, nearly every hour through the night, and yet slept but little. At 8 o'clock A. M. of the 28th of April, he was vomiting incessantly a thin, yellowish liquid; pain somewhat diminished; tendency to be cold, with clammy perspiration; he was pulseless; intellect clear. There was now great tumefaction over the entire region of the transverse colon, with tenderness; abdomen still very hard; no hicough. He still complained of pain in his shoulders, to which he had long been subject. He remained in this condition till about 3 o'clock P. M., when he died, twenty-eight hours after the acute attack.

Post-mortem examination, eighteen hours after death.—General peritonitis; serum, pus, and lymph in the abdominal cavity; universal injection of the peritoneum, with agglutination of some folds of intestine; inflammation most intense in the region of the transverse colon and stomach; three or four cica-

trices of ulcers in the mucous membrane of the duodenum, through the centre of one of which, very near to the pylorus, and in immediate contiguity to most intense inflammation, a circular perforation was discovered. The intense inflammation was not in the ulcer, nor in the duodenum, but in the folds of intestine lying over the latter. The base of these ulcers (which do not seem to be recent) is the muscular coat of the intestine.

Cauliflower Tumour of the Uterus (Sir Charles Mansfield Clarke). *Tumeur Polypeuse de Nature Végétante et Fibro-plastique* (Lebert). *Epithelial Cancer of the Uterus*.—Dr. SARGENT sent the specimen to the Society for their Cabinet, having observed that it contained none such. Dr. S. remarks that the tumour is rare, and that the usual mode of extirpation, by ligature, destroys the specimen. Dr. S. sent the following account: "I operated by the knife, arresting the hemorrhage by the actual cautery, and also by plugging the vagina with sponge filled with powdered alum. The patient was 47 years of age, and of irregular menstruation. She had had nine children, the last born five years ago. For six months, there had been unusual vaginal hemorrhage, which, for three months, had been slightly offensive, though the fetor has not been such as to be noticed by the friends. There had also been abundant watery discharges from the vagina. On examination, I found a long, cauliflower-form growth, springing from the upper lip of the os uteri (which latter was itself very much elongated and attenuated, but soft and healthy), and filling the whole vagina so as to present itself at the vulva. The os uteri was open, so that I could readily pass my finger into the uterus; the fundus of the organ was soft and natural; there was nothing abnormal in the feeling of the bladder or rectum; no glandular enlargement in the groins, nor tumours to be perceived in the abdomen, on palpation. Subsequent examination by the speculum showed the uterine tumour to be lobulated on its vulvar surface, and to be covered with an abundant jelly-like mucus.

The patient complained especially of weakness and of morbid excitement and wakefulness. All her functions were well performed. The tumour was increasing so fast that it would soon be troublesome by its bulk, while it was already exhausting the patient by the anxiety which it induced as well as by the discharges it occasioned; and I removed it, in the manner above stated, on the 14th of March. On the 7th of April the wound was nearly healed, and the patient was about her house and doing very well. The local treatment after the operation, and after suppuration was established, was by alum injections daily, with the occasional application of nitrate of silver.

Abscess of the Tibia: Trephining of the Bone, &c.—A section of the tibia upon which the above operation was done, was sent by Dr. SARGENT, and exhibited to the Society; it shows great thickening of the bone by reason of inflammation. Dr. S. operated last week upon the patient, a young man of 27 years, who had had periostitis of the left tibia, near the knee—perhaps owing to a blow—for eighteen years, at times with exfoliation, and, until quite recently, with little tumefaction. During this period of eighteen years, the patient had, however, been free from pain most of the time; he had been able to complete a collegiate and professional education, but had always been subject to seizures of severe pain in the region indicated, with soreness, and also with pain in the course of the sciatic nerve; there were also disturbance of the digestive system, and, occasionally, wandering neuralgic pains. Within the past year these attacks had been more frequent than before, and much more severe, so that the patient had been confined to the house much of his

time. The leg had become considerably flexed, and any extension or motion whatever was painful. For two months past the swelling had been gradually increasing, and obviously it was an enlargement of the *bone*, as well as a thickening of the periosteum; it was quite tender to the touch, and finally became œdematous over its surface. The pain was so severe that the patient often begged for amputation. The joint was entirely unaffected. General health good, except for disturbance by pain; no rigors. Dr. S. trephined the tibia April 23, 1854, perforating it just below and within the insertion of the ligament of the patella, and opened into an abscess, from which there issued perhaps two ounces of creamy pus. The periosteum over the seat of the disease was more than half an inch thick, and the bone, as seen in the specimen, greatly thickened. The patient had some pain following the operation, but has been entirely relieved since; he is gradually acquiring the power of motion with the limb.

Ovarian Disease, with Specimens, &c.—Sections of the diseased ovaries were sent by Dr. SARGENT, and were exhibited to the Society by the Secretary, who also read the following account of the patient's case, furnished by Dr. S.:—

A maiden lady, 34 years of age, first examined by Dr. S. in August, 1846 (aged, then, 26 years), was, at that time, ascertained to have the entire pelvis, as examined by the vagina and by the rectum, filled with lobulated disease, which did not seem to be of scirrhus hardness, nor to have invaded the os uteri. Dr. S. has examined her about once a year, since the first exploration, and has found the abdomen to be gradually filling up with a tumour which was not fluctuating, while the general health had remained tolerably good. It is now about two years since Dr. S. last saw her, living. Her health, this spring, had been as good as usual, so that she was occupied in domestic affairs, and had a good appetite up to the 3d of May, when she was induced to submit to an attempt at extirpation of the diseased mass by an itinerant surgeon, who told her that her chances of cure thereby were as three to one. She died three days after the operation, with symptoms of peritonitis. The *autopsy* was performed twenty hours after death. The integument of the depending parts of the body was much discoloured, and there were several ecchymoses, with bloody serum beneath the cuticle, about the lower part of the abdomen, which latter was as much distended as in pregnancy at the full term. An incision of about three inches in length had been made through the abdominal parietes, just at the right of the umbilicus, and a little below it, from which wound a bloody fluid was escaping. The abdomen had also been punctured a little below and to the right of this above-mentioned incision. On cutting through its walls, the whole cavity of the abdomen seemed to be filled with a large tumour, which covered the intestines, to which it was attached by some old adhesions, especially in the epigastrium and on the left side of the abdomen. There were also slight adhesions about the umbilicus, and some traces of pus and of recent lymph in the same region; but the general investment of the tumour presented a smooth serous surface, which was about two feet in every diameter, but divided, transversely, by a fissure, which was found afterwards to be between the two ovaries, which organs had contracted adhesions to each other that admitted of easy separation; the right ovary constituting the larger part of the mass, and rising so as to fill the right hypochondrium, the epigastrium, and the left hypochondrium, leaving only the space from the umbilicus to the left lumbar region, and to the left groin, for the other ovary. Along this said fissure the Fallopian tubes could be traced, each about eight inches long, with their fimbriated extremities open,

admitting a sound. The round ligaments of the uterus were also seen passing under the investment of the tumour, to the groins. The whole mass removed weighed $46\frac{1}{2}$ pounds. On dissection, the vagina was found to be elongated to seven inches; the os uteri almost obliterated by attenuation; the body of the uterus itself being elongated to fourteen inches, reaching upwards over the whole of that portion of the tumour which was afterwards found to be constituted by the left ovary, which itself presented a surface of about twelve inches square. The anterior wall of the uterus was quite thin, perhaps one-eighth of an inch in thickness, while the posterior wall was half an inch thick. The upper part of the uterus was filled with a coagulum; the uterus itself was found to be perforated by a trocar, which the "surgeon" above referred to had passed quite through both walls of the organ, prior to his capital operation; the puncture extended from the uterus into the left ovary, in and about which there was extensive ecchymosis (nearly a pint of blood was said to have flowed through the canula of the trocar, before its removal). The left ovary was about one foot square on its surface, and six inches in thickness; the right ovary was about one foot, cube. The structure of both was analogous, and throughout was like the specimens exhibited to the Society, and which are sections, one from each ovary. The *bladder* and *vagina* were healthy. *Right ureter* dilated to the size of a finger; and the pelvis of the *kidney* was so enlarged that the kidney itself was only a sac of urine. *Left kidney* perhaps three times the natural size, and unusually friable; the broken surface resembling that of the spleen, while the lining of its pelvis was dark, by distension of the venous capillaries. *Lungs* free from adhesions, excepting slight ones at their apices; their general surface dark, by venous congestion; no tubercles. Dr. S. adds, that it is understood that the "surgeon" gave up his operation because he found adhesions.

Dr. B. S. SHAW, who examined a portion of the specimen microscopically, pronounced its structure to be fibrous. No trace of cells visible.

Dr. SARGENT presented to the Society, with the above specimens, the *identical pitchfork*, upon the handle of which a lady was impaled to the extent of twenty-two inches, the handle entering the vagina. (See *Amer. Journ. of Med. Sciences*, Oct. 1853, p. 355.)

Irregularities in Measles.—Dr. PUTNAM referred to some cases of measles that he had reported some time since, showing that the irregularities in the disease had, during the present season, been unusually frequent. He further read the following history of three cases, communicated to the Society by Dr. JAMES JACKSON:—

The following is a case of idiosyncrasy in resisting the measles, which was finally overcome. It is the more remarkable, as it was transmitted to the offspring, and modified the disease in them. In regard to the facts, I should believe that some error might have occurred, had not the subjects of them belonged to my own family.

F. H. was exposed to measles in early childhood. He was with a sister who had the disease, and was himself unwell. I remarked, at the time, that he had the constitutional affection, so that if he had had any eruption, I should say he had had the disease. I do not now recall the particular symptoms showing the constitutional affection; but, at the time, I regarded them as being characteristic of the disease. Since that time, he has been exposed, more than once, to the measles, without any result as to himself.

He has two children. The oldest, a boy, was taken sick on the 31st of December, 1853, with a sore throat and febrile symptoms. We were looking

for the measles in him. He had not any affection of the eyes, nor of the nose. The affection of the throat was principally in the soft palate, at its margin, which was red, swollen, and painful. This continued five days, during which he kept mostly on a couch, but walked about occasionally, and had not much appetite. There were not at any time distinct spots on the palate, such as frequently precede and attend measles. On the fifth day he appeared better; but, after the morning, he discovered an eruption, which had the distinct characteristics of measles, about his elbows and about his knees, mostly on the latter. This subsided on the third day from its appearance, and left him well, though somewhat emaciated and enfeebled. In the two following days (the 8th and 9th of the disease), he convalesced rapidly; on the 10th and 11th, he walked abroad, and at his own request he went to school on the 12th, 13th, 14th, and 15th. On the 16th, he had a rheumatic pain, with some slight constitutional affection; the pain was less on the 17th, but the fever more; on the 18th, there came on an eruption of measles on his face and breast, which spread rapidly over the body. It was very full, and strongly marked in every respect. He was somewhat prostrated at this time, and coughed much. The eruption abated on the third day, and terminated on the fifth. From this time he convalesced rapidly.

It will be perceived that the full eruption occurred on the 19th of January, 1854.

On the 28th of January, the sister had an eruption of measles, preceded by a slight indisposition for two or three days, in which the eyes and nose were slightly affected, but the throat not at all. The eruption was on the face and breast, and afterwards on the other parts of the body, but was not full nor high coloured. The patient was on her feet much of the time. In the course of the fourth day of the eruption (January 31), when I looked for a decline of it, on the face at least, it began to increase in every respect, so that the next day it was very full and high coloured. The fever increased with this change, and on the next day it was quite severe; she was entirely prostrated; pulse very frequent; skin hot; and she had some delirium. She remained severely sick till the 5th of February, on which day the eruption faded, and from that day it subsided slowly. During this serious attack, the passages of the nose and one ear were very much affected, as sometimes happens in scarlatina. The convalescence was very slow, and the ear has not recovered entirely at this time, May 6.

On the 7th of February, the father was taken sick. He was chilly, and had a great feeling of soreness over the whole surface of his body. He did not lose his appetite, or only in a slight degree, and went abroad a little every day. At the same time, he had a sore throat. The tonsils were inflamed, and had white patches on them. The disease with the general symptoms continued till the 12th, when he was much better in every respect, and from that time the throat got well. But on the three days following the 12th, he felt sick again, and his countenance was much altered, and a loss of flesh was very manifest. On the 16th of February, he was much better, so that on the 17th, he went a journey of 200 miles into the country, which had been deferred on account of his sickness. He returned on the 20th, having encountered very cold weather, but felt very well. Yet on the night of his return, he discovered an eruption on his thighs and legs, though not on any other part of his body. On examination, the next day (21st February, and the fifteenth of his disease), I found that the eruption was that of the true measles. This continued four days, the general health not being disturbed.

There are here shown three cases of remarkable irregularity in measles, all

occurring in one family, a father and his two only children. Nothing of the kind had occurred in any of the progenitors of the father, so far as I know; and I probably should have heard of it, had it occurred among any of his relatives in the generation preceding him. The cases differ from each other as to their course, but in each of them the rash came out with difficulty, and in both the children it burst out with violence after it had made one effort to show itself. In the father, there seemed to have been an effort to produce the disease when he was exposed to it in his childhood. Again, while he was exposed to it in his children, in his thirty-ninth year, a new struggle took place, such that I said to him he was affected by measles without an eruption. Then, after complete recovery, and while much exposed in cold weather, the eruption came out, but limited to the lower extremities.

It is proper to add that the idiosyncrasy in these cases cannot be attributed to any feebleness of constitution. The father has always enjoyed good health, and so have his children.

ART. V.—*Observations on the Pathology of Cases of Yellow Fever, admitted into the Pennsylvania Hospital during the Summer of 1853.* By THOMAS HEWSON BACHE, M.D.

It will be observed, of the fourteen cases on which *post-mortem* examinations were made, that the head was examined in four only. It is, therefore, necessary to state, to prevent wrong conclusions, that, in cases dying without any symptoms which indicated the probability of any lesion of the brain, the calvarium was not removed.

CASE I.—Examined thirteen hours after death. The external surface was of a bronze-yellow colour over nearly the whole body, with bloody discoloration of the scrotum and all dependent parts; rigor mortis being well pronounced. The dura mater was tinged yellow, and the vessels of the membranes were congested with venous blood. The substance of the brain, however, appeared healthy. In the thorax, some slight adhesions of the left pleura were found, which were of a yellow tinge. Both cavities of the heart were full of blood. The cavity of the pericardium contained a fluidounce of yellow fluid, which was tested for bile, and found to contain it. The stomach was red and somewhat corrugated, and very much distended with a black fluid, in appearance similar to that which had been vomited before death. The small intestine contained a black fluid, which became more pasty and of a lighter colour as it was traced downwards, until, arriving at about three or four feet from the ileo-cæcal valve, where it ceased, it became of a lead colour. The liver was finely mottled with yellow and red spots. The microscope showed a great excess of oil-globules, not only in the secreting cells themselves, but floating freely in the field. The gall-bladder contained bile. On removing the right kidney, which was abnormally yellow, a large psoas abscess was found.

CASE II.—Examined sixteen hours after death. The general appearances of the external surface, as well as of the internal organs, were, with the exception of the gall-bladder containing no bile, and both intestines being contracted, the same as in Case 1.

CASE III.—Examined eleven hours after death. General appearances of the surface as in the above. The right pleural cavity contained half a fluid-ounce of orange-yellow fluid. The pericardium was injected. The liver, though darker in colour than in Case 1, was, like it, finely mottled. The stomach contained a black fluid, with some gas. Its peritoneal covering was congested, as well as its mucous surface; the latter more especially at the cardiac orifice. It showed, however, no sign of softening.

CASE IV.—Examined ten hours after death. A little fluid was found in both pleural cavities, and the pericardium was of a decided orange-yellow colour. The stomach was injected at the cardiac orifice, its contents being the same as in Case 4. Its mucous membrane, however, was softened, and, in one place near the larger extremity, ulcerated; the ulcerated places being of a black colour. This case was the first in which the œsophagus was carefully noted; and this tube was found reddened, and denuded of its epithelial covering. The liver was tawny, with fine brown and red spots, and the gall-bladder nearly full of rich brown bile. The small intestine contained the same fluid observed in Case 1. The mucous membrane of the duodenum, for six inches, was red with effused blood, for six inches more, was covered with spots like ecchymoses; similar ones being scattered, here and there, down the intestine, to within two feet of the ileo-cæcal valve. The large intestine also presented some spots at its commencement, and was filled with a thick slate-coloured fluid.

CASE V.—Examined eleven hours after death. External surface as in the preceding cases. This patient having died suddenly with delirium, a few hours after his admission, the head was examined. Nothing was presented but venous congestion of the membranes of the brain, with the dura mater more tightly adherent to the calvarium than usual. This may have been occasioned by the Pacchionian glands, which were unusually large. The substance of the brain showed no alteration. The mucous membrane of the large bronchial tubes was highly congested, though the lungs themselves presented no lesions. The pericardium contained half a fluid-ounce of bloody fluid, with a small black clot. The right cavity of the heart contained a fibrinous clot, whereas the left was empty. The lining membrane of the aorta was very yellow. The stomach contained a dark coffee-coloured fluid. Its mucous membrane was not softened, and was, for the most part, of a leaden colour; still, both the greater and lesser curvatures were red, like a dysentery gut. The liver was reddish yellow and mottled. The mucous membrane of both the small and large intestines was of a lead or slate colour, and filled with a fluid of the same hue, but was free from ulceration.

CASE VI.—Examined fifteen hours after death. The external surface was not exactly as in the other cases; probably because the examination was made after a longer interval from the time of death than in any of the preceding. The skin was of a much brighter yellow, and the dependent parts, though somewhat purple, had not the dark bronze hue so often seen. This, perhaps, may be accounted for by the fact that this patient died at a very late stage of his disease, or rather from its after-effects, and not, as commonly, in the third stage (*i. e.* collapse); his attack assuming a protracted typhoid form. The pericardium contained as much as four fluid-ounces of yellowish-brown fluid, but exhibited no evidence of inflammation. The mucous membrane of the stomach showed signs of old inflammation, being of a mottled slate colour, the greater extremity of a brownish-green, and the pylorus red. It contained no black fluid,

but a fluidounce and a half of dark thick greenish matter. The whole œsophagus was in a state of sphacelation. The liver, with the exception of being soft and flabby, with the gall-bladder full of bile, did not differ from those observed in the preceding cases. The duodenum was inflamed; some of Peyer's patches were unusually distinct, and in their vicinity the gut was injected. The colon was much inflamed, particularly at its head; and near the sigmoid flexure there was a fibrinous patch.

CASE VII.—Examined nine and a half hours after death. The pericardium contained about two fluidounces of gamboge-coloured fluid; whereas that of the pleura was bloody. The bronchial tubes were injected. The aorta and pulmonary artery presented a buff colour. The liver was different from those of the preceding cases, being of a pale-yellow or tawny colour, and the gall-bladder of a dark-red hue. The stomach showed signs of inflammation, as did also the intestines; the latter being of a slate-green colour. The contents of the former were very dark-brown, but, by transmitted light, of a claret colour.

CASES VIII. and IX. presented similar lesions to those already noted. It is interesting to mention, however, that, in one of these cases, the larger biliary ducts were dissected out, and found to contain bile to all appearance healthy.

CASE X.—Examined nine hours after death. This case is one of peculiar interest, from the fact that the pericardium contained four fluidounces of a very turbid greenish-brown fluid, resembling black vomit, which, being poured into a bottle and allowed to stand a short time, separated into two portions; that at the bottom of the bottle being of a white or pale-yellow, while the rest remained of the original colour. An examination by the microscope proved the brownish-black fluid to be altered blood-corpuscles, with less of the granular amorphous matter than is usually found in the genuine black vomit. These blood-corpuscles were not as much altered as they generally are, when they constitute one of the elements of black vomit. No epithelial scales could be detected, although it was natural to suppose that they were present; since it is difficult to understand how a hemorrhage could occur from a serous surface without a loss of some of its epithelial covering. The whitish fluid was pus. The heart itself was stained of a dark colour at its base, and the pericardium was minutely injected in points. The stomach was uniformly red over all its surface, except at the cardiac orifice. Its centre presented a number of black spots of various sizes, like ecchymoses, and two ulcerated fissures, each about an inch long, filled with black matter, each of which corresponded to the track of a vein, found on the peritoneal side of the organ. The liver presented nothing different from the same viscus in the other cases.

CASE XI.—Examined sixteen hours after death. In this case, the head was examined. All the membranes were injected and thickened; the hemispheres being partially adherent, and the right lateral ventricle containing some fluid. The other appearances were in no respect different from those in the other cases; except that the fluid found in the stomach (five fluidounces) was not black, but of a chocolate colour, and that half of the mucous membrane towards the pylorus was of a slate colour, the other half red.

CASE XII.—Examined fifteen hours after death, on the same morning that the preceding case was inspected; the organs of the two cases being compared. The stomach contained as much as a pint and a half of thick black fluid. Its mu-

cous membrane presented the same colours as in the preceding cases; the slate colour, however, being towards the greater extremity. The appearance of the liver, intestines, &c., was the same as in the other cases.

CASE XIII.—Examined fourteen hours after death. The only difference worthy of notice was, that, although the stomach much resembled that of Case 10, having one fissure more than an inch and a half long, yet no vessel could be detected to correspond to it.

CASE XIV.—Examined sixteen hours after death. This patient died in convulsions. The dura mater was much congested, some effusion being under it. The superior part of the arachnoid presented a number of opaque spots. The vessels of the pia mater were congested. The right lateral ventricle contained two fluidrachms of fluid; the membrane lining it, as well as the choroid plexus, being congested. The substance of the brain appeared normal; although, when cut, the points which indicate its vessels presented a black instead of a red colour. The lesions of the other organs were very analogous to those found in the preceding cases.

In reviewing the lesions presented in the above cases, those of the most interest, certainly, are the great excess of oil, found in the liver of all the fourteen cases by the aid of the microscope; and the fluid, resembling black vomit, observed in the pericardium of Case 10. Budd, in his work on diseases of the liver, makes some allusion to the probability of acute disease inducing fatty degeneration of that organ; for, at page 300 of the second edition, he says: "In all cases in which I have yet ascribed fatty degeneration of the liver to local causes affecting the nutrition of the part, the accumulation of the fat has been partial. It may be, however, that the entire organ may be damaged by some acute disease, or in other ways, and may become fatty in consequence. I strongly suspect that this happens in yellow fever, and in the severe bilious remittents of tropical climates." Dr. A. Clark, in a paper contained in the *New York Medical Times*, for May, 1853, called attention to the microscopical examination of the liver of a patient who had died of yellow fever; it having demonstrated "a fatty state of all the secreting epithelial cells, and an abundance of free fat-globules." He expresses a doubt as to whether the lesion is peculiar to yellow fever; as, in the case in which he made the observation, the liver may have been fatty before the attack of the fatal disease; and he puts the question to those who may have the opportunity of observing, "Is not the change so constantly observed in the livers of those dying of yellow fever, an acute fatty degeneration?" The paper of Dr. Clark is not now by us to refer to, and, consequently, we cannot compare his observations with those made in the fourteen fatal cases occurring at the Pennsylvania Hospital. However, in order that others may do so, we will describe the microscopical appearances of the liver in the hospital cases. The secreting cells were pale, ill-defined, and less granular than when in the normal state. In the cells, with few exceptions, no nucleus could be detected, but its place was supplied by a single oil-globule. This was observed even in those cases in which the granular part

of the cells was not so full of oil as in some others. Generally, the cells were so studded with oil-globules as to give one the idea of looking at a number of these latter, which had by chance become agglomerated or entangled by granular matter, leading to the conclusion that the cells were broken down. I am very sure we at first mistook some diseased cells for such oil-globules. Nor did the oil-globules confine themselves merely to the granular part of the cells and their nuclei; but they were found floating freely, of various sizes, all over the field of the microscope.

These appearances were not only seen at the Pennsylvania Hospital, where they were frequently shown to physicians curious in such matters; but specimens of the morbid structure were sent to others familiar with the use of the microscope, and its application to pathology, who, in every case, confirmed the above conclusions. Moreover, several specimens, taken from the livers of persons who had fallen victims to the epidemic, and had not been admitted into the Pennsylvania Hospital, were sent there for examination, and found to present the same pathological appearances. From the above cases, are we not warranted in concluding that the liver of persons dying of yellow fever is a fatty liver? Indeed, may not the morbid change be called, as Dr. Clark has called it, "an acute fatty degeneration?"

Some may be curious to know whether the microscope shows any difference between the fatty degeneration above alluded to, and that occurring in the drunkard's liver. This question we have endeavoured to solve, and, from a careful examination of a limited number of specimens taken from persons of intemperate habits, must declare that we have not detected any.

Louis, in his work on the yellow fever of 1828, at Gibraltar, page 162, describes the liver as being "sometimes of the colour of fresh butter, sometimes of a straw-yellow, a clear coffee-and-milk colour, sometimes of a gum-yellow, sometimes of an orange colour." We saw no livers of the colour of fresh butter, and only two or three of a clear coffee-and-milk colour; one of which was brought from the St. Joseph's Hospital, and, therefore, is not enumerated in the fourteen fatal cases we have tabulated. These livers seemed to have little or no blood in them, and were familiarly called by us the anemic liver. As a general rule, the livers we saw were of an orange-yellow colour, sometimes gamboge-coloured on the cut surfaces, finely granular, and becoming redder by exposure to the air.

The contents of the stomach, as well as many specimens of black vomit, taken at various times from different patients, were carefully examined by the microscope, and found to contain—

1. Amorphous masses of coagulated mucus or serum, and the debris of blood-corpuscles, which gave the mass a red or brown colour.
2. Numerous small irregular masses, sometimes presenting an imperfect crystalline form (probably altered hematin), which were of different shades, from a reddish-brown to a deep-black colour.

3. Red blood-corpuscles, in different stages of alteration, which did not correspond to the degree of acidity of the vomit.

4. Various forms of epithelial cells, as the squamous and conoidal. In one specimen, a perfect mucous crypt was observed.

5. Brown oil-globules, some of which bristled with fine crystals of margaric acid.

Besides these constant elements, there were some occasional ones, such as the sarcina ventriculi and the torula, the former more frequently than the latter, and some inflammation corpuscles.

Although the preceding cases, with the exception of the last two, occupied the medical wards of the Pennsylvania Hospital, during my term of service in those wards as resident physician; yet it is right to mention that Drs. J. E. Rhoads, James Darrach, and J. Green, also residing in the hospital in the same capacity, but having charge of the surgical wards, took part in the above investigations, and aided materially to give them whatever value they may possess. To the first-named gentleman, particularly, I am indebted for the notes taken during the microscopical investigations of the morbid specimens.

The fatal cases, 15 in number, were all examined with but one exception.

Summary of the Post-mortem Appearances.

<i>Generalities.</i> —Examination made within 11 hours after death in	6 cases.
“ “ 14 “	2 “
“ “ 17 “	6 “

<i>External Appearances.</i> —Yellow of various shades in	14 cases.
Bloody discoloration of dependent parts (no observation in 1) in	13 “
Rigor mortis (no observation made in 5) in	9 “
Conjunctiva yellow (no observation made in 8) in	6 “

<i>The Head</i> was examined in	4 cases.
Pachionian bodies very prominent in	2 “
Dura mater firmly adherent to calvarium in	2 “
“ congested in	3 “
“ thickened in	1 case.
“ tinged yellow in	2 cases.
“ had effusion under it in	1 case.
Arachnoid opaque in	1 “
Pia mater congested in	3 cases.
“ thickened and opaque in	1 case.
Hemispheres partially adherent in	1 “
Structure of brain normal in	4 cases.
Effusion into the ventricles in	2 “

<i>The Lungs</i> were examined in	13 cases.
Structure normal in	7 “
Old adhesions (which were frequently tinged yellow) in	5 “
General congestion posteriorly in	2 “
Congested in spots at posterior portion (ecchymoses) in	5 “
Effusion of serum in	3 “
“ blood in	1 case.

<i>The Trachea and Bronchial Tubes</i> were examined in	8 cases.
Injected in	8 “

<i>The Pericardium</i> was examined in	11 cases.
Normal in	7 “

Tinged yellow in	1 case.
Injected in	3 cases.
“ in spots in	1 case.
Effusion of yellow or orange-brown fluid in	9 cases.
“ blood with clot in	1 case.
“ dark greenish-brown fluid in	1 “
<i>The Heart</i> was examined in	12 cases.
Tissue normal in	7 “
Right cavity distended with blood in	2 “
“ containing a clot in	7 “
Left cavity distended with blood in	3 “
“ containing a clot in	6 “
Large in	3 “
Small in	1 case.
Pale in	2 cases.
Black ecchymoses at base in	1 case.
<i>The Aorta</i> was examined in	8 cases.
Lining membrane yellow of different shades in	3 “
“ buff in	2 “
“ red of different shades in	3 “
<i>The Pulmonary Artery</i> was noted in	4 cases.
Lining membrane pale-yellow in	1 case.
“ yellowish-pink in	1 “
“ buff in	2 cases.
<i>The Stomach</i> was noted in	14 cases.
Peritoneal covering congested in	3 “
“ “ ecchymosed in	1 case.
Contained black fluid and gas in	7 cases.
“ dark coffee fluid in	1 case.
“ deep chocolate in	5 cases.
“ thick dark-green fluid in	1 case.
Mucous membrane slate-coloured in	3 cases.
“ dark-green in	1 case.
“ softened in	2 cases.
“ corrugated in	2 “
“ uniformly red in	4 “
“ yellowish-red in	1 case.
“ spotted red in	1 “
“ “ black in	4 cases.
“ “ brown in	1 case.
“ mottled yellow, green, and slate in	1 “
“ with linear depressions like ulceration in	3 cases.
“ of cardiac extremity red in	2 “
“ “ smooth and spotted red in	2 “
“ “ “ ulcerated in	1 case.
“ “ “ ecchymosed in	1 “
“ “ “ brownish-green in	1 “
“ “ “ slate-coloured in	1 “
“ “ “ softened in	1 “
“ of pyloric extremity red in	2 cases.
“ “ “ slate-coloured in	1 case.
“ “ “ rough in	1 “
“ of greater curvature red in	1 “
“ “ “ ecchymosed in	1 “
“ of lesser curvature red in	1 “
“ “ “ spotted red in	1 “

<i>The Oesophagus</i> was noted in	9 cases.
Mucous membrane injected in	4 “
“ slate-coloured in	1 case.
“ spotted red in	2 cases.
“ softened in	1 case.
“ spotted and striped black in	2 cases.
“ partially denuded of epithelium in	9 “
“ sphacelated in	1 case.
<i>The Liver</i> was noted in	14 cases.
Size normal in	12 “
“ small in	1 case.
“ large in	1 “
Consistence normal in	13 cases.
“ soft in	1 case.
External surface yellow of various shades in	2 cases.
“ “ and bronzed in	2 “
“ “ with ecchymosed spots in	1 case.
“ mottled yellow and red in	7 cases.
“ “ brown in	1 case.
“ “ purple in	1 “
Superior surface pale-yellow in	1 “
“ yellowish-green in	1 “
“ mottled yellow and red in	2 cases.
Inferior surface bronzed in	2 “
“ at anterior portion bronzed in	2 “
“ at posterior portion yellow in	1 case.
“ “ orange-red in	1 “
Right lobe, superior surface, spots of ecchymosis in	1 “
“ “ striped green in	1 “
“ inferior surface, greenish-brown in	1 “
“ “ orange-red and green in	1 “
Internal tissue gamboge-coloured in	1 “
“ mottled yellow and orange-red in	12 cases.
“ pale-yellow in	1 case.
Hepatic vein congested in	2 cases.
“ not congested in	2 “
Portal vein congested in	3 “
“ not congested in	2 “
Large hepatic duct contained bile in	2 “
Microscope showed oil in cells in	14 “
<i>The Gall-bladder</i> was noted in	12 cases.
Contained bile of various shades in	11 “
Empty in	1 case.
<i>The Small Intestine</i> was noted in	12 cases.
Contracted in	1 case.
External surface yellow in	1 “
“ very dark in	1 “
Contained black fluid in	1 “
“ gas and slate-coloured fluid in	4 cases.
Mucous membrane reddened in	3 “
“ yellow in	1 case.
“ slate-coloured in	3 cases.
“ not softened in	12 “
“ having ecchymoses in	3 “
“ mottled red and slate-colour in	1 case.
“ of duodenum reddened in	2 cases.
“ of dark purple-red in	1 case.
“ at ileo-cæcal valve red in	1 “
Peyer's patches unusually distinct in	1 “

<i>The Large Intestine</i> was noted in	11 cases.
External surface very dark in	1 case.
Contracted in	1 "
Contained gas and slate-coloured fluid in	3 cases.
" black tarry matter in	1 case.
" slimy pink matter in	1 "
Mucous membrane reddened in	2 cases.
" reddened in spots in	1 case.
" slate-coloured in	3 cases.
" not softened in	12 "
" near ileo-cæcal valve ecchymosed in	1 case.
" at sigmoid flexure had a patch of fibrin in	1 "
<i>The Spleen</i> was noted in	10 cases.
Normal in	6 "
Hardened in	1 case.
Enlarged and hardened in	1 "
" softened in	2 cases.
<i>The Kidneys</i> were noted in	13 cases.
Normal in	4 "
Pale-yellow in	4 "
Yellowish-red in	1 case.
Congested in	3 cases.
Enlarged in	1 case.
<i>The Bladder</i> was noted in	6 cases.
Contained urine in	4 "
Empty in	2 "

ART. VI.—*Case of very extensive Fistula, successfully treated by a New Method.* By HORATIO G. JAMESON, Sen., M. D., formerly of Baltimore, Maryland, now of Philadelphia.

A GENTLEMAN of Prince's County, Maryland, was afflicted with a fistulous opening, which had been discharging freely for several years. Surgeon G. S. Pattison was sent for, and attempted to cure the case by laying open the sinus freely. An incision was made about thirteen inches in length, and four in depth, where it passed through the gluteal muscles. Some arteries were tied, and the wound dressed by putting in what sponges had been thought necessary for the purpose; but such was the extent of the wound, that six napkins were added to the sponges to fill it.

The next morning hemorrhage occurred, and it became necessary to remove the dressings; some more arteries were tied, but a vast deal of blood was lost, so that the patient was pulseless, or nearly so, for twenty-four hours, though plied freely with wine, brandy, and whatever it was thought would tend to stimulate and sustain. His life, it was supposed, was, so to speak, for several hours suspended on a hair. Judging by the cicatrix, I have never seen a wound of such extent, either from casualty or the surgeon's knife, but was thereby forcibly reminded of the bold and successful operation by John Bell, for tying a wounded artery at the ischiatic notch. Several weeks served to heal the wound made to cure the fistula, but the gentleman was now greatly disappointed to find his case in no degree improved.

His health reasonably restored, he put himself under my care in Baltimore.

Inquiring into the history of the case, Mr. S. informed me that he had no disease about the rectum or anus for many years; but that, about sixteen years previously, there had been an affection of those parts; but he was not at all aware, at that time, of anything amiss except in his thigh. By examination of the rectum, I soon found a sensible depression at a point directly opposite to the upper end of the sinus outside. It was firm and glossy to the touch, and would have held about the half of a musket-ball. In this depository, a passage had been formed for the matter to escape to the thigh. The true nature of the case is ascertained, but where is the remedy? John Bell's operation, two feet long, and down to the ischiatic notch, will not answer, for the head of the sinus is inside. I had seen that, if sinuses are laid open, they would heal—then, why? Because their surface is broken up, and an inflammatory action set up, which led to adhesion, and adhesion was the cure. So here; I hoped that, if I could by any means abrade the surface, and excite a low degree of inflammation, I might hope to cure. But, here is a sinus ten or eleven inches in length, turning off nearly at a right angle, deep down among firm parts, and having to rise somewhat up into the pelvis.

With a view of fulfilling these indications, I procured a steel probe of spring temper, of the proper length, and furnished with a button point, and an eye at the other end for carrying a thread; from the point it was gradually tapering, so that it would bend most easily for the space of more than an inch; this probe was armed with a strong flax thread. The surface of the sinus being callous, I depended upon the blunt point keeping in the canal, while pressure at the lower end of the probe would cause it to bend where it was weaker than the other part of it. It may seem strange to say that I spent nearly an hour in accomplishing my purpose; eventually the probe entered the rectum, and now all was safe. A strong thread being tied to the eye of the probe, the latter was drawn out at the rectum, and the thread drawn through the sinus and rectum.

A strong thread was now armed with small shot, the way boys put sinkers on their fishing-lines; the thread was strung with the shot at the distance of three-fourths of an inch. The reader is no doubt aware that these shot, being slit open to receive the line, and then closed in again so as to fasten the line, become very rough. This shotted line being tied to the first, was now drawn in by drawing the other out at the anus. It produced some irritation and slight inflammation, but not much pain; free suppuration soon took place. The patient was restricted in diet, and took some light physic. Perceiving that the shot, after some weeks, were becoming tighter in the sinus, and the gentleman being anxious to go home, I provided a gold wire, which, being attached to the shotted line, was drawn in as the other was drawn out. In a few months there was a perfect cure without any confinement or suffering, and without any remedy except the wire, which, being of gold, there was no fear of its rusting or breaking; and the two ends being tied on the thigh at the lower end of the sinus, the patient could attend to his ordinary business without hinderance.

A girl, about ten years of age, of scrofulous habit, was put under my care for a fistulous affection of the thigh, a little above the middle; it discharged freely, was painful, causing great lameness. The usual remedies were used for the constitutional symptoms, and various injections made use of for some months with but little benefit. I had recourse to an iron spring, made by flattening the wire, and leaving the edges rough as they came from the hammer. The sinus was about six inches in length, and the spring suited to the

sore, but allowing room to push the head of the spring into the orifice of the sinus; and it was provided with a short string, by which the spring could at any time be removed. This is necessary once in two or three weeks whenever iron is used, lest it rust, and break off in the canal. The spring must be so graduated, as to the size of the wire, as that it shall have strength to keep straight, and press slightly against the sides of the sinus, but not so as to cut in and cause pain; for this case, the wire used was perhaps one size larger than what is called bonnet-wire—this is the form of it:—



I have seen an unfortunate woman, who was operated on for fistula in perineo, wherein the partition between the vagina and rectum was split open, and so it ever remained for many years. Soon after I had seen this case, a case presented itself in a respectable poor woman, who attended market for a living. It gave her much pain, with annoying discharges, which admitted hardly a possibility of cleanliness. Long walks, and heavy loads, were her daily task. The usual operation could only have been effected by her laying up in some charitable institution, and a confinement of several weeks. I adopted the method by the iron spring. The spring was introduced and worn about four months, when, being withdrawn, she was found to be entirely freed from the disease, and sound and healthy in the parts. She never lost a day, never made a complaint, walked with loads, and stood long at her stall; never took a dose of medicine. The spring was cleaned once in three or four weeks. I weighed one of the springs, it weighed between nine and ten grains. We have heard of iron being much more valuable than gold in the form of hair-springs for watches, but if ten grains of wire can be made to cure troublesome and dangerous fistulæ, what price shall we set upon it? These are only specimens of what has been done, and what I hope may yet follow. These are made by flattening what is called thread wire with the hammer; we must not make the head too large, so that we can push the head through the orifice of the sinus. They should have heads of different sizes, to suit the size of the orifice. We have cured, by this means, fistulæ in other parts of the body—fistula in ano among them.

ART. VII.—*Cases of False Anchylosis of the Lower Jaw.*

By C. S. Fenner, M. D., of Memphis, Tenn.

CASE I.—Thomas Frank, a young man eighteen years of age, came to me in July, 1851, with false anchylosis of the lower jaw; the result of extensive sloughing, caused by salivation six years previously. On an examination, I found the jaws so completely closed that a thin knife-blade would not pass between the teeth; and no force that we could apply produced the slightest depression of the inferior maxillary. There was a very slight lateral motion, just sufficient to be perceptible, from which I inferred there was no osseous deposit about the joint. The condyles of the inferior maxillary bone rested in the anterior portion of the glenoid cavity, causing the inferior incisors to project beyond those of the upper jaw, leaving a small space through which he passed

his food. On the left side, a firm cartilaginous substance commenced just within the angle of the mouth, extending backwards, occupied the entire place of the masseter muscle; involving the gums and mucous membrane, and embracing the teeth so firmly that a probe would not pass backwards. He had previously submitted to two operations with the knife, but without benefit from either. He had also been subjected to the long-continued action of the screw, without any satisfactory result. I thought he could be relieved by an operation, and advised him accordingly. He was willing to submit to anything that promised the least prospect of success, and expressed a wish to die under the operation, if it should fail to relieve him from his truly pitiable condition. I prepared myself for the operation after the manner recommended by Dr. Mott, and others, to use a large amount of force to separate the jaws after the ligamentous bands had been freely divided with the knife. The patient was placed in a recumbent position, and chloroform administered. I then placed a scalpel (having been previously wrapped to within an inch of the point to avoid cutting the angle of the mouth), with the back resting against the teeth, and pushed it backwards to a point about an inch above the angle of the jaw, continuing the incision as the knife was withdrawn, until the fibro-cartilaginous ligament was completely divided. The finger now easily passed back to the ramus of the jaw. The knife was laid aside, and as much force applied, to depress the jaw, as we thought the bone capable of sustaining; but without effect. Upon further examination, this same ligamentous formation was found to occupy all the space back of the alveolar processes, and between the coronoid process and pterygoid process of the sphenoid bone; still forming a powerful bond of union to keep the jaws together. I determined to divide this. The point of the scalpel was placed immediately back of the last molar, with its edge looking upward and backwards; the flat surface rested against the anterior edge of the coronoid process, and pushed it forwards to the pterygoid process, carrying the incision upwards along the neck of the bone. With the left hand placed on the chin, the jaw was felt to yield at each stroke of the knife, until the separation was complete. This was accomplished without the application of any more force than the simple weight of the hand resting on the chin.

The last part of the operation was attended with some difficulty, owing to its being necessary to keep the patient's mouth in a dependent position, to allow the escape of blood, which also obscured the parts from view; and before the jaws were separated it was difficult to use the finger of the left hand as a director for the knife. A wedge-shaped piece of cork was now placed between the teeth to keep the jaws apart, and the operation was completed, proving entirely successful.

CASE II.—Miss Robinson, eleven years of age, had been similarly afflicted for two years, from the same cause, the disease occupying the same side as in the above-described case; but here the lower incisors closed behind those of the upper jaw. This case was operated on in the same manner as described above. During the latter stage of the operation, the jaws separated at each incision of the scalpel, until they opened sufficiently wide for all useful purposes. After two or three weeks there was a strong tendency to contractions of the divided parts to their original position. This, however, was entirely overcome by perseverance, using the jaws freely in mastication, and keeping them for a considerable time widely separated by substances forced between the teeth. A thick piece of caoutchouc, doubled on itself and placed between the molar teeth, was found to accomplish this object admirably, without fatiguing the muscles.

Remarks.—In all the successful operations for false ankylosis of the lower jaw that I have seen reported, great force has been applied to separate the jaws after the knife has been laid aside. In Dr. Mott's case, reported in this *Journal* for Nov. 1829, p. 102, the screw was brought to bear on the parts, and a report followed like the "laceration of ligaments;" this was undoubtedly caused by the tearing asunder of the cartilaginous bond of union immediately back of the alveolar processes. That there was no bony deposition, is evident from the fact that slight lateral motion existed. Similar motion could be produced in both of the cases I have described, and the freedom with which the patients could use the parts immediately after the operation, shows there could have been no bony deposit about the joint.

In the only case of true ankylosis of the lower jaw I have met with, the jaws were slightly separated, but no motion whatever could be produced in any direction, although no unnatural ligamentous bands existed. Dr. Sims, several years since, reported in *The New Orleans Medical Journal*, a case similar to those I have above described, in which he succeeded in separating the jaws after his incisions were made, by means of levers of wood introduced between the teeth. I believe that, in both Drs. Mott and Sims's cases, success would have been much more easily attained, and equally as satisfactory, had they completed their operations with the knife alone. It is certainly easier, and much less painful to the patient, to divide a firm cartilaginous bond of union with a sharp knife, than to tear it asunder by the application of powerful force. Cases frequently occur, in which the cicatrix does not extend back as far as the coronoid process. I operated, in November last, on a young man, for immobility of the lower jaw, where the false ligament occupied both sides, but in neither did it reach further back than the second molar tooth. I divided the cicatrices on both sides, and free motion of the jaw followed. The operation in such cases is much more simple than it is when the parts are involved back of the alveolar processes.

ART. VIII.—*A Case of Hydrometra.* By LEWIS SHANKS, M. D., Professor of Obstetrics, in the Memphis Medical College.

THE subject of this case, Mrs. W., was about fifty-three years of age, sanguineous temperament, tall and rather slender, of more than ordinary intelligence and physical energy; she had given birth to, and raised, ten children. The youngest was born in 1841, twelve years since; soon after which she lost her husband, and remained a widow six or seven years. She has been married to her present husband five or six years. Menstruation ceased at the age of forty-seven or forty-eight, about five years since. Previous to, and for three years after the cessation of her menstrual periods, her health was good.

She was attacked two years ago with an acute bowel affection of a dysen-

teric character, which became chronic and protracted, and, as she supposed, originated uterine disease.

The first symptoms of the disease of the uterus supervened upon the chronic dysentery, and consisted of a tumour in the lower part of the abdomen.

This uterine tumour, though somewhat sensitive upon pressure, did not produce for months, much inconvenience, either from its size or tenderness. Twelve months since, however, she was induced to consult her medical attendant, and subsequently several physicians. Different opinions having been expressed to her, as to the organs involved, and their true pathological state, and the enlargement of the abdomen having increased so much as to make her condition very uncomfortable, she came to the city for the purpose of consulting, and putting herself under the treatment, of Dr. Fruyser and myself.

Upon a careful examination of her condition, and the history of her case, we were satisfied that the great enlargement of the abdomen was produced by the expanded uterus, and that the large amount of fluid in the uterus was contained either in its proper cavity—the internal opening of the cervix being occluded—or in a large intra-uterine cyst, which expanded the organ. This diagnosis was made from the very distinct abdominal fluctuation produced by palpation, and from the expanded condition of the cervix and lower segment of the uterus, ascertained by the vaginal and rectal examination. Though the enlarged and expanded state of the lower portion of the uterus was certainly ascertained to constitute the lower portion of the *great* tumour which filled the abdomen; there was so much hypertrophy and induration of this portion of the uterine walls, that no distinct fluctuation at the point of vaginal touch could be produced by abdominal palpation. The os was low down in the pelvis, and could readily be reached, above the posterior commissure of the vulva; and though the walls of the cervix were abruptly expanded and greatly consolidated, the first phalanx of the index finger could readily be introduced into the os.

Having made this diagnosis of the case, it was decided that the occluded cervix, or the cyst within the cavity of the womb, should be opened the next day, and the contained fluid drawn off, as there was danger, from the great distension of the abdomen, of a rupture of the womb. There was no ulceration of the os, no ichorous or offensive discharge, indicating either concealed ulceration or malignant disease, though the induration and thickening of the walls of the cervix were unusually great, and, to the touch, of almost cartilaginous hardness.

February 6, 1854. After an unsuccessful effort to introduce a common-sized metallic bougie, and different-sized catheters, I resorted to the common-sized uterine porte-caustique. After bending the end of the staff, which projected an inch and a half through the canula, so as to enable me to push it upwards and forwards behind the pubes, in the direction of the axis of the cervix, and toward the centre of the tumour; I succeeded, by using a moderate degree of force, in passing it two inches into the cervix; then meeting with elastic resistance, produced by the cyst, I forced the point of the staff in the direction of the centre of the tumour, through the cyst into its cavity; the canula was then pushed into the cavity of the cyst, and the staff withdrawn.

Two ounces of a thick and gelatinous fluid, of a brownish colour, like honey, were evacuated. The staff was again introduced through the canula, and pushed into the large cyst, when eighteen pints of sero-sanguinolent fluid were drawn off without further difficulty.

The hypertrophied walls of the cervix were so consolidated as to nearly

close the opening or channel through the neck, and to embrace firmly the canula, though not larger than a small-sized catheter. This narrow channel from the os, through the cervix to the cavity, was two inches or more, and clasped the canula so tight as to require some force for its withdrawal; and the density of the cyst was rendered obvious by the very perceptible jerk produced by its walls slipping over the end of the canula when it was withdrawn.

After the water was discharged, the hypertrophied and indurated state of the neck and lower segment of the body of the uterus was more manifest and better defined. The general structure of the uterus was soft and flabby, and remained uncontracted; but the thickened and indurated neck and lower segment of the body projected up on the sides in the iliac fossa, so as to form on the lateral uterine walls, a distinct circular ridge, like the sides of a bowl. This indurated portion of the womb was attached by adhesive inflammation to the pubes and other surrounding parts, so as to fix the womb firmly in its position. The chief pain and soreness, during the progress of the disease, and at the time of the operation, was in this indurated portion and the surrounding tissues to which it was attached. For several weeks before the operation, the great distension of the womb and enlargement of the abdomen, not only made her constantly uncomfortable, but disqualified her from turning in bed, without raising herself up so as to prevent the dragging and pain produced by it in the lower portion of the tumour and its surrounding parts.

After the womb was evacuated she was much more comfortable, and continued so until the fluid accumulated again. Though quietude, laxatives, and alteratives were instituted to prevent inflammation, and the accumulation of the fluid, in a month she was again so much enlarged as to require another operation.

On the 7th of March, I drew off, in the same way as by the first operation, fourteen pints of fluid. Though not so large as before, her stomach and general health were more impaired. The fluid presented more the appearance of an admixture of pus and mucus, or albuminous matter with the serum, being thicker and more tenacious. When the cyst was evacuated, I injected through the canula about 20 oz. of water, with $\mathfrak{z}\text{ij}$ of tr. of iodine added to it. This was agitated in the sac a few minutes and then withdrawn.

As a general course of treatment, she was then directed to wear a tight flannel abdominal bandage, twice a day to paint the hypogastric and iliac regions with the tr. of iodine; to take at bedtime, as an alterative and topic, a pill containing proto-iod. hydrarg., extr. colocynth. comp., extr. cinchon., each, a grain, and 8 drops of syr. ferri iod., three times a day. Under this course her general health improved, and the fluid accumulated much slower.

April 16. Six weeks since the last operation; she came in her carriage from her home, about twenty miles, to the city. Though the womb was very much distended again, her general health was much better. Being desirous to try the effect of Bailey's Spring, near Tusculumbia, Ala., a water of much celebrity in dropsical cases; to prepare her for her journey, and for the more favourable action of the medicinal water, I drew off eighteen pints of fluid again on the 17th of April. It presented less appearance of the admixture of pus, or mucus with the serum, than at any previous operation. I injected again about $\mathfrak{z}\text{xx}$ of water with $\mathfrak{z}\text{iv}$ of tr. of iod. This produced a slight diffused sensation of burning in the cavity of the sac, which soon passed off, and she felt very comfortable after it.

The second and third operations indicated less consolidation and contraction in the cervix, but the dense membranous cyst was more obvious and resisting

to the blunt end of the porte-caustique staff, requiring considerable force to puncture it and penetrate the cavity.

She continued comfortable after the third operation, and in three days started on a boat to Bailey's Spring. The journey to be made by water, except ten miles from Tusculumbia.

Since her departure from here on the 19th of April, three days after the operation, I have not heard from her.

Remarks.—I have described this case somewhat in detail, as no case of hydrometra so well marked has been reported, within my knowledge, in this country. Its existence, indeed, being more than doubted by Prof. Meigs, in his works on obstetrics and the diseases of females, in which he says, "as to hydrometra, I do not believe in it. It is indifferent to me who has seen it, or who has heard of it. I repeat, I do not believe in such a malady." To prove the disease does not exist, he says: "To have a true dropsy of the womb, you must imagine the os uteri hermetically sealed, and the cavity of the organ filled and distended with serum."

How near the description of this case, the quantity of serum contained in the cavity of the womb, and the difficulty of drawing it off through the consolidated neck, with its cavity contracted, and its internal opening perfectly occluded, comes to what Dr. Meigs describes as being necessary to constitute hydrometra, the reader must judge.

The result of this case, time only can determine. The long and narrow channel in the neck rendered it impracticable to introduce an instrument sufficiently large to make a free incision in the dense membranous sac which contained the fluid, so as to keep it open, and thus discharge the accumulating serum, and prevent the inconvenience and pain produced by the great distension of the womb.

REVIEWS.

ART. IX.—*Human Ovulation and Menstruation.* (Beiträge zur Lehre von der Menstruation und Befruchtung.) Prof. Dr. Th. L. W. BISCHOFF. In Henle and Pfeufer's Zeitschrift für rationelle Medicin, IV. (N. S.) Hft. i. pp. 129–175. 1853.

THIS recent contribution to Human Generation, by one who stands prominent as an investigator of that difficult department of physiology, and whose researches have contributed so largely to its elucidation, is well worthy of an attentive consideration. We improve the opportunity for some extended remarks on this subject, in the shape of a somewhat discursive review.

Human physiology has always laboured under the disadvantage that its students have been inclined to isolate man from the rest of zoological creation, as much in his physical as in his moral capacities and relations. There seems to have been a tacit assumption pervading all their inquiries that the human species, in virtue of its high prerogative morally, is likewise removed, as to the laws governing its physiological conditions, from the rest of mammalia. It is true that the influences of artificial modes of life, as introduced by advancing civilization, have so masked the naturalness of many of his functions as to apparently favour this view; but viewed from a normal healthy point, man's physiological conditions hang upon the same general laws, in the strictest sense of the terms, as those of the great order to which he belongs. He has, it is true, degenerated so much physically that his pathology almost exceeds his physiology, and, therefore, there is so much the more need that we use as stand-points general principles, in our examination of the conditions of particular functions.

As might be supposed, in no department has this error been more prominent than in everything relating to the subject of human generation. Aside from the intrinsic difficulties of the subject itself, there is another reason why this construction should be put upon it: this is that, in civilized nations, no class of functions have been so perverted as those relating to reproduction. These perversions, these anomalies, combined with the fact that the nature of the case has obliged us to receive testimony, as to facts, from persons incompetent to observe, have very naturally led to the general opinion that, in as far as relates to this department, man stands aloof from the other mammalia. I refer here particularly to ovulation and impregnation.

Even from the days of Aristotle, the leading physiologists of every age have alluded to or even predicated the more or less complete uniformity of these functions in man with the animals beneath him; but a demonstration of this important point, based upon the careful observation of facts, belongs to very modern times.

Baer, Bischoff, and Pouchet take this merit almost wholly to themselves, although others have followed in their line of inquiry, and furnished valuable results.

Confining our attention to ovulation, menstruation, and impregnation, we may say that these more modern researches have shown that, in the human

female, the menstruation corresponds physiologically to the periodical heat or rutting time of other mammalia; that at each menstrual period, in the healthy state, an egg is discharged from the ovaries, received into the oviducts or Fallopian tubes, and there is ready for impregnation; that the evidence of this discharge of an ovum from the ovary, is the formation of a corpus luteum, so called, in the tissue of the ovary (I omit here any allusion to the relations of the *verum* and *spurium*); that impregnation, therefore, *should* in the normal state be possible only when coincident with these last-mentioned conditions, and impossible at intervening periods.

These are the prominent points, more or less distinctly made out by the difficult and oft-repeated experiments and observations of modern inquirers. That menstruation is not a function *sui generis* and peculiar to man, in its physiological signification, and that an egg is, in the normal state, discharged at every menstrual period—these two points stand well determined; but the others, the relations of the corpora lutea, and the possible period of impregnation, are far less satisfactorily positive; the first, from difference of opinion on the same data; the second, because the contingencies depending directly or indirectly upon the artificial mode of life of man form almost always a barrier to the truly normal discharge of the function.

The present paper of Bischoff is the more interesting and important, as bearing quite directly upon these last two points; and we will now analyze its contents.

This paper was based upon the observation of thirteen cases, which he had had the opportunity to examine during a period of many years. The examinations were made with the characteristic care of this investigator. After alluding to the difficulties attendant upon this class of observations, especially in the human species, such as those necessarily intrinsic to the nature of the case and subject; the usually great length of time after death that examinations can only be made; the difficulty of examining oviducts and ovaries, from partial decomposition; the liability of the egg to disappear, or not to be found, from its extreme tenuity, &c., he proceeds with the details of the cases, of which we give the following synopsis:—¹

OBSERVATION I. Person, æt. 20. Died during menstruation. Had borne a child in former years. Uterus was found filled with blood, and the ovaries contained numerous highly developed Graafian follicles, and several corpora lutea in different stages of retrogression; but one of these last appeared recent, and was filled with blood. No egg and no spermatie particles were anywhere found, upon the most careful search. The inner uterine surface presented some changes, having a velvety aspect, and a more or less development of the uterine glands.

OBSERVATION II. Person of unknown life, who had drowned herself. Right ovary contained a very large Graafian follicle, inclosing a distinct egg. No appearances of existing menstruation, but rather those indicating that it was about to occur, such as the villous development of the uterine surface.

OBSERVATION III. Person æt. 21, who had drowned herself. Had had no children. The period of menstruation had probably just passed. The left ovary contained a very distinct corpus luteum, and the right ovary a ruptured Graafian follicle filled with fresh blood; the walls of this last were already

¹ In consequence of the general manner with which we can here discuss this subject, we omit the numerous measurements of the size of the different parts of the internal genitalia, which Bischoff has given in many of his cases with the greatest care.

thickened, and the first stages of the formation of a corpus luteum indicated. Uterine surface somewhat changed, but no appearance of villi or developed glands. No egg found in the right oviduct, or in the uterus.

OBSERVATIONS IV. and V. The internal genitalia of two young persons received for examination. Death from acute disease during menstruation. The right ovary of each showed a freshly ruptured Graafian follicle, filled with a blood coagulum. Inner surface of uterus presented no marked development, no prominence of uterine glands, and no beginning of a decidual formation. The oviduct and uterus were searched in vain for an ovum, in both cases; neither were any spermatic particles found.

OBSERVATION VI. This case was related in *Müller's Arch.*, 1846, p. 111. The uterine surface presented a considerably far advanced formation of a decidua by the highly developed uterine glands; a recent corpus luteum had begun to be well formed, but, notwithstanding, no egg was found. Bischoff thinks that, three weeks having elapsed since the last menstruation which was coincident with a coitus, pregnancy had commenced.

OBSERVATION VII. Person æt. 19. Suicide by drowning, three weeks after marriage. Right ovary contained a recently ruptured Graafian follicle, inclosing a more or less altered coagulum. The inner surface of uterus was highly developed, vascular, velvety, and indicating the decidual formation; although, from progressing decomposition, the uterine glands were not distinct. No egg or spermatic particles found in oviduct or uterus.

OBSERVATION VIII. Person childless; æt. 19. Suicide by hanging. Eighteen days before death, menses, as usual, of four days' duration. Coitus coincident.

Right ovary contained a very large corpus luteum, and both ovaries had marks of old but spurious corpora lutea, also numerous Graafian follicles. Uterine mucous membrane considerably developed, and two to three lines in thickness; uterine glands also prominent. No egg found in oviduct or uterus; but close to the insertion of the right oviduct upon the uterus, there was a peculiar formation in the mucous membrane, composed of fusiform cells and granules, which resembled remarkably a nest. But in this Bischoff could find nothing resembling an egg; it may be, however, that this structure served as a lodging-place for the egg during its first development in the uterus.

OBSERVATION IX. Maiden, æt. 19. Died of acute disease two days after a menstruation of three days' duration. Right ovary contained a pretty large projecting Graafian follicle, which was still closed, but filled with coagulated blood. Both ovaries contained very small corpora lutea. Uterine mucous membrane presented no marked development; uterine glands not distinct. No egg was found in the oviduct or the uterus.

OBSERVATION X. Person æt. 20. Suicide by drowning. Menses known to have appeared ten days before death. Genitalia, and especially uterus, largely developed, and bore marks of a previous pregnancy. Right ovary contained a recently ruptured Graafian follicle, which, however, formed no projection on the ovary, but rather a dark red spot on its surface. Here, also, out of five irregular lobules, a fresh corpus luteum could be made out, filled with dark coagulated blood, and containing a cavity surrounded by a denticulated yellow mass.

Uterine surface changed, soft, velvety, and highly reddened. Uterine glands developed and quite distinct. No egg found in oviduct or uterus, and no spermatic particles.

The prominent development of the corpus luteum, and of the uterine mucous membrane, renders it highly probable that pregnancy, depending upon the last menstrual period, had commenced.

OBSERVATION XI. Person childless, young, but age unknown. Suicide by drowning. Imperfect menses ten days before death. Coitus coincident. Both ovaries contained the remains of many spurious corpora lutea, as also numerous transparent Graafian follicles. Right ovary contained a very large Graafian follicle *unopened*, but inclosing a brownish, chocolate-coloured matter, evidently altered blood. The walls of the follicle were not thickened, and there were no appearances of a forming corpus luteum. Uterus contained no blood, and its mucous membrane was scarcely changed. Here, also, no egg or spermatie particles were found after the most careful search.

In this case the imperfect menstruation, the want of development of uterine surface, the appearance of Graafian follicle, indicate that probably there was no real escape of an egg, but simply an effusion into the follicle.

OBSERVATION XII. Person childless, æt. 34. Death by epilepsy. Menses of several days' duration seven days before death. Both ovaries presented the marks of old corpora lutea; the right ovary contained a recent corpus luteum. The inner surface of the uterus was not smeared with blood, and the mucous membrane was feebly developed. No egg, as usual, was found after diligent search.

OBSERVATION XIII. Person æt. 34. Suicide by hanging. Menses four weeks, and coitus for several weeks, but lastly four days, before death. Right ovary showed a ruptured Graafian follicle containing no blood-coagulum, and whose walls were only corrugated, not thickened. Uterine surface villous, with glands developed, and the appearance of a decidual formation. As usual no egg, neither spermatie particles, could be found. The history of this case showed that the person was married, and, fourteen months before death, had borne her only child, which she had nursed until within three weeks, when milk failed. During lactation, menses had been regular. Here it is most probable that all the phenomena connected with menstruation were developed without pregnancy, rather than that this last had really taken place.

These thirteen cases I regard as of high value in physiology. Our theories have always been good enough, but there has been a want of well-observed facts on this subject, especially in our own species.

The results here obtained show that in the human female, at each menstruation, a Graafian follicle is ripened, swells, and usually bursts, discharging an egg, and forming a corpus luteum.¹ This is the grand physiological standpoint from which radiate many inquiries and queries. The first query is, is there a rupture of a follicle, with a discharge of an egg, at each menstruation, without exception?

Coste has stated his opinion that menstruation may occur without such rupture, but this statement seems to have been made more upon hypothesis

¹ It is not meant, of course, that this is the first time these doctrines have been put upon the substantial basis of observation, although the present cases are of great value in a confirmatory point of view. As early as 1842, Bischoff and Pouchet brought out these views in a more or less decisive manner, and since an occasional, though all the more precious contribution, has tended to a corroboration of these important doctrines. Among these last, see the excellent paper of Dalton, "On the Corpus Luteum of Menstruation and Pregnancy," a prize essay, in the *Trans. of the Amer. Med. Assoc.* vol. iv. 1851.

than observation.¹ Observation XI. shows that the *apparent* phenomena of menstruation, such as discharge of blood, &c., may be present without even the rupture of a matured Graafian follicle. When such is the case the female is sterile, although regular apparently in her menses. Bischoff remarks that perhaps the dysmenorrhœa may be due to such imperfect development. Bischoff thinks he has observed the same phenomena in the hog; the Graafian follicles seem well developed, but they do not burst, perhaps from an undue thickness of the walls, and blood is effused into their cavity.

The maturation of the egg in the ovary is the essential point in the whole process of menstruation; the menstrual secretion is secondary and resultant. Normally, these different processes all concur exactly, and then conception is possible if there is a coincident presence of spermatic particles. But if all are not concurrent, conception cannot follow; such prominently are, where the egg is matured properly and in due time, but the Graafian follicle does not burst and allow its escape; and where it is matured and discharged normally, but there being no corresponding uterine changes, it does not meet a fit nidus in the uterus, and is therefore lost. From these considerations it is evident that there may occur menstruation in a subject without fertility, but as the maturation and discharge of the egg is the leading inducing physiological process in this compound function, it (that is, this last) can never occur without a menstruation of *some kind*. Medical literature of this department contains numerous cases which, if we could credit, would support almost any theory or view connected with the phenomena of menstruation and conception. But few of these statements are worthy of any reliance, whether they apparently support or not the true physiological view of the case as founded upon observed facts—for upon the whole subject of sexual relation, especially when in a perverted state from physical disease, there seems to be with the patient a kind of moral obliquity which precludes a correct, truthful statement of his or her case, even where the physician or inquirer can see no possible reason for a suppression of the whole truth. This is indeed a most singular fact, but its truth will be attested to by many a medical man who has more than once had his well-grounded physiological views on a sexual subject completely unsettled by statements of patients whose veracity and singleness of mind (on other matters at least) had long been well known. We think it may be stated, therefore, that in everything pertaining to perverted sexual conditions, facts observed by one's own senses must hold the importance of being objective, while the statements of the individuals themselves must be received only as subjective.

Bischoff has related a case finely illustrative of this point, no less than of the sagacity of his mind and clearness of his knowledge on these subjects.

But we will proceed. There can be no doubt from the results of the preceding cases that, when a regular menstruation occurs in a healthy person, and of course preceded by a normal maturation and discharge of an egg, there is always a development of the uterine mucous surface, whether impregnation occurs or not. Thus, this would be the case in every well-developed healthy maiden. But this uterine development to receive the egg may be deficient from various causes, such as sickness, general weakness, local circumstances, &c. This leads to some interesting inquiries and suggestions.

Thus absent or defective formation of this uterine mucous membrane is most probably a frequent cause of sterility, especially if all the ordinary observable

¹ See Coste, *Hist. du Developpement*, &c. i. p. 221, and Bischoff's notice thereof in Schmidt's *Jahrbuch*, lxi. p. 367, 1851.

conditions of menstruation are present. An egg is discharged normally, and may even be as regularly impregnated, but it meets no proper nidus in the uterus. Herein, also, Bischoff remarks, may perhaps be found the reason why so many women who have borne one or several children, suddenly cease to be fruitful, and remain so for many years, notwithstanding they continue to menstruate normally and there is no general apparent cause for this condition of things. In civilized life, and perhaps also in savage life where the conditions of generation are more or less normal, it is certain that the productiveness of the uterus ceases long before that of the ovaries. We may add that this is a topic of no small moment outside of the domain of scientific physiology. In the present high march of civilization—the continued introduction of artificial conditions of life—there is evidence of a sad degeneracy in this respect. Early marriages, before the uterine powers are fully matured; the confinement, art, and excesses of a city life, even with a well-developed, robust constitution; these may be mentioned as quite adequate to induce either a complete sterility at first, or at least after the first or second child.

But there is another point in this connection of considerable physiological interest; it is, that it is very probable that the *time* of this uterine development necessary for conception, varies in different females, so that some may conceive a longer, and others a shorter time after menstruation; perhaps also there is a like variation as to the coincidence of development between the egg and this uterine formation. The longest time this uterine development was observed after menstruation was eighteen days, as recorded in Observation VIII.¹ This point, however, has other relations, particularly the question of the *inclusive* possible period of impregnation in the female, which we propose to notice hereafter.

In regard to the so often discussed question of the value of the corpus luteum, and its relations to conception and pregnancy, Bischoff has some valuable remarks which we notice more especially as indicating his real matured views on this subject.²

Bischoff declares that his extended observations have led him to the opinion that there is a real distinction between the corpus luteum of simple menstruation and that of pregnancy; in other words, there are corpora lutea *vera* and *spuria*.³ But the differences are not due to primary physiological relations,

¹ Dalton (*loc. cit.* Observations V., VI.) has recorded two cases in which this decidua formation was observed, one fourteen, the other twenty-one days after menstruation. But it appears to have been subsiding, especially in the latter case.

² It is clear that Bischoff's views on this subject have not sometimes been correctly given, and we think it worth while to here allude to the matter. In a translation of one of Bischoff's writings there is the following passage: "Now, from all these observations, it is quite certain that the ova in mammalia, in the time of health, no coition taking place, are detached from the ovary, enter the tube and perish there; and that the corpora lutea are formed in the ovaries *just as though coition and fecundation had taken place.*" See "On the Maturation and Discharge of Ova, independent of Coition," Gilman's and Telkamp's *Translation*, New York, 1847, p. 45; quoted in Dalton, *loc. cit.* p. 14. This translation does not distinctly convey the idea meant by Bischoff. Bischoff means that a corpus luteum is always formed when an ovum is normally discharged, but does not refer to the question of true and false corpora lutea.

³ It may be well to allude here to the views of those who have more or less recently given attention to the subject. First may be mentioned Pouchet (*Théorie Positive de l'Ovulation Spontanée et de la fécondation des Mammifères et de l'espèce Humaine*, &c. &c. Paris 1847), who, basing his opinion upon theoretical grounds rather than upon direct observation, declares that the distinction between the true and false corpora lutea is unfounded. (See p. 185.)

This is also the view of Raciborski (*De la Puberté et de l'Age critique*, &c. Paris, 1844). But decidedly the work of the most importance in this connection, because

for these are the same in both cases; they depend rather upon secondary conditions. In the normal menstruation, necessary to conception, an egg is discharged as we have seen, and the corpus luteum which is *first* found is precisely the same, whether the egg be impregnated and pregnancy follow, or whether it passes away unaltered. So much for the primary process. But here the parallelism ceases. If conception does not occur, the turgescence of the ovary incident upon the maturation and discharge of the egg quickly subsides, and the opposite state follows. The retrograde changes, therefore, in such a corpus luteum are rapid, absorption is constantly going on, and by the time of the succeeding menstrual period, the remains of the ruptured Graafian follicle, with its effused blood, have been more or less removed, leaving behind only a few markings. This is the corpus luteum *spurium*.

On the other hand, if pregnancy does occur, the changes in the ovary above described are much less marked, the whole internal sexual apparatus being the seat of high vascular action. The consequence is, that the corpus luteum, in its full size, changes very slowly, and generally has diminished but little during the first month of pregnancy; and these alterations are so slow, that they often do not cease until after parturition. It will be readily perceived that a body thus formed will differ, in texture, compactness, colour, &c., from the former, which was subjected to the action of absorption. "But what," asks Bischoff, "is the diagnostic value of this distinction, well-marked as it is at the time? During pregnancy it is, of course, of no worth; and, after parturition, when the marks on the uterus of pregnancy have disappeared, it is very difficult to decide, in any given case, between a true corpus luteum fast changing, or even more or less disappearing, and a spurious one, which has experienced the changes of two or three weeks after menstruation." He, therefore, concludes that, in really doubtful cases, the corpus luteum presents distinctions of no decisive practical value.

Bischoff here introduces another interesting point—the dependence of conception upon menstruation. We have already remarked that the periodic maturation and discharge of the egg is the primitive essential feature of the menstrual function in the human female. The menstrual flux is a secondary phenomenon; although, in the truly normal condition, there is an exact relation in all these parts of this function. The discharge of a sanguineous fluid would not, therefore, seem necessary for the process of conception to occur, provided all the other phenomena were present—such as a fit discharge of the egg, and a proper state of the uterine surface to receive and foster it. Pregnancy can and has occurred without menstruation,¹ that is, without the usual sanguineous discharge; but this is an abnormal state of things, and, as is well known, from

founded upon well-conducted observation, is the prize essay of Dalton, already referred to. "The object of this paper is," using the language of its author, "to show that this conclusion of Pouchet [such as we have mentioned] is entirely erroneous; that the corpus luteum of pregnancy is different from the corpus luteum of menstruation; and that it may, under ordinary circumstances, be readily recognized and distinguished from it.

Bischoff's view corresponds quite with that of Dalton, and the statements we have made in the text may be regarded as exponent of the views now entertained by most if not all recent inquirers.

For an excellent historical *résumé* of the subject of the corpus luteum, see Dalton, *loc. cit.*

¹ In these cases of the non-appearance of the usual menstrual discharge, it cannot for a moment be supposed that the ordinary decidual formation occurred nevertheless in the uterus; and it is most probable that there was a discharge of some kind *per vaginam*, indicating that these changes were taking place. For a list of the recorded instances of such pregnancies, see Pouchet, *loc. cit.* p. 290.

the earliest times, a regular complete menstruation has been deemed a requisite of fruitfulness. But the inverse of this may be safely asserted as unknown, that is, that pregnancy can occur without this maturation and discharge of the egg, more or less coincident with some form of menstruation. There are many circumstances which may tend to disturb the healthy relations of the menstrual function, leading to its premature and too frequent recurrence; such are, as is well known, change of climate, diet, and particularly sexual excitement. In these cases, although there may be changes and developments in the ovary, yet, in our opinion, they are all abnormal; thus, an egg may be prematurely ripened and discharged, or it may be matured but not discharged, the Graafian follicle being filled with blood. At least, the infertility of such individuals is well known. It would, then, seem pretty well determined that extraneous influences, such, particularly, as sexual excitement, cannot hasten the *normal* maturation and discharge of the egg, as has been supposed, and that all those conditions which superinduce some or all the apparent phenomena of too frequent menstruation lead to abnormal results or infertility.

The last point we shall here take up, in connection with this interesting paper, is the *time relations* of the menstrual period or the maturation and discharge of the egg, to impregnation in the human female.

In the first place, it should be stated that the result of experiment and reliable observation, so far, indicate that the egg is discharged from the ovary and received into the oviduct, not at the *commencement*, but at the *end* of the menstrual period, or rather when there has been prepared in the uterine cavity a nidus for the egg's reception. Another fact, of a collateral character, to be here stated, is, that observation shows that the impregnation takes place while the egg is passing the oviduct, and most probably at the uterine end of this last. In fact, Bischoff says that, in all his observations upon mammalia, he knows of no instance where conception occurred after the egg had reached the uterus. From these data it may, therefore, be stated that the *inclusive* time of possible impregnation is that from the discharge of the egg from the ovary until it reaches the uterus; and that this period must be a longer or shorter one *after* the end of menstruation. But what is this period? Bischoff thinks it is from ten to twelve days' duration, and, therefore, that, as a rule, impregnation is possible only during this interval. This would probably stand as a correct and reliable statement, could normal conditions of this function be always predicated. But, in the human female, who is most often in a somewhat abnormal state, subjected to the influences of art and luxury, &c., the possibilities of non-conception render the subject quite complex, open it to individual exceptions, and much impair the reliability of an application of the physiological rule. We will look to some of these possibilities. Thus, the egg may be delayed in its discharge from the ovary; or it may be discharged too soon, say during the menstrual flow. There are contingencies relating to its not meeting with the spermatic particles, and becoming fecundated. Other contingencies belong to what may be termed its not being properly nested after impregnation; these are, a tardiness of its passage through the oviduct, not reaching the uterus until the decidua has disappeared, or its normal passage through the oviduct, but reaching the uterus before the changes in the latter are fully complete; in other words, where there is a want of coincidence between the arrival of egg and uterine changes; or, finally, the egg having reached the uterus normally, it there meets no properly formed membrane or decidua, this last not being developed on account of weakness, sickness, &c. Last, but perhaps not least, in this category of contingencies, and which also will always serve to render the more unsettled the whole subject, are possible relations of the spermatic particles. They may remain unchanged, and still alive, in the

vagina, or rather in the uterus, a long time, ready to meet any possibilities relating to the egg that may occur during the intermenstrual period.

We need not enter upon further details of discussion of these contingencies and possibilities; most of them would be attended by non-conception, and that, too, often when all the general conditions would seem to favour the opposite state. Then, again, suppose that in the same individuals changes of life and habits have taken place, and then these abnormal conditions of want of coincidence between the action of all the parts are removed, conception may quickly occur just when it would have least been supposed; and this either after hitherto complete sterility, or an intervening unfruitfulness of perhaps only a few years. We would particularly insist upon these points as worthy of attention in connection with that most intricate and scarcely understood subject, *sterility*.

On the other hand, suppose conception is to take place—that is, the normal coincidences of ovarian, ovular, and uterine developments occur—yet there may be, as observation has already shown, individual peculiarities which affect and diversify the time relations of possible conception. Thus, the passage of the egg through the oviduct, and the formation of the decidua, may be rapid, and, therefore, impregnation possible only very shortly after menstruation. On the other hand, the inverse of this may be true, with a corresponding result.

In conclusion, therefore, we may say that, although physiology and well-authenticated observation concur to show that impregnation is most possible during the twelve days succeeding menstruation, yet individual idiosyncrasies, combined with contingencies due to abnormal relations of life, seriously affect and unsettle the subject as having any practical value.

Lastly, in this connection, we wish to advance a view which we have been disposed to entertain for some time. It is that, in the male of our species, there is a regular periodical sexual excitation, dependent upon constitutional relations alone, which corresponds to the monthly menstrual sexual excitement in the female. The ordinary conditions of life in our own sex, more or less constant occupation of the mind, the greater or less presence continually of outward sexual influences, these could well preclude the notice generally of any excess of sexual ardour at regular special periods during each month. But, in disease, where these conditions and influences are more or less removed, it might readily be noticed, especially in that class of chronic affections attended with some general febrile action, such as phthisis, &c. It was under such circumstances that our attention was first called to the subject; and since, repeated inquiries have tended to verify this view.

We would say, then, that there appears to be evidence that, in the human male, there is each month a period in which the sexual feeling and ardour is increased, and that, too, independent of external influences. This condition of the system here is attended, as in the female, with an exacerbation of the action of the whole vascular system, an increased physical or general animal feeling, and, finally, manifestations of plethora and excitement in the genitalia. No doubt there is at this period an increased formation of the spermatic particles; and the parts, becoming loaded with this secretion, if the natural mode of relief is not obtained, an emission, involuntary and generally at night, is the result; and this discharge is just as normal and necessary as that of any other overloaded organ or organs, and attended with a like relief.

In disease, this monthly excitement has appeared to us indicated by an exacerbation of those symptoms which relate directly to the vascular system. Thus, in phthisis, the fever will be more marked, the cough more troublesome, and the tightness about the chest increased, conditions corresponding precisely to those of the female under the same circumstances.

W. I. B.

ART. X.—*An Enquiry into the Pathological Importance of Ulceration of the Os Uteri. Being the Croonian Lectures for the year 1854.* By CHARLES WEST, M. D., Fellow of the Royal College of Physicians; Physician Accoucheur to St. Bartholomew's Hospital; and Physician to the Hospital for Sick Children, London, 1854. 8vo. pp. 95.

It is time that an accurate and unbiased investigation should be made into the present condition of our knowledge as respects the diseases of the uterus, and their proper treatment; to determine how far the opinions promulgated in regard to uterine pathology are based on correct observation, and the remedial measures deduced from these opinions are calculated to fulfil the necessary indications.

For a long period the several morbid conditions of the uterus were either entirely overlooked or their frequency and importance underrated, and, as a necessary consequence, their therapeutical management was irrational, inefficient, and empirical. Of late years, however, an impulse in an opposite direction has been given to uterine pathology. Various morbid conditions of the uterus, but more especially of its os and cervix, are now considered by many physicians, not only to be of extreme frequency, but the cause of many of the derangements of the other organs, as well as of the various general disturbances of health, to which the female is liable. So far, indeed, is this metritic pathology carried by a few, that, in the investigation of the diseases of the female sex, they would seem to view the speculum uteri as of equal, if not more importance, than is the stethoscope in the exploration of the morbid conditions of the heart and respiratory organs. In too many instances, it is to be feared, that they have inverted the pathological series, and, in directing, too exclusively, their attention to certain abnormal conditions of the neck and orifice of the uterus, that have been brought to light since the introduction of the speculum, they have placed cause for effect, and effect for cause, while many important questions in reference to the diseased conditions of the uterus, their true character, causes, and results, have been allowed to pass without even an attempt at investigation. Nor does the evil stop here. The exclusiveness of the views referred to reacts injuriously upon him who entertains them. As Dr. West very truly remarks—

“He unlearns what physiology might teach him of the uterus and its functions, and sees in all the varied manifestations of disorder the expression of one fact, and one fact alone; namely, the existence of ulceration at the orifice of the womb, and its reaction first on the uterine system, then on the general health. For him, indeed, there is little more to learn in uterine pathology; for when once a case has been ascertained not to be one of fibrous tumour, polypus; or cancer, then ulceration of the os uteri is the almost invariable cause to which the symptoms are referred, and the cure of this ulceration is the one grand object at which he endeavours. All the evils inseparable from the practice of a speciality are thus aggravated, and the natural tendency of such practice to subside into routine, or to degenerate into empiricism (I use the word in no invidious sense) becomes almost unavoidable.”

The frequency with which the uterus becomes the seat of disease, and the importance of all its morbid conditions in their influence upon the comfort, health, and, occasionally, upon the life of the female, are such as to demand from the medical practitioner, the closest investigation, that he may become

acquainted with their nature, their causes, and distinctive characters, and the means best adapted for their prevention and cure.

In the lectures before us, Dr. West presents the results of a very logical and apparently candid inquiry into the pathological importance of ulceration of the os uteri; a question of deep interest at the present day, in consequence of the disputes to which it has given rise. "Although the conclusions to which the lecturer has arrived are in direct opposition to the opinions of those who maintain that inflammation of the cervix and ulceration of the os uteri are the first and the last links in the chain of uterine pathology, they nevertheless claim the serious consideration of the medical practitioner.

The important question discussed in these lectures is—

"Whether ulceration of the os uteri is to be regarded as the first in a train of processes which are the direct or indirect occasion of by far the greater number of the ailments of the generative system; or whether, on the other hand, it is to be considered as a condition of slight pathological importance, and of small semeiological value—a casual concomitant, perhaps, of many disorders of the womb, but of itself giving rise to few symptoms, and rarely calling for special treatment?"

Dr. West arranges the evidence by which he has endeavoured to solve the question just stated under four principal heads:—

Under the *first* head is included the evidence deduced from what we know of the anatomy and physiology of the uterus in a state of health. The evidence derived from this source, Dr. West, however, admits, cannot from its very nature, be conclusive.

"It may render a certain occurrence probable or improbable, may substantiate or disprove the correctness of certain opinions or explanations, but cannot invalidate the evidence of positive facts."

Under the *second* head is included the evidence derived from morbid anatomy. Whether examination of the dead body shows the morbid conditions of the os uteri which have been described to be frequent or rare, slight or extensive; and whether we can make out what connection subsists between ulceration of the os and cervix uteri, and other changes in the tissues of the organ.

"It must, however, be borne in mind," remarks Dr. W., "that many evidences of disease, such as are very obvious during life, may be greatly obscured, or may even entirely disappear after death: and further, that uterine disorders of the class which we are considering, though exceedingly painful, and seriously interfering with a woman's health and comfort, are yet not of a kind to prove the direct occasion of her death. Evidence derived from this source will therefore be open to the objection that it understates both the frequency and the importance of these diseases."

Under the *third* head is adduced the evidence derived from the characters, course, and consequences of ulceration of the os uteri, as it presents itself to our notice, unconnected with other disease, in the case of the procident uterus.

"But," observes Dr. W., "whatever conclusions we may deduce from this source are open to all the objections inseparable from analogical reasoning. The probabilities of certain occurrences taking place in the uterus under other circumstances may be increased or weakened, but the evidence still falls short of absolute proof, either of the affirmative or of the negative character."

Under the *fourth* head is comprised the evidence derived from clinical observation. The determination from clinical experience, as to the frequency of ulcerations of the os uteri under those circumstances in which they ordi-

narily come under our notice, and call, or are supposed to call, for our interference, as well as the conditions generally associated with the ulceration, and the symptoms to which it commonly gives rise.

"If," Dr. W. remarks, "the alleged symptoms of ulceration are found to be not rarely present without ulceration, and if ulceration is discovered even where there are no symptoms; or if, in the same case, the ulceration may vary in extent, with no corresponding change in the symptoms; if an indurated state of the cervix uteri exists without ulceration, and ulceration even of long standing, without induration—the conclusion, especially if supported by the answers obtained to our previous inquiries, seems to me irresistible, that the importance of inflammation of the cervix, and of ulceration of the os uteri, has been overstated; that they are not the cause of all the symptoms which they have been alleged to occasion, and that, in the treatment of uterine disease, many other considerations must influence us more than the mere removal of ulceration of the orifice of the womb."

In regard to the testimony derived from anatomy and physiology, the lecturer shows, that in organization and physiological importance the cervix is inferior to the body of the uterus; that it is much less liable to morbid alterations in its intimate structure, and sustains, with surprising impunity, mechanical violence and the contact of the strongest caustics.

"But," he remarks, "if structurally so lowly organized—if physiologically of such secondary importance—if so much less subject than the body of the uterus to alterations in its intimate structure—and if so comparatively insensible even to rude modes of therapeutical interference—it certainly does appear to me that the assumption that some slight abrasion of the mucous membrane covering this part is capable of causing a list of ills so formidable as are attributed to it, ought to rest for its support upon some other and stronger foundation than any inference fairly deducible from anatomical or physiological data."

Under the second head of evidence, Dr. W. presents the results of the examination of *sixty-two* uteri taken from patients who died in the medical wards of St. Bartholomew's Hospital of other than uterine disease.

"Of the total number, 13 were above forty-five years of age, the remaining 49, between the years of fifteen and forty-five. Concerning all of the former class, and 30 of the latter, making a total of 43, it was either known with certainty, or concluded with great probability, that they were married, or had had sexual intercourse; the remaining 19 were believed to be virgins.

"The subjoined table shows the general results of the examination of the uterus in these cases, and the relations borne to ulceration of the os uteri by the more important morbid appearances.

Uterus healthy in	33
" diseased in	29
Ulceration of os uteri in	17
<hr/>	
Ulceration existed alone in	11
" with diseased lining of uterus in	3
" with induration of walls of uterus in	3
	— 17
Induration of walls of uterus, without ulceration of os	5
Disease of lining of uterus, without ulceration of os	7
	—
Total of diseased uteri,	29

"The os uteri was abraded in one of the subjects above forty-five years of age, and the lining of its interior was diseased in five of that number. In eleven of the nineteen patients, all under forty-five years old, who were virgins, the uterus was perfectly healthy; in eight, it presented some sign or other of disease. This

consisted five times in slight abrasion of the os uteri, which existed alone in three cases, but was associated in the other two with some morbid state of the interior of the womb. Twice the interior of the uterus was the only part affected, and once the uterine walls were much harder than natural.

"There is certainly something," remarks Dr. W., "at first not a little startling in the result at which we arrive, that the womb was found in a perfectly healthy condition in little more than the half of sixty-two women, none of whom died of uterine disease, nor were supposed to be suffering from any grave uterine ailment. But it may, it ought indeed, to be asked, what is the value of these appearances? Some of them may be of little moment, and the very frequency of their occurrence, instead of substantiating the opinion that they are of great importance, rather militates against that supposition. When ulceration of the os uteri was first observed it was natural enough to attribute to it many symptoms, and to refer to its influence many structural changes. But what if such ulceration be found to be usually very limited in extent, and so superficial as to be unassociated with changes in the basement membrane of the affected surface, and exercising so little influence on the state of the uterus in general as to be unconnected, in a large number of instances, with changes either in the interior of the womb or in its substance, while induration of the uterine tissue and disease of the lining membrane of the womb are found independent of it or of each other? Should such appear to be the case, it will, I think, be rendered in the highest degree probable that this abrasion of the os uteri has not the long train of sequences which have been supposed to follow it, but that it is of comparatively small pathological import; that it may be found to vary under the influence of comparatively trifling causes; and not unfrequently to be dependent on functional disorder of the uterus, just as the mucous membrane of the tongue and mouth betrays the disturbance of the digestive system; that it may, in short, be the consequence and sometimes the index, but rarely the occasion of the ailments with which it is associated.

"Abrasion of the os uteri was observed in eleven instances unconnected with any other morbid condition of the womb. In six cases it was extremely slight, affecting just the edges of the os uteri, but not extending for more than a line in breadth; the mucous membrane lining the canal of the cervix was in all of these instances quite pale, but twice the lining of the uterine cavity was of a brighter red than natural. In the other five cases, the abrasion, though retaining the same character, was more extensive; once the abraded surface presented a finely granular aspect, but was quite uniform; but in the other four cases it had an uneven worm-eaten appearance, probably due to a partial destruction of the papillæ which beset the os uteri. In four of these cases the abrasion extended for a short distance up the canal of the cervix, while once it was limited to that exclusively, the lips of the os being perfectly pale and healthy, and the mucous membrane of the cervix unaltered, excepting along a strip of a third of an inch in breadth by an inch in length, where the posterior wall was abraded. In three of the above four instances there was some increase of vascularity in the mucous membrane of the cervix, which on one occasion extended for nearly half an inch up its canal; and once this condition was very marked, and the mucous membrane appeared swollen and infiltrated, but in no other case was there any appearance of thickening of the membrane either at the seat or in the immediate neighbourhood of the abrasion."

"Under what circumstances is induration of the uterine tissue met with, and in connection with what other changes in the organ? It existed in nine cases: in five of which it was not associated with any other disease of the uterine substance; in three, it coexisted with ulceration of the os; and in one, with a morbid state of the interior of the uterus. In an unmarried girl, aged eighteen, who died of cardiac dropsy, the tissue of the fundus, and of the upper half of the body of the uterus, presented its usual characters; but about half-way down the body of the organ there began a strip of a dead yellow colour and much denser texture, resembling fibro-cartilage or the elastic coat of an artery. The dense tissue lay immediately beneath the lining membrane of the uterus, and being at first only one line in thickness, increased in width till it came to constitute the whole thickness of the cervix uteri. In the case of another patient, aged forty-seven,

a similar condition was met with in the body of the uterus, but scarcely at all involved the cervix; and in the three other cases, in all of which the women were under thirty years old, the cervix uteri alone was affected, being white, hard, creaking under the knife, and seeming, under the microscope, to be composed of an extremely dense fibrous tissue.

"It appears, then, that most marked induration of the tissue of the cervix, and of part of the body of the womb, may exist where there is no other trace of inflammation either past or present. It may also occur in connection with inflammation and ulceration of the lining membrane of the uterine cavity. In a woman, who died at the age of fifty-six, about a third of the thickness of the wall both of the body and neck of the womb was exceedingly firm and creaked under the knife. Abundant glairy secretion from the cervical glands, and some want of transparency of its lining membrane, were the only unusual conditions of the interior of the uterine neck; but the cavity of the organ contained a copious purulent secretion mixed with blood; its mucous membrane was thickened, vascular, and destitute of polish, and about the middle of the posterior wall completely destroyed, leaving the substance of the womb beneath uneven, rather soft, and presenting the appearance of a granulating surface.

"Ulceration of the os uteri, and induration of the uterine walls, were associated together in three instances. On one occasion the ulceration was but slight, and the interior of the cervix extremely pale, though there was great injection of the lining of the uterine cavity. In this instance the cervical wall was much indurated, that of the body of the uterus rather less so. Extreme induration of the cervix existed in one case where there was rather extensive ulceration of the os uteri; and, in this instance, the cervix was considerably hypertrophied. The patient from whom this uterus was taken had been under my care for some years previously, suffering from symptoms such as Gooch describes under the name of irritable uterus; her sufferings had been most severe, and the enlargement of her womb most considerable at a time when there was no abrasion of its orifice. In one case only, in which there was considerable induration of the cervix, there was a distinct line of congestion about half a line in depth, between the ulcerated surface and the pale tissue of the indurated cervix.

"In ten cases, the condition of the lining membrane of the uterine cavity deviated from that which characterizes it in a state of health. Thrice this state of the interior of the womb coexisted with ulceration of its orifice of moderate extent, and presenting its ordinary appearance; but in the remaining seven instances the os uteri was perfectly healthy. In seven of the ten cases the uterine mucous membrane was vividly injected so as to present a bright rose tint, and was more or less swollen and softened. Once very extensive disease of the lining membrane of the uterine cavity, probably of a tuberculous character, was discovered in the body of a woman fifty-six years old. In a second case, in which the patient was stated to have had a copious leucorrhœal discharge, and to have complained of pain and of a sense of heat at the lower part of the abdomen, the intensely red mucous membrane of the uterine cavity presented an almost gelatinous appearance, and looked not unlike decidua. In this instance, though there was some ulceration of the os, yet the lining membrane of the cervix was quite pale; no secretion occupied its canal, and the tissue of the uterus was quite healthy. In a third case, a small patch of ecchymosis was present beneath the lining of the uterine cavity; and in a fourth, where the patient had not menstruated for five months, the lining membrane, though of a pinkish colour, had lost its polish, and looked more like an injected serous membrane than like the mucous lining of the womb."

In relation to many of the above abnormal appearances, they ought probably, Dr. W. believes, to be classed with pseudo-morbid rather than with pathological conditions; but the data, he remarks, at present fail us for distinguishing with accuracy the one from the other.

"But, be this as it may, it is yet abundantly evident that many of them imply deviations from a healthy state more considerable than the trifling abra-

sion or ulceration of the os uteri, which existed on several occasions. We have seen that, in by far the majority of cases, the ulceration, when present, was not merely trifling in extent, but that it had not given rise to so much irritation of the neighbouring tissues as to produce any appreciable congestion of the mucous membrane in its vicinity, while the changes in the uterine substance alleged to depend upon it were oftener present without than in connection with it; and, moreover, none of the alterations about the os and cervix of the womb were so considerable as those which were apparent in its cavity."

Under the third head of evidence, Dr. W. examines the effects commonly produced by ulceration of the os uteri in cases of prolapse of the womb beyond the external parts, and the symptoms to which it generally gives rise, and where we can trace the ulceration in its progress; can watch it for weeks or months together, and even see what it has led to when it has existed for years.

"It can scarcely be necessary to say," remarks Dr. W. in summing up his conclusions under this head of the inquiry, "that it is not my intention for one moment to assert that misplacement of the womb produces no inconvenience, or that ulceration of its orifice, when it is thus misplaced, is of no importance. Daily experience yields abundant proof to the contrary; but a detail of the symptoms of prolapse of the uterus forms no part of our present object. I referred to the accident and its consequences only for the sake of suggesting the reasonable inference that, if inflammation of the neck of the womb were as frequent as has been supposed, or ulceration of its orifice the necessary occasion of such serious disorder of function and alteration of structure, we ought to meet with some of the most striking illustrations of these facts in cases where the womb, by its misplacement, is exposed to injuries from without, such as it was never intended to encounter.

"But though it be conceded, as I think it must be by all observers, that the symptoms supposed to characterize inflammation of the neck of the womb, and ulceration of its orifice, are not met with either constantly or in a specially marked degree in cases of prolapsus or procidentia uteri; still, we should not be justified in drawing an absolute conclusion from what we observe in the misplaced uterus, as to the effects produced by similar ailments attacking the organ when in its natural position. It may be alleged, and with plausibility, that during the gradual process of its displacement, the sympathies of the womb have been rendered less keen than they were while the organ retained its natural position; and that thus it comes to bear, with comparative impunity, injuries which might otherwise have produced great disorder of its functions and great alteration of its tissue."

Under the fourth and last head of the inquiry: What clinical observation generally teaches us concerning ulceration of the os uteri, its course, its symptoms, and its importance? the questions to which especial attention is directed by the lecturer are, whether sterility is more frequent, whether the rate of fecundity is lower, and whether abortion occurs oftener in cases marked by the presence of ulceration of the os uteri than in those in which ulceration is not present? Whether menstrual disorder is more common, more severe, or different in kind; whether leucorrhœa is more abundant, or furnished from a different source; or whether pain is less tolerable in the one class of cases than in the other. And, lastly, whether similar or different causes produce the uterine affections in the two classes of cases; whether the duration of illness is the same, and whether the structural alterations of the womb are alike or diverse?

The materials from which the lecturer has endeavoured to make some approach to a satisfactory answer to these questions are derived from 1226 cases, of which records were preserved while the patients were under his care, either at the Middlesex or at St. Bartholomew's Hospital. Of these, 300

were in-patients of one or other institution, and the remaining 926 were out-patients of St. Bartholomew's Hospital, between January 1, 1850, and October 15, 1853.

The investigation of the materials thus derived, as presented by the lecturer; the analysis and collation of the facts they present in their bearing upon the questions proposed for investigation, are valuable and interesting; they are well deserving of a careful and candid examination. We shall not pretend to present an abstract of them. To form a just estimate of their accuracy and the correctness of the author's deductions from them, they must be studied in detail. We can only lay before our readers the general conclusions to which the author has been led.

"1. Uterine pain, menstrual disorder, and leucorrhoeal discharges—the symptoms ordinarily attributed to ulceration of the os uteri—are met with independently of that condition almost as often as in connection with it.

"2. These symptoms are observed in both classes of cases with a vastly predominating frequency at the time of the greatest vigour of the sexual functions, and no cause has so great a share in their production as the different incidents connected with the active exercise of the reproductive powers. But it does not appear that ulceration of the os uteri exerts any special influence, either in causing sterility or in producing abortion.

"3. While the symptoms are identical in character in the two classes of cases, they seem to present a slightly increased degree of intensity in those instances in which ulceration of the os uteri existed.

"4. In as far as could be ascertained by careful examination, four-fifths of the cases of either class presented appreciable changes in the condition of the uterus—such as misplacement, enlargement, and hardening of its tissue, while frequently several of these conditions coexisted. An indurated or hypertrophied state of the cervix uteri was, however, more frequent in connection with ulceration of the os uteri than independently of that condition.

"5. The inference, however, to which the last-mentioned fact would seem to lead, as to the existence of some necessary relation—such as that of cause and effect—between ulceration of the os uteri and induration of its cervix, is in great measure negatived by two circumstances:—

"1. The number of instances in which an indurated cervix coexisted with a healthy os uteri.

"2. The fact that, while induration of the cervix was present in 25 out of 46 cases in which the ulceration of the os was very slight, it was altogether absent in 9 out of 16 cases in which the ulceration was noted as having been very extensive."

Thus, it will be perceived that the final conclusion of Dr. West's inquiry into the pathological importance of ulceration of the os uteri is, that such ulceration is neither the general cause of the symptoms which have been attributed to it, nor even a general concomitant of them, and an index of their degree and severity.

In the third and concluding lecture, Dr. W. enters into a brief inquiry into the actual causes of the various morbid phenomena, that are considered by many as dependent solely upon ulceration of the os uteri.

These causes he believes to be very various, sometimes independent of local disease, as in the case of chlorosis, of hepatic disorder, of granular disease of the kidneys, of the gouty or rheumatic patient, all of which instances illustrate the dependence of uterine disorder on constitutional disease. Ulceration of the os uteri, when it occurs in such cases, Dr. W. considers to be of secondary importance, and equally so in many instances where disease really begins in the uterus itself, as in ailments succeeding to pregnancy, abortion, delivery, etc. In these latter cases, Dr. W. believes that the mischief commences in the interior of the womb as well as in other instances where the symptoms of

sexual disorder have succeeded to marriage, or where they have followed suppressed menstruation, and in those also in which uterine misplacements are succeeded by signs of sexual disorder, or where these signs have been associated with misplacement of the ovary.

Referring to the treatment pursued by those who advocate the pathological importance of ulceration of the os uteri, Dr. W. remarks :—

“It may be asked, how is it that such successful results have followed a course of treatment directed exclusively to the cure of the ulceration—that the application of caustic to the os uteri has been succeeded by the restoration of the patient to health? Now, I think, it should be borne in mind that, in connection with this mode of treatment, various other measures are of necessity adopted, eminently calculated to relieve many of the slighter forms of uterine ailment. The married woman is for a time taken from her husband's bed, the severe exertion to which either a sense of duty urged, or a love of pleasure prompted her, is discontinued; while rest in the recumbent posture places the uterus and the pelvic viscera in just that position in which the return of blood from them encounters the smallest difficulties. The condition of the bowels, probably, before habitually neglected, is now carefully regulated, and the patient's diet, bland, nutritious, and unstimulating, often differs widely from that with which, while all her functions were over-taxed, she vainly strove to tempt her failing appetite. Add to this, that the occurrence of the menstrual period is carefully watched for; that all precautions are then redoubled, and each symptom of disorder, such as, on former occasions, had been borne uncomplainingly, though often not without much suffering, is at once encountered by its appropriate remedy; while, generally, returning convalescence is met in the higher classes of society by a quiet visit to the country, or to some watering-place, in pursuit not of gaiety but of health; and we have assembled just those conditions best fitted to remove three out of four of the disorders to which the sexual system of woman is subject. But the very simplicity of these measures is a bar to their adoption; for you will bear me out in saying, that the rules which common sense cannot but approve, but which seem to require nothing more than common sense to suggest them, are just those to which our patients least readily submit. The case is altered, however, when these said rules are laid down not as the means of cure themselves, but only as conditions indispensable to the success of that cauterization which, repeated once or oftener in the week, is the great remedy for the ulceration that the doctor has discovered, and which he assures his patient, and with the most perfect good faith, produces all the symptoms from which she suffers. The caustic used in these milder cases is the nitrate of silver; the surface to which it is applied is covered by a thin layer of albuminous secretion, which it is not easy to remove completely, and which serves greatly to diminish the power of the agent, while the slightly stimulating action that it nevertheless exerts seldom does harm; sometimes, I believe, does real good, though no more than might have been equally attained by vaginal injections, or other similar remedies, which the patient might have employed without the intervention of her medical attendant.”

Notwithstanding it is the conviction of Dr. West, that in the great majority of instances in which the nitrate of silver is applied to the os uteri, the proceeding is simply superfluous, while to the use of caustic potash in cases in which the neck of the womb is more or less enlarged, he is altogether adverse, still, he admits that there are some exceptional cases in which ulceration or some allied morbid condition of the os uteri is found to exist, independent of any appreciable disease elsewhere; and others, equally rare, in which, after symptoms of uterine ailment have been subdued, a morbid state of the os uteri persists, that are benefited by stimulant applications.

“In such cases,” he observes, “I use either the nitrate of silver, or the acid nitrate of mercury, though neither of them frequently; and for weeks together no case appears among my patients at St. Bartholomew's Hospital, in which

the employment of either appears to me indicated. In justice to others, it should, I think, be observed, that we have no right to infer that the majority of practitioners who resort to these agents with much greater frequency than some of us feel warranted in doing, regard them as absolutely the best remedies that could be used, but merely as the best under the peculiar circumstances in which uterine diseases have to be treated. Were it possible to keep any of those milder agents in contact with the abraded os uteri which can generally be applied to an irritated or ulcerated surface elsewhere, this would doubtless be allowed, in many instances, to be a preferable proceeding. The problem, however, is to find some agent sufficiently powerful to exert an influence which may continue for several days, and thus to obviate the necessity for that frequent painful interference which would otherwise be required. That lotions, baths, and other remedial agents, which may be safely entrusted to the patient herself, will answer the desired ends more frequently than some practitioners imagine, is my firm conviction, but I could not refrain from stating what seems to me to be the candid interpretation of their conduct who pursue a different course of proceeding."

We have thus endeavoured to present to our readers an exposition of the views to which Dr. West has arrived in relation to the pathological value of ulceration of the os uteri and its proper treatment. We bespeak for his lectures a careful and candid study on the part of the physicians of the United States, believing them to be calculated to impart important suggestions in reference to the true character and pathological relationship of these ulcerations, and their correct therapeutical management. The series of facts presented by the lecturer, and the deductions to which he believes them to lead, place ulceration of the neck and orifice of the womb in a point of view that has almost entirely escaped the attention of the leading writers on the subject, and indicate a plan of treatment of the cases in which it occurs more rational and less objectionable than the one now almost universally practised.

D. F. C.

BIBLIOGRAPHICAL NOTICES.

ART. XI. *Clinical Report on Dysentery, based upon an Analysis of Forty-nine Cases, with Remarks on the Causation, Pathology, and Management of the Disease.* By AUSTIN FLINT, M. D. Buffalo, 1853—pp. 90.

Clinical Report on Chronic Pleurisy based on an Analysis of Forty-seven Cases. By the same—pp. 58.

THESE reports were originally published in the *Buffalo Medical Journal*. They have been reissued in their present form with the hope "that they may prove acceptable to some who are not readers of the periodical just named." To all physicians who justly appreciate the results of pure observation, they will undoubtedly be welcome as an addition to positive knowledge, and as a pledge that the principles upon which they have been framed will be widely disseminated by the professional lessons of their author. If we were to say that they contain nothing new, the statement would perhaps be literally correct, yet it would be far from warranting the conclusion that they are deficient in value. On the contrary, we regard them as of great value, because they lend the support of demonstration to doctrines and to modes of practice, some of which have rested too much upon authority; and they show with equal clearness the groundless nature of certain prevalent opinions. But their highest worth consists in their being models of patient and candid investigation—studies of nature under the guidance of a severe analytical method. The spirit which presided over their construction is so eminently a rational and conscientious one that the comparative paucity of their results may be overlooked in favour of the value of their example. This spirit the author himself illustrates didactically in the opening to the first-named report. We commend his remarks to the careful perusal and reflection of those who do not, as well as to those who will not, understand the application of numerical analysis to medical facts.

The subject of the first report is Dysentery. Its phenomena are analyzed in various sections, including the previous condition of the patient, the symptoms of invasion, those of the fully established disease in the digestive, circulatory, and other systems, each by itself, its duration, &c. We shall note a few of the prominent results which are developed by the analysis. Nearly all of the cases occurred in the months between July and October inclusive. There was no antecedent diarrhoea that did not evidently belong to the dysenteric disease. The prognostic value of several varieties of alvine evacuations is thus given: fibrinous laminæ were present only in the grave and fatal cases, and sero-sanguinolent stools (*lotura carni*) when copious, were of most serious and generally fatal import. If the pulse, says Dr. Flint, exceed 120 in the adult, the patient is in considerable danger, and the danger increases in a geometrical ratio as the pulse rises above this point, and in the same ratio to the rapidity of its rise. Delirium occurred in no case that recovered; but, in five out of nine fatal cases it was present, and in three of these was of a maniacal form. It is stated that of thirty cases whose duration was known, thirteen were in hospital and seventeen in private practice. The mean duration of the former was thirteen days, and of the latter only seven days. As regards mortality, twenty-seven patients were in private, and twenty-two in hospital practice; of the first class eight died, and of the latter, three. Consequently, the mean duration of the disease was shorter, and the mortality greater, in private practice. The author explains this by suggesting that the patients in the hospital were more favourably situated for successful treatment than those in private practice. Might the reason not have been that the cases in private practice comprised two classes, the one of patients who were so lightly attacked that their removal

to the hospital was thought to be unnecessary, and the other of such as were too ill to be carried thither? In no case was there any relapse, *nor has the author ever met with a second attack of dysentery* in the same person.

The result just quoted is, we believe, quite novel, and it is opposed to the positive statements of several writers. Dr. Flint does not assert, but he is disposed by the results of his own experience to suspect, that such statements have been made incautiously. The question is a very interesting one. As indirectly bearing upon it, the author presents several reasons for regarding dysentery as something more than a colitis—as having a specific nature which would entitle it to be ranked amongst the diseases which attack but once in a lifetime. The proofs of its possessing such a nature he discovers in the character of its cause. This he affirms to be occult, because, among other reasons, no obvious source can be assigned for it; because its prevalence is confined to certain periods and places when and where no appreciable new cause of disease is in operation; because of its *apparent* analogy to diseases which attack but once, &c.

The author inquires how far we possess a knowledge of the intimate nature, or, as he not very correctly styles it, the pathology of dysentery. He proves clearly enough, in connection with the argument just referred to, that the disease is not merely a colitis; but he fails, as all have failed, in showing what other pathological elements it comprises.

In considering the treatment of dysentery, the author gives a negative answer to the question: Have any remedies been found to exert a specific control over dysentery? He is then at some pains to determine the indications of a rational treatment, but arrives at the inevitable conclusion that the best remedies are those which have been found best. It is in vain to lay down gravely such rules as that the objects of treatment in this affection are “to endeavour to prevent or abate the severity of the local manifestations of the disease; to relieve the symptoms incident thereto; to obviate unfavourable events,” &c. They amount to nothing more than that the symptoms of the disease individually or collectively are to be opposed by remedies which experience has shown capable of palliating or removing them; not individual experience, nor that of any one place, season, country, or period of time, but the resultants of all the various combinations of these elements, and not the common or average resultant of all experience of the disease under the various circumstances mentioned. That would be to establish a therapeutical formula inapplicable to any individual case, or epidemic, or locality. Sound inductive reasoning would go no further than to apply the results of treatment in a group of cases of the same kind to another group of analogous cases. That is the nearest approach which the art of therapeutics can make to a scientific expression or application. Our author says that “diseases must be treated, to a greater or less extent, on *rational principles*—that is to say, principles belonging to general therapeutics applied to the management of particular affections analogically or *à priori*.” It would be interesting to witness the treatment of a case even of pure sthenic dysentery on “rational principles” by a person who had never before seen or heard of the disease. Would he use purgatives or astringents?

Dr. Flint is no advocate of the *lancet* in dysentery, although he thinks cases now and then occur which it might benefit. Of *purgatives*, and particularly those of the saline class, he is disposed to think favourably; but less from his own experience than that of others. We hope that he may have an opportunity of trying them in sthenic cases and near onset of the attack. There is little doubt of his esteeming them still more highly than at present. *Mercury* is dismissed by our author as a remedy introduced into the treatment of the disease, and still retained upon hypothetical grounds alone. He says, and we agree with him, that it was a happy thought to make its combination with opium the condition of its success. Of *opium* itself, he can only say that he subscribes to the undeviating testimony of practitioners through many ages in its favour. He prescribes it in large—we may say in sometimes very large doses. He has even known “twenty-four grains of the sulphate of morphia to be taken in twenty-four consecutive hours by a patient not habituated to the use of opium, with no evidence of narcotism; nor could this quantity be dimi-

nished without marked aggravation of the symptoms." His impression of the utility of *astringents*, derived from his own experience, is unfavourable. Even astringent enemata did not appear to him useful except so far as they favoured the retention of the opiates which they contained. This opinion, if applied to small enemata, we should subscribe to; but not if it relates to such as are large enough to reach the colon and be retained. The effect of these upon the frequency of the discharges is always striking. The author lays great stress on the importance of administering *diffusible stimulants and nutriment* systematically. He says: "I am convinced that, with a due appreciation of the importance of supporting measures, on the part of the practitioner, together with promptness, boldness, and perseverance in their use, and a careful supervision of all the details which they involve, patients are sometimes carried safely through this disease, when the gravity of the affection is such that a fatal termination would otherwise be inevitable." He has frequently found the pulse to lose its excessive frequency under their liberal use, and the safe passage of the crisis of the attack to depend upon their artificial support. To rightly employ these agents in dysentery requires much tact and judgment; for as much harm may be done by their untimely or undue administration as by withholding them altogether.

The Report on Chronic Pleurisy offers less interest than the one which has just been noticed, because, in most cases, this affection is a very simple one—is, in fact, rather an infirmity than a disease; and the questions concerning it mooted by our author scarcely divide the opinions of medical men. It would appear, however, that a portion of the profession is less informed upon the subject than could have been expected, for, in eighteen cases out of forty-seven analyzed by Dr. Flint, an error of diagnosis had been committed. The affection had been mistaken for "disease of the heart, abscess between the pleura and the walls of the chest, bilious fever, hepatization of lung, liver complaint, general debility, and some pulmonary affection, the nature of which was confessedly not known." It is to be hoped that the possibility of such errors is diminishing every year; but, while clinical instruction is regarded as a superfluous branch of medical education by many of our colleges, a long period must elapse before even such gross blunders will cease to be made.

In four cases of the author's series, pleurisy was developed by perforation of the lung, which in two cases was tuberculous, and, in the other two, gangrenous.

The medicinal treatment of chronic pleurisy was found by Dr. Flint to be of secondary value, and, in this respect, he agrees with good observers generally. He noticed in several cases, however, an apparent subsidence of the effusion under the use of diuretics, particularly squill, digitalis, and the supertartrate or nitrate of potash. But these agents were generally associated with mercury, so that their effects, and that of the latter medicine, cannot be strictly estimated. The influence of tonic remedies, diffusible stimulants, a generous diet, moderate exercise in the open air—and particularly of the last of these—was much more decided. This fact harmonizes well with the view that a serous effusion in the pleural cavity is not, in strictness, a disease.

The reports which have been thus briefly noticed are so excellent in their spirit, and, considering the imperfection of the materials, so satisfactory in their execution, that we cannot but anticipate from their author still more important contributions to the literature of practical medicine. A. S.

ART. XII.—*First Annual Report to the General Assembly of Kentucky relating to the Registry and Returns of Births, Marriages, and Deaths, from January 1 to December 31, 1852.*

THIS brochure contains 112 octavo pages; of which more than 100 consist of statistical tables, presenting a very unprepossessing object for the eye of the general reader. Owing, however, to some rather singular predilections, we

have been tempted to look into the contents, among which we have found materials capable of being worked up to no little profit.

In establishing this system of general registration, Kentucky has commenced a noble work—one which reflects credit on her physicians, by whom it was originally devised, and the statesmen by whose legislative action it was consummated. It requires considerable intelligence to comprehend the advantages to be derived from a mass of minute records of deaths, births, and marriages, accumulated through years of labour, and at no little cost. But in the hands of the skilful statistician such details reveal facts of the greatest value to the public. They teach us where the causes of mortality most abound, and thus admonish us to adopt plans for removing malign influences and for promoting health. They show the average duration of human existence, and thus lead to more perfect computations of the chances of life upon which companies for life insurance and granting annuities, found their calculations. Those who consider the bearings of such institutions upon the security of property, and lessening the risks of individuals, and more especially of the most helpless and dependent members of families, may form some idea of the value of data which, like all correctly kept life-tables, serve as the basis of life insurance and annuity computations. Such are a few of the more direct advantages derived from a registration of deaths, with the ages at which these occur. In regard to marriages and births, records are extremely valuable as establishing many facts of great value to the political economist, and the proper succession of property, as well as to the illustration of physiological laws. In all countries where the affairs are managed by the most intelligent of the population, systems of registration are carefully maintained, and we therefore regard the establishment of these in the various States of our Union as direct evidences of the wisdom by which the governments are administered. With the first year's Kentucky report, and the elaborate eleventh year's return from Massachusetts on our table, we scarcely know what to say of the abortive attempt to introduce a system of registration into Pennsylvania, where a law enacted two years ago remains a dead letter. This is a disgrace to our State, which we trust will be removed ere long.

The Auditor of the State of Kentucky, Thomas S. Page, Esq., whose duty it is to render the Registry Returns to the General Assembly, remarks in his communication, that—

“As this is an entirely new measure in our State, considerable difficulties and imperfections were anticipated in getting the law into operation. It is the more gratifying, therefore, that I am enabled to say that, although there are many imperfections, and some very gross negligence, yet, altogether, the enterprise thus far has been eminently successful—it is believed much more successful than in any other State during the first year.

“Having ascertained about the middle of September that, with the clerks in my office, it would be impossible for me to digest and arrange the assessors' returns in a proper manner within the time prescribed by law, and as there were very serious difficulties attending the subject, without the personal supervision of a person versed in the phrases of the medical profession, I procured the services of W. L. SUTTON, M. D., of Georgetown, under whose supervision and direction this report has been prepared.”

In a communication addressed by Dr. Sutton to the Auditor, transmitting his report, together with the assessors' books, blanks, &c., he refers to many of the difficulties encountered in the initiatory stages of a registration law. We make from it the following extracts for the benefit of those in other States who may follow in the same track:—

“I received returns from every county in the State. These returns are very generally made in a form highly creditable to the gentlemen who made them out. There are doubtless many omissions in the returns (perhaps from every county), yet, upon the whole, they are much nearer being full than could reasonably have been anticipated at the first effort; and it is believed more full than those produced from any State heretofore at the commencement.

“In the returns from various counties, there have been great difficulties in determining the colour, and sometimes the sex, in particular instances. After

the tables had been nearly completed, it was ascertained that the number of births and of deaths returned from Simpson County was the same. This gave reason to think that there might be a mistake somewhere. Accordingly, upon examining the returns, it would seem that the returns made to the Auditor's office from that county, consisted of the original return of the assessor as to births, and the same copied into the schedule for deaths, as far as one schedule could be made to agree with the other. It is therefore *highly probable* that all the tables are wrong, so far as the deaths of Simpson are concerned. Nevertheless, as they appear in the proper schedule, I have permitted them to remain in the tables.

"With the schedule of deaths from Meade County, I have not been able to do anything. There are, indeed, entries under all the heads of the schedule; but the one-half of each entry is evidently wanting, and no possible connection between the entries on opposite pages can be perceived. This schedule is therefore thrown out.

"From these causes, and from the inherent difficulty in making so many calculations, and constructing so many long tables, this has been an exceedingly laborious and perplexing piece of work; nor dare I say that there are no errors in it. I do say, however, that no pains which the time allowed have been spared, either by myself or the gentlemen who assisted me, to make it as correct as possible; and it is confidently believed that no serious error, or one at all affecting the general result, exists.

"Perhaps the difference between the number of cases of hydrothorax in table IX. from that in table X. will most readily attract attention. It is explained thus: Ten cases of dropsy of the heart are in table IX. placed with dropsy of the chest, whilst in table X. they are placed more appropriately with disease of the organs of circulation. Again, under the head of accidents, three are returned in table IX. which in table X. are returned, 'killed accidentally.' So, also, the gentlemen engaged in making out the tables could not always keep in remembrance the distinction between disease of an organ and inflammation of that organ, and used one term for table IX. and the other for table X. In this way a few errors crept into the report, which were not detected until the tables were finished, and could not then be corrected without referring to the assessors' returns. Again, in classifying those deaths which occurred from convulsions, spasms, fits, and epilepsy, some discrepancies in the two tables are observed—spasms and fits being rather vaguely divided between convulsions and epilepsy. The errors from all these causes, however, are inconsiderable in number, and much more so in importance.

"For the better understanding of the subject, the population of each county, divided into white and coloured, according to the census of 1850, is introduced. It would have been desirable also to have added the population of each county, as indicated by the census of 1840, so as to show the relative increase of the population of different portions of the State; but it was thought that, as there had been twelve new counties formed since the census of 1840 was taken, and as the population of nearly or quite half the counties in the State had been materially affected by the formation of these new counties, nothing satisfactory or definite could be learned by inserting it. It is therefore not inserted."

The great amount of labour expended in the preparation of this report from the crude materials furnished may be imagined by the fact that the tabular statements constitute over one hundred closely printed pages.

The population of Kentucky consists of whites and blacks in the proportion of about 7 of the first to 2 of the last. In 1850, it was 764,688 whites, and 220,717 blacks. In his first table, Dr. Sutton shows the number of white births, male and female, in each county of the State, as returned by the assessors; the number of coloured births, with the sexes; the number of marriages as returned by the assessors, and also the number of licenses for marriages issued in each county during the year 1852; the deaths of whites and blacks, with the sexes; the proportions of births to the white and coloured populations; the proportion of deaths to the whole population, and the average ages of the whites and blacks at death.

"From this table," he observes, "it appears that the white race is something

more fruitful than the coloured, in the proportion of 1 in 38 to 1 in 40. Whether this is real or apparent, is uncertain, as it is thought that, in the enumeration, births among the blacks would be more apt to be forgotten than among the whites. In estimating the proportion of marriages, the number of licenses issued has been the basis. Where the clerk of the county has not furnished the list, the number returned by the assessor has been substituted. Our laws recognize no marriage between the coloured population, though a marriage ceremony of some kind is very generally observed among them."

Marriages.—There were 7,430 marriages reported in the State in 1852, of which 5,105 were returned by the assessors. The proportion is 1 to every 102.92 of the white population. In some counties, the ratio far exceeds that of others. In Harrison and Jefferson, the proportions are 1 in 50.34, and 1 in 54.90; whilst in Simpson and Livingston, it was 1 in 239, and 1 in 216. There was doubtless some delinquency in making up the returns from the last two counties. The order of the months presenting the number of marriages, beginning with the greatest, is as follows: December, 753; October, November, September, March, August, February, January, April, June, July, and May, 271.

Of 5,105 marriages, 2,160 females, or 40.30 per cent., married under the 20th year. The marriages of females under the 25th year, amounted to 80.13 per cent.

Of males, 2,669, or 52 per cent., married under the 25th year.

Births.—Of these, 25,906 are reported for the year 1852, giving a proportion of 1 to 38.04 of the population, a ratio sufficiently large to render it unnecessary to make any allowance for defective returns. Arranged so as to present the greatest numbers first, the months would stand as follows: December, 2,698; September, October, November, August, March, May, April, June, July, February, January, 1,916. From this statement, it is easy to ascertain the months in which the conceptions took place, by reckoning back nine months previous. The results would be as follows, beginning with the greatest number corresponding with the December births: 1. April; 2. January; 3. February; 4. March; 5. December; 6. July; 7. September; 8. August; 9. October; 10. November; 11. June; 12. May. It must be remarked that this is the result of only one year's observation, and that no allowance has been made for the short months, so as to equalize the proportion due to an equal space of time. This has been done by the celebrated statistician Villermé, of Paris, and was followed in a similar investigation made in regard to the births in Philadelphia, where the rule adopted was to ascertain for each of the short months the average births per day, and multiply this number by 31. By adopting this plan, the monthly Kentucky births and conceptions for 1852, so arranged as to show the highest numbers first, would assume an order slightly differing from that presented in Dr. Sutton's report:—

Births.	Excess of Males.	Months of Conception.
1. December	206	April.
2. September	111	January.
3. November	181	March.
4. October	145	February.
5. August	145	December.
6. February	154	June.
7. March	195	July.
8. April	20	August.
9. May	81	September.
10. July	116	November.
11. January	112	May.
12. June	7	October.

The months of conception are obtained by reckoning back nine months, including the month made the starting-point.

In a short paper published in July, 1848,¹ upon the "Proportions of the

¹ See the American Journal of the Medical Sciences, vol. xxxi. p. 78.

Sexes at Birth," we endeavoured to assign the causes which operated in changing the ratios subsisting between the males and females. We were led to this inquiry by an accidental discovery made whilst examining the Philadelphia bills of mortality, namely, that nine months after the prevalence of Asiatic Cholera in the city, there was a remarkable diminution in the proportion of male births—so much so as even to give a preponderance to the females. Subsequent investigations of the operation of this and other causes which we found operative in determining the proportions of the sexes at birth, led us to the conclusion that disease, exhausting labour, meagre diet, impure air, intemperance, and other social evils, exert depressing influences which manifest themselves in diminishing the proportion of male births, and favouring the preponderance of females; whilst, on the contrary, every agency operating in a community to produce and maintain a high state of physical health and energy, leads to an increase in the proportion of male births. In many parts of Europe, where so many of the people are over-worked and under-fed, the excess of male births is very small. Throughout France and Prussia, it is usually about six per cent., and in England about five per cent. for the whole population. In Philadelphia, the excess of male births is about seven per cent. M. Quetelet, of Brussels, distinguished for his statistical investigations, observes, in one of his publications: "It is a remarkable, but well-established fact, the causes of which are unknown at the present day, that there are annually more births of boys than of girls." We have not, as formerly observed, the presumption to attempt an explanation of the great primary law regulating the distribution of the sexes, but at the same time we believe we have pointed out causes—some of which are under human control—which exert an efficient agency in determining the proportions of males and females at birth in the human race. We believe that all measures tending to promote the health and welfare of a population, whilst serving immediately to increase its capacities for profitable labour, tend also to promote the multiplication of the sex supplying the main physical power. In fact, we regard the increased proportion of males over females at birth, as a sure index of the physical advantages enjoyed by the population in which the births took place.

The data embraced in the Kentucky report furnish the strongest evidence in support of our conclusions. First, it will be seen that they confirm the view thrown out in the paper "On the Medical Statistics of Philadelphia," published so long ago as 1831, that the excess of males at birth, which, in Philadelphia, was found to average seven per cent., would, in the more newly settled States, be found to exceed that ratio considerably.¹ This first report of births from a Western State, shows an excess of male births exceeding twelve and a half per cent. (or 112.52 males to 100 females), being greatest with the black population, in which it reaches nearly fourteen per cent.! The excess of male births is still more remarkable in plurality cases and in the stillborn. Of 230 twins, with the sexes designated, 142 are males, and only 88 females, being at the rate of 161.36 males to 100 females. Of the stillborn, which constitute 3.09 of all the births, the whole number reported is 785, of which 481 are noted as males, and 304 females, making 158.22 males to 100 females. No instance of triplets is reported. A case is recorded of a coloured girl, eleven years old, giving birth to a child.

Now, perhaps there is no spot on the face of the earth where more circumstances are combined to favour the highest development of the animal physique than that embraced by the limits of Kentucky; and the well known superior stature and physical energy of Kentuckians is proverbial. Here, then, is a most striking fact in favour of the positions laid down,² that the excess of males over females at birth is increased by every circumstance tending to improve the physical condition of a population, whilst, on the contrary, everything tending to diminish the standard of physical vigour, tends directly to lessen the preponderance of male over female births. The first part of this law having

¹ See American Journal of the Medical Sciences for 1831, vol. x. note at p. 120.

² In the American Journal of the Medical Sciences, July, 1848, vol. xvi. p. 85.

been so strongly sustained by the new evidence laid before us, it adds greatly to our satisfaction to find equal confirmation of the effects of the depressing forces.

In running the eye along a column showing the excess of male births for each month, we observe this varying from 206 in December to only 7 in June. Now, the three months presenting the smallest excess of male births are: April, 20; May, 81; June, 7—the corresponding months of conception to which are, August, September, and October—the season when the physical forces are most depressed by excessive heat of climate and protracted labour, to which may be added the malign influences of malaria abounding in many situations during the same months, and, in this particular year, a visitation of epidemic cholera. Whilst the excess of male conceptions for August, September, and November, only amount to 108, those for February, March, and April, rose as high as 532. The season furnishing the smallest amount of conceptions also shows the smallest excess in the male over the female births.

To these highly interesting facts deduced from the returns of Kentucky, the mass of the population of which we may set down as rural, we are happy to be able to add some derived from recent returns made from the general registration of births, &c. in Massachusetts. For these, we are indebted to that zealous labourer in the field of statistical inquiry, Dr. Edward Jarvis, of Dorchester. Taking the births in several cities and towns in his State, and comparing them with those of the rural population, the result in reference to the excess of male over female births is as follows:—

	Males.	Females.	Excess of males.	
			Number.	Per cent.
Thirty-five cities and manufacturing towns	30,270	28,594	1,576	5.9
Rest of the State, embracing mostly the agricultural and rural population	26,052	23,951	2,101	8.7

From which it appears that of the births in Massachusetts, the excess of males over females in the cities and manufacturing towns is about six per cent., whilst among the agricultural population it reaches nearly nine per cent. This shows a strong contrast with the results of similar computations made from data collected among the inhabitants of European countries which we regard as subjected to much greater depressing influences than are to be met with in any part of our country. But there is even a greater contrast found between the proportions of the sexes born in Massachusetts and Kentucky than exists between the ratios observed in Massachusetts and England. In the births taking place in the large European cities and manufacturing towns, there is no excess of males in those which take place among the most squalid poor, whilst the proportion for the general population is only about four per cent. Among the rural population of England, the excess of male over female births is about six per cent.; that is to say, the same ratio found in the Massachusetts cities and manufacturing towns. In Kentucky, the excess of males at birth is twelve and a half per cent. of the general population, whilst among the coloured portion, representing the poor population, it reaches fourteen per cent!

Deaths.—The extremely varying ratios in the proportions of deaths to population in many of the counties, can only be explained by great delinquencies occurring here and there in collecting information and making out the returns. That the proportion of deaths to inhabitants should differ in different parts of the State, is a thing to be expected. We have no hesitation in admitting the accuracy of the returns from Hickman, Bourbon, Hart, Oldham, and Marshall counties, which give the largest mortality, varying from 1 death in 34.22 to 1 in 43.54 of the population; but we reject at once the returns from Muhlenburg, Union, and Jefferson, where the deaths reported only make 1 to every 817, 391, and 291 of the inhabitants of the respective counties. Every one, at all acquainted with the nature of statistical investigations, must be aware of the fact that numerous errors are to be found in the collections of data from which he has to work out his results. But the statistician contents himself with knowing

that, as a general rule, occasional errors are neutralized by the multiplication of observations. Without such considerations, we should often be disposed to throw aside publications containing statements with obvious errors, a remark particularly applicable to the reports made at the commencement of almost every registration system. Enterprises which have developed into great importance, have had very faulty beginnings. But time and proper attention have gradually brought them to perfection.

For statistical purposes, Dr. Sutton informs us:—

“The population of a county has been divided into two classes, the supporting and the supported. In the first class are placed all between 15 and 60 years; in the second, all below 15 and over 60 years. Of the mortality in Kentucky, 39.39 per cent. belonged to the first class, and 60.61 per cent. to the second. In Massachusetts, in 1851, 37.66 belonged to the first class, and 62.34 per cent. to the second.

“Of those who died, 6,658 were males; 6,175 were females, and 215 sex unknown; being an excess of 486 males over the females, or 52 per cent. males to 48 per cent. females. In Massachusetts, in 1851, the deaths were larger among the females in the proportion of 51 to 49 per cent. It will be remembered that the females preponderate in the population.

“Of 12,361, whose ages are specified, 4,985 or 40.33 per cent. died under 5 years of age. This is within 1 per cent. of those dying of the same ages in Massachusetts. The proportion of those dying in advanced age is small, and yet, 12 (2 white males, 2 white females, 2 black males and 6 black females), over 100, are reported. The most advanced age was 110, a white female. In Massachusetts, the whole number of centenarians was 5.

“The month is designated in 12,357 instances, of which the greatest mortalities occurred in August, 1,805; September, 1,574; July, 1,432; October, 1,205; December, November, June, March, April, May, January, and February, which last afforded 643. In March, there was an excess of females. In May, February, and January, the mortality was equal, or nearly so. In August, in which the mortality was greatest, the male excess was 163. In July, which was the third in mortality, the male excess was 87; whilst in September, the second in mortality, it was only 23.”

“By examination, we find that the hand of death lies heavier upon the coloured race. They die in greater proportion, and at an earlier age. The proportion of deaths is 1 in 65.57, whilst of the whites it is 1 in 76.03. This proportion, though greater than that of the whites, is yet greatly less than the mortality among the black population of Philadelphia—the only comparison which it has been convenient to make. By a paper on the vital statistics of Philadelphia, published in the *American Journal of Medical Sciences* in 1848, the average mortality was stated to be, among the black population for ten years (from 1820 to 1830), 1 in 21; and for ten years from 1830 to 1840, 1 in 31—greatest mortality, 1 in 22½; least, 1 in 38½.”

“Again, the tables reveal two facts, which appear somewhat contradictory. 1. The proportional mortality is small, even allowing what would be thought a sufficient latitude for omissions by the assessors. 2. The average age at death is low. This may be accounted for if the census should show a large proportion of very young persons among us.

“Being desirous to discriminate the relative degrees of mortality, as indicated by the registration returns, I thought it advisable to do so in connection with the geological formation of the State, as at present understood. For this purpose, I procured a copy of the map presented by Professor Peter, in his report to the Society, and have made spots upon it, which, without interfering at all with the geological delineations, will, it is hoped, indicate distinctly the relative mortality. The diamond-shaped spots, thirty in number, indicate a mortality of less than one per cent.; the triangles, twenty-five in number, indicate a mortality of more than one and less than one and a half per cent.; the squares, thirty-four in number, indicate a mortality between one and a half and two per cent.; and the round, ten in number, indicate a mortality over two per cent. Meade and Powell counties are not marked, because Meade fur-

nished an account of her deaths which it was impossible to understand; and Powell having been created since the census of 1850 was taken, nothing is known of her population. If her population and mortality are merged with the three counties of Clarke, Estill, and Montgomery, from which she was formed, the whole group will be characterized by a square."

Causes of Death.—In classifying these, Dr. Sutton has adopted the system laid down by the American Medical Association:—

"Of 13,043 deaths reported for the year 1852, 10,411 have the causes assigned, leaving 2,637 in which the causes are not designated. This, says Dr. Sutton, being something more than one-fifth of all the deaths, would seem to be a large proportion; yet, when we reflect that it is the starting of our registration, and that very many physicians, and the whole mass of the people were ignorant, both as to their duty under the law, and as to what was expected at their hands, we ought to be satisfied.

"In examining table X., we shall be forcibly struck by the fatality occasioned by class I, ZYMOTICS. This gives a mortality of 56.43 per cent. of all ascertained causes of death. This class is large in all countries, but very rarely assumes the magnitude here displayed. In Massachusetts, it has ranged from 27 to 38 per cent.—the average for 10 years being 30.42 per cent. This class contains diseases which it is thought can be very much guarded against by proper sanitary regulations, and therefore ought not to be permitted to produce such an awful mortality.

"At the head of the alphabetical list in this class, we have *Cholera*, which is represented by a mortality of 722, or 6.94 per cent. By running the eye along the list of counties, it will be seen that this disease prevailed to any considerable effect in only a few counties. A few cases of cholera morbus are included under this head.

"Next comes *Cholera Infantum*, which twenty or thirty years ago was a great outlet of infant life. Of late years, the mortality from this cause has very much diminished; but 135 cases, or 1.30 per cent. of deaths, is ascribed to it.

"*Croup* occupies a more prominent place, having caused 461 deaths, or 4.43 per cent. of deaths.

"*Diarrhœa* occupies but little space; but *Dysentery* stands forth in bold relief, no less than 1,923, or 18.47¹ per cent., having died of it.

"Of *Fever*, intermittent, remittent, and continued fever, the last of which contains typhus and typhoid fevers, we may speak in a group—continued fever having produced a greater mortality than any disease, except dysentery; and if we include (what probably enough belong there) all entries under 'fever' simply, we have no less than 13.09 per cent. of deaths from all ascertained causes. Remittent fever, which includes all entries under the heads of bilious fever, congestive fever, and one of yellow fever, gives a mortality of 2.07 per cent.

"The fevers above mentioned, with cholera, cholera infantum, diarrhœa, and dysentery, constitute the great outlet of life, affording as they do 43.27 per cent. of all deaths. It is generally admitted that these diseases are dependent upon the same causes, more or less modified; and further, that these causes are very considerably under the control of man. Why, then, should they not be looked to and obviated?

"It is true, that a difference of opinion exists among the best informed medical men, as to the cause of these diseases; but this difference of opinion, so far from inducing us to look upon the subject as inscrutable and unsusceptible of demonstration, ought rather to stimulate us to more numerous and more exact observation. Surely, the proper elucidation of a subject upon which the life and health of so great a number of human beings depend, is worthy of a serious, thorough, scrutinizing investigation. It is very generally believed

¹ The per cent. of each disease was calculated before the returns from Letcher County came in. This slightly increased the per cent. of dysentery, but did not sensibly affect any other disease.

that exhalations from decaying animal and vegetable matter, imprudent indulgence in eating, and improper exposure to vicissitudes of weather, have very much to do in the production of these diseases. Whilst such is a common conviction, surely the dictates of common prudence would impress us with the propriety of avoiding these causes. Every one knows that cholera, cholera infantum, and dysentery, are very apt to occur in the latter part of summer, especially upon a sudden diminution of temperature during the night, after hot weather. Now continued heat tends to debilitate the digestive powers. A sudden chilling of the surface of the body, under these circumstances, adds a greater burden than the digestive powers can bear, and vomiting, or purging, or both, take place. Again: every one who lives in a section of country where typhoid fever prevails, has seen large numbers of a particular family prostrated by that fever, whilst the neighbours enjoy good health. It may be true, that nothing about the premises *appears sufficient* to account for the sickness; but it is there. Diseases no more appear without a cause than corn grows without being put in the earth.

"Investigations as to the causes of diseases are more apt to be instituted and prosecuted in thickly settled communities, especially in cities, than in rural districts. Such investigations have taken place, and seem satisfactorily to demonstrate, that accumulations of filth, together with an obstruction of ventilation and light, are almost always attended by a great mortality in the shape of cholera, fever, dysentery, or some other disease belonging to the Zymotic class.

"We hear much of the filth, sickness, and death of other countries, and sympathize with the sufferers, but fondly and as vainly hope that these scenes are confined to other lands, and are unknown in our happy country. A better acquaintance with existing facts, however, will dispel the illusion, and show us that the worst state of things in the old world has perfect parallels among us. We will adduce but a single example, yet one which it is hoped will set the matter in a strong light.

"We have been accustomed to look to Philadelphia and Boston as model cities, where everything is kept neat and clean, and where a man might very comfortably spend a lifetime. It turns out, however, that even Boston has its dark spots. Three portions of Boston are given as points of comparison. In the first, 'Beacon Hill,' is high, well drained, by nature, the houses large and well ventilated; population 2,054, mortality 1 in 74.71. In the second, 'Black Bay,' new made land; the streets narrow; sewerage and drainage imperfect; the houses small, built around alleys as well as on streets; population, 5,121, two-thirds foreigners; mortality 1 in 52.07. In the third, including Broad, Cove, and Lea streets; built principally upon made land, and have numerous blind alleys leading into them; the streets and alleys are badly drained, and crowded with an overflowing population; a large number of the houses have no means of sewerage whatever, and all their refuse, of every description, stagnates about the yard, spreading in every direction poisonous exhalations laden with disease and death. A majority of the houses contain several families, some of them nine or ten. Even the cellars of the houses are often inhabited; and, in some instances, one cellar leads to another, and this to a third—a sort of dungeon—all inhabited by human beings, of both sexes, and of every age. The population of these streets is 2,813, of whom 2,738 are foreigners; the mortality was 1 in 17.6 of the population—and this was a year remarkable for its healthiness; what it would have been in a sickly year, we dare not conjecture! One more word, and I have done. A writer, speaking of this same district, says:—

"* * * 'Human beings, men, women, and children, boys and girls, herding together like swine, and like swine wallowing in filth—and worse than swine, steeped in vices which we dare not mention—will be seen by any one who will visit Burgess Alley, and other localities in Broad street district.'

"But, probably, it will be replied, that this still is in a distant State—nothing of the kind exists in Kentucky. This may be true; but who knows that it is true? How many people in Boston know what these corners contain?

If it is true that no such things now exist in Kentucky, how long will it remain true? These things had a beginning in Boston, and it is possible that they have already had a beginning in Louisville. I saw it stated in a paper published in that city, that very frequently houses are leased before their foundations are laid; that families, upon arriving there, frequently had to board a considerable time before they could procure houses to live in. We must not expect the people of Louisville to be more disinterested than those of other cities. Houses that pay well for the money laid out will be furnished to all sorts of tenants, and we shall soon have the poor crowding together to save the expense of rent at the risk of health and life. This is the beginning of cupidity on one hand and seeming necessity on the other, which lead to the creation and occupancy of tenements which prove a snare and a curse to both body and soul of those who are tempted to occupy them. Now, before the thing is commenced, or at least before it has made much progress, the legislature should utterly prohibit the erection of such habitations. But it may be said that these considerations pertain to large cities, and the great bulk of the country have no interest in them. It is replied, that the time is not far distant when one-twelfth or even one-tenth part of the population of Kentucky will be gathered together in the cities of Louisville and Covington. Again, the same principles as to health and disease apply to both city and country; what is true of one is true of the other. The principle, and the proper application of it, are what we want.

"Scarlet fever did not cause many deaths in 1852—a fraction more than one in each county on an average. Hooping-cough and measles each caused more than thrice as many deaths as scarlatina, and about a fair proportion.

"Milk sickness is now for the first time introduced into a classified table, and this seems the most appropriate place for it. Nevertheless, many physicians in the West doubt whether it is a distinct disease, or is a modification of bilious fever. The present returns show a much less mortality from it than was expected.

"Croup gives as usual a large proportion, 461, or 4.43 per cent. of the mortality. The other diseases of this class exhibit a small mortality, thrush being the greatest in proportion to what would be expected.

"Class II. Diseases of general or uncertain Seat, composed of a considerable number of diseases, gives a small per cent. as a class, being only 5.78 per cent. Of this class dropsy produced the greatest mortality, 230 or 2.21 per cent. Next comes scrofula, which caused 155 deaths or 1.49 per cent.

"Class III. Diseases of the Nervous System, gives a small proportion, 6.69 per cent.; inflammation of the brain, 242, and convulsions, 130, being the only diseases which cut much figure. Paralysis follows, giving a mortality of 68 or .65 per cent. *Trismus nascentium* is introduced, because it is generally believed to be a considerable outlet of life, especially among the coloured race. The proportion returned under it, however, is very small—only 4 in all.

"Class IV. Diseases of the Respiratory System, as in all countries, gives a considerable mortality; and, as in other countries, consumption and pneumonia make up a very large proportion of the whole class. Whilst the whole class, 17.27 per cent., and consumption 9.20 per cent., are considerably less than an average, in Massachusetts, pneumonia, 5.74, is nearly 1 per cent. greater.

"Class V. Diseases of the Circulatory System, consisting almost entirely of diseases of the heart, gives only .68 per cent. of deaths; 10 deaths from dropsy of the heart are classed in the alphabetical list under 'dropsy of the chest,' and 2 cases of malformation of the heart are included."

"Class VI. Diseases of the Digestive System, gives a smaller per cent. than might have been expected, only 4.34 per cent. Enteritis gives 112 deaths, or 1.08 per cent. Worms 105, or 1.01 per cent. No other disease amounting to one-half of one per cent.

"Class VII. Diseases of the Urinary System, is quite small. Gravel, which gives but 16 cases, being nearly one-half of the whole class.

"Class VIII. Diseases of the Generative System, gives a rather large com-

parative proportion, 130, or 1.25 per cent. The principal mortality was occasioned by puerperal fever, 63, or .61 per cent. Childbirth caused 50 deaths, or 1 in 517.56 births; perhaps a large proportion. By comparing the deaths from this cause it appears that 36 were whites, or 1 in 562 births; and 14 were coloured, or 1 in 387 births. We find that puerperal fever caused 1 death to 413 births among the whites, whilst it caused just the same number among the coloured as childbirth, 1 in 387.

“Class IX. Diseases of the Locomotive System, caused few deaths—57 in all; of which 37 were from rheumatism.

Class X. Diseases of the Integumentary System, gives 15 deaths in all.

“Class XI. Old age, gives a rather small per cent., 2.35. By referring to the alphabetical list of deaths, we find some rather strange entries, viz. 1 death under 5 years, and 1 between 5 and 10. It is presumed that the assessor made an erroneous entry, either having no ink in his pen when he attempted to make a figure, or else making the entry on the wrong line.

“Class XII. Deaths from External Causes, furnishes about a fair proportion, 4.20 per cent. This class is a tolerably fair barometer of the carefulness, sobriety, and good order of a community. As compared with the report of Massachusetts for 1851, the list of accidents, and persons killed accidentally, are, together, less than the accidents in that report, as 293 is to 138. Burns and scalds furnish a list as 59 is to 78. Drowned is much smaller, as 56 is to 236. Indeed, it seems strange that our State, bounded for two-thirds of its circumference by the Big Sandy, Ohio, and Mississippi rivers, and intersected by the Little Sandy, Licking, Kentucky, Salt, Green, Cumberland, and Tennessee rivers, besides numerous mill-streams, should not furnish a larger list from this cause; especially when we consider the great proneness of our coloured population to travelling at night.

“But one person is returned as having been executed.

“Eight are reported as having been frozen, and, as under old age, we have the strangeness of having three of the eight frozen in April, May, and June. It is presumed that these errors were made in transcribing the assessor's books, by the clerks. Intemperance is charged with 28 deaths against 41. Killed by design is charged with 31 deaths against 6 murdered in Massachusetts. It may be well to remark, that in this table all the killed are set down as accidentally done, except from one county, in which a deadly feud was known to exist between two families, which feud was known to have caused several deaths during the year; all the killed in this county, amounting to 5, are presumed to have been killed by design.

“It must be acknowledged this list of killed by design, 31, looks badly for an orderly people. It proves very conclusively that the people of our State have not that control over their passions which rational Christian people ought to exert. Admitting that a large majority of these homicides were in self defence, it yet proves that there is a very unfortunate spirit of aggression in the land, which alone could have warranted the taking of so many lives. The suffocated, which includes smothered and strangled, seems to be greater in number than should be. The same is true of the poisoned; whilst the suicides were small in number.

“The object of this CLASSIFIED TABLE is to show the number who died of each particular disease, in each county in the State, without reference to colour, sex, or age. But the ALPHABETICAL LIST is intended to exhibit the mortality of each colour, sex, and age, throughout the State, from each disease, and the month during which those deaths took place.

“To Kentucky belongs the credit of having first instituted a series of observations upon a large scale, the object of which is to ascertain the relative healthfulness, fecundity, and longevity of the white and of the coloured population. Nothing can be inferred from the returns of a single year, especially when that year is the very beginning of observations; but the anticipation is now hazarded, that, when observations shall have been sufficiently numerous and long continued to authorize the formation of a judgment, it will be found that, so far as health and longevity are concerned, the slaves in the slaveholding

States are in a better condition than that class of any nation which is nearest assimilated to them in social position.

"Even if it were allowable to draw conclusions from a single year's experience, time at present does not permit any extended analysis. Yet it may be permitted, as a means of directing attention to the subject, to advert to the great proportional mortality among the coloured race from certain causes, as convulsions, tetanus, worms, burns, and scalds; whilst there are other causes affecting the whites to a proportionally great degree, among which stand out in bold relief, dysentery and 'killed.'" G. E.

ART. XIII. *Lectures on the Diseases of Infancy and Childhood.* By CHARLES WEST, M. D., Fellow of the Royal College of Physicians, Physician to the Hospital of Sick Children, Physician-Accoucheur to, and Lecturer on Midwifery at, Saint Bartholomew's Hospital. Second American from the second and enlarged London edition. Philadelphia: Blanchard & Lea, 1854. 8vo. pp. 486.

THE high estimate of these lectures, formed and expressed by us upon their first appearance in 1848, is in no degree diminished after an attentive perusal of the second and enlarged edition now before us.

The views laid down in relation to the pathology and therapeutics of the diseases, embraced in the lectures of Dr. West, being based mainly upon his own observations, carefully collected during a series of years, in an ample and favourable field for the clinical investigation of the leading maladies incident to the period of infancy and childhood, render the volume before us one of great value to the student and practitioner.

In the preparation of this second edition, the entire work bears the evidence of having undergone a careful revision, while numerous additions, the result of the author's more extended investigations, and more careful reflections, have been made to several portions of it.

The work now comprises the results of 640 observations, and 199 *post-mortem* examinations, chiefly made among 16,276 patients who came under the author's notice during the ten years of his connection with the Children's Infirmary in Lambeth.

The lectures of Dr. West devoted to the consideration of the affections of the respiratory organs, as they occur during infancy and childhood, are particularly excellent. They furnish a clear, satisfactory, and able exposition of the symptomatology, pathology, and therapeutics of those frequent and too often fatal maladies of the earlier years of existence, and are deserving of an attentive study on the part not merely of the medical pupil, but of the practitioner also. The directions for the treatment of croup, we would especially urge upon the attention of the junior members of the profession, but, at the same time, with the sensible caution with which the lecturer premises his examination into the pathology of that disease.

"With reference," he remarks, "to many of the more important points in the history of the malady, writers are now, indeed, pretty well agreed; but croup, like many other diseases that depend to a great degree on atmospheric and telluric causes, is modified in many of its symptoms by peculiarities of air, water, and situation. The affection assumes one character among the poor of a crowded city, and another among the children of the labourer in some rural district. If, therefore, you find that my account of the disease varies in any respect from the description given by some other writers, or from the results of your own observation hereafter, do not too hastily assume either that your teacher has been mistaken, or that your own observation has been incorrect. The difference may be nothing more than a fresh exemplification of the old story of the shield; silver on the one side, and golden on the other, about which the knights in the fable quarrelled."

This admonition might, with great propriety, be repeated in reference to other of the diseases of early life besides croup.

The several diseases in the portion of Dr. West's lectures just referred to have evidently held a prominent place in the clinical investigations of the author. The thirteen lectures devoted to their consideration occupy 156 pages, or over one-third of the entire volume.

The affections of the brain and nervous system that ordinarily occur during infancy and childhood are, also, ably treated by Dr. West. We could have desired, it is true, a somewhat more detailed investigation of some of the points connected with their pathology than that given by the author. There is no class of the diseases of early life in relation to the true character of which the inexperienced practitioner is more liable to be deceived—none in reference to which a close attention to the value of particular phenomena is of greater importance.

The eleven lectures on the diseases of the brain and nervous system occupy 120 pages, or about one-fourth of the entire volume.

The account given by the lecturer of the diseases of the heart, and of the urinary organs, of the acute and chronic forms of peritonitis, and the fevers, including smallpox, chicken-pox, measles, and scarlatina, are in general full and satisfactory.

The lecturer's account of the diseases of the digestive organs of children, are, perhaps, the least satisfactory of any in the course. We find no particular fault with the views advanced in relation to the pathology of these affections, and agree with him in the general outlines of the course of treatment he lays down; but there are many points of importance in relation both to their pathology and management he has scarcely touched upon, of which a more detailed exposition would seem desirable to the student and young practitioner.

Upon the whole, however, Dr. West has ably executed his task. Comprising the results of extensive personal experience and matured reflection on the part of, evidently, an acute and cautious observer, his lectures have that degree of interest and of authority which strongly press them upon our attention. They already constitute a standard work of reference on the diseases of which they treat: while, from the enlarged and still more favourable opportunity for clinical observation afforded the author, by his appointment as one of the Physicians to the London Hospital for Sick Children, and his earnest devotion to the cultivation of medical knowledge, we anticipate, as renewed editions of the work shall be demanded, to derive, through its pages, a still more intimate acquaintance with the several maladies of early life, and with the means best adapted to conduct them to a favourable termination.

D. F. C.

ART. XIV.—*The Diseases of the Heart and the Aorta.* By WILLIAM STOKES, Regius Professor of Physic in the University of Dublin, Author of "The Treatment and Diagnosis of the Diseases of the Chest," &c. Philadelphia: Lindsay & Blakiston, 1854. 8vo. pp. 689.

A WORK embodying the results of the author's clinical observations, "continued almost unceasingly for upwards of a quarter of a century," can hardly fail of attracting attention; but when it is remembered that this author is Dr. William Stokes, and that this long experience is that of one whose medical perception, veracity, and skill, have placed him in the highest seat in the British School of Medicine, we should be grateful that the profession can still profit by his instruction, and humanity by his labours. We would fain dwell upon the personal traits of our distinguished author, although in these the medical public are not directly interested; yet we cannot avoid thinking that the personal character of a man has much to do with the confidence we should award to his written opinions. Who, then, that has been privileged to listen to his teachings,

and enjoy his social intercourse, that will not bear willing and enthusiastic witness to his noble qualities. Affection, admiration, and respect, are the ready tribute to those rare excellences of heart and head, to those native graces of temper, and acquired treasures of wisdom, by which so many have been delighted, taught, and enriched. His book is a reflection of himself. Truth without pretension, firmness without dogmatism, originality without affectation, are found upon every page, and give value to every precept. The true object of the healing art is not lost sight of in the minute handling of merely curious details, or in scientific abstractions. The author, himself thoroughly learned in physical diagnosis, does not disdain to acknowledge the short-comings of this mode of investigation. The safety and cure of his patient is the object which he keeps constantly before him; disease, he describes as he finds it at the bedside, and not as it is abstractly when divested of its attendant complications. He says: "The diagnosis of the combinations of diseases, even in so small an organ as the heart, is still to be worked out; and, until this be done, the rules of physical diagnosis founded on the presumed isolation of disease, must be used with great caution. I cannot, even at the risk of being charged with under-rating the position of physical investigation at the present day, avoid expressing my opinion, that a too great positiveness marks some of the statements in our standard works, and that the difficulties of special diagnosis are still infinitely greater than many might be led to believe."

We propose, in the following pages, to give the reader a general notion of the contents of this treatise, premising, however, that any notice, however full, can give but an insufficient idea of a work, one of whose chief merits lies in its condensation.

Inflammation of the Heart and its Membranes, is the subject of the first chapter. Pericarditis, naturally, occupies the first place, since the occurrence of either carditis or endocarditis separately, without the pericardium being involved, is rare and exceptional. It is considered under two aspects, first, in combination with general or local disease (except that of the substance or lining membrane of the heart), and secondly, as complicated either with rheumatic, intermittent, or typhus fever, gout, phlebitis, or dropsy; or again, when the disease is of a local character, with typhoid inflammations, fatty degeneration, chronic hypertrophy, and ulcerative perforation. In the diagnosis of pericarditis, Dr. Stokes naturally places more reliance upon physical investigation, than on the general symptoms of the disease, these being very inconstant in their character. Pericarditis, he says, is one of the most frequent of the unrecognized and often harmless diseases which affect the human body, but, at the same time, it may be both latent and dangerous. In those cases where the danger is not revealed by the functional signs, and which are complicated with rheumatism, Dr. Stokes says (p. 47), "That though the combination of pericarditis with acute articular rheumatism is common, yet, that the disease of the heart is more closely related to the rheumatic fever than to the inflammation of the joints;" and "every variety and degree of pericarditis may occur in connection with acute rheumatism, from the simple, dry, latent pericarditis, to the worst forms, combined with inflammation of the endocardium and muscular structure." In his account of the general symptoms of this disease, Dr. S. dwells upon the difference between the oppression of breathing in pericarditis and in pleuritic effusions. The tolerance of great effusion is much less in the former disease, and is not acquired, as it is in pleurisy or empyema. His clinical experiments upon the *pulse*, establish that no special condition of the pulse can be distinguished as belonging to any one form or stage of the affection. The *physical signs* are discussed with great perspicuity, and the practical importance of keeping in mind the varying pathological conditions upon which they depend, is made very apparent to the reader. Thus, although the first stage of pericarditis may be unattended with the friction signs, yet this stage is of short duration; and, if the disease be mild and simple, its discovery in the first stage is of comparatively little importance; while, if the disease be of a violent and dangerous kind, it will be indicated by symptoms. The absence of friction signs, in the early stages is, however, no indication of the safety of the patient, since several days may elapse before they

appear. We cannot follow the author in his discussion of these signs, their causes, and physical characters. In reference to the error sometimes made, in which the signs of pericarditis have been mistaken for those of diseased valves, he says: "Their sudden supervention in a case where they had never before existed, the accompanying sign (when present) of the rubbing sensation communicated to the hand, the rapid change of situation, the equally rapid modification by treatment, and the occurrence of the signs with both sounds of the heart, in a case which previously presented no evidence of organic disease, form a combination of circumstances which can hardly mislead." Dr. S. also refers at length to the modification of the friction sounds produced by the presence of air in the pericardium, derived either from the stomach or lungs by fistulous communication. In a case related, he found "a mixture of the various attrition murmurs, with a large crepitating and gurgling sound, while to all these phenomena was added a distinct metallic character." The extraordinary loudness and singularity of the sounds was such that the patient and his wife, who occupied the same apartment, were unable to obtain a moment's repose. This case, Dr. Stokes considers to have been one of pure pneumo-pericarditis. Obliteration of the pericardium by adhesion of its surfaces after inflammation, does not, according to the author's experience, entail upon the heart serious diseases. Hypertrophy and dilatation, do not necessarily, or even commonly follow it; he says: "I have often found the heart in a perfectly natural condition with the exception of an obliterated pericardium."

Treatment.—Dr. Stokes is not an advocate of an indiscriminate or too active antiphlogistic treatment in pericarditis. He thinks that the possibility of weakening the general system and the muscles of the heart, is too often overlooked, and that too free and repeated bloodlettings are unnecessary and dangerous. While the impulse of the heart continues vigorous, the danger is indeed remote; but weakness of the heart, like that of the diaphragm and intercostals in pleurisy, may supervene in a sudden manner. "In pericarditis, this accident is of great danger, threatening paralysis of an organ which is the fountain of life." The author places his chief reliance on local depletion by means of leeches, put on in relays, and alternated with the application of warm poultices, and aided by the administration of calomel in doses of from ten to twenty grains. This mode of giving mercurials, which is particularly recommended by Dr. Graves, should be continued until the violence of the inflammation has abated. Stimulants are, he thinks, often absolutely required, after the first violence of the disease has been subdued. Among the indications for their use are feebleness, intermittence, and irregularity of the pulse, and the general evidences of a weakened circulation. Dr. S. says nothing of any special treatment in rheumatic pericarditis.

Endocarditis, which is more frequently found in combination with pericarditis than isolated, is, according to our author, less frequently met with in rheumatic fever than the latter. "Its diagnosis depends on the recent production of a valvular murmur, under circumstances indicative of cardiac irritation, or the existence of special morbid states of the system, which predisposes to inflammation of the heart." He is far from being willing to attribute all valvular lesions to this cause, since organic changes affecting the integrity of the valves may spring from an infinite variety of sources, independent of inflammation. "In practice, the disease may be considered in the following forms, which are given in the order of their frequency. First, it may accompany, follow, or precede an attack of pericarditis. Secondly, as occurring without pericarditis, when it is in general manifested by symptoms of cardiac irritation, with signs of recently formed valvular disease. The absence of pericarditic signs may be owing to the actual want of any peripheral irritation, or to the obliteration of the sac by previous disease. Thirdly, we find that, without any symptom which would lead to the suspicion that the heart was diseased, endocarditis may be insidiously and silently developed in the course of rheumatic fever. Fourthly, symptoms of irritation of the heart may occur in a case where the organ has been previously diseased. These may be shown either by an increase in the violence of the old, or in the production of new symptoms; or lastly, by making manifest

the signs of former organic disease, which, up to the period in question, had been unrecognized or wanting. Fifthly, symptoms of cardiac irritation may be developed, unattended by any evidence of valvular lesion. This form is of rare occurrence, and I put it forward with diffidence; but, I have seen cases which could not be explained upon any hypothesis, except that of the absence of murmur in endocarditis."

Diseases of the Valves of the Heart.—The analysis of the contents of this chapter would lead us far beyond our limits. There is no branch of his subject which the author has treated with more ability and earnestness, nor which presents so many features to arrest the attention and interest and instruct the mind. While avoiding, as much as possible, the discussion of purely theoretical questions relative to the mode of production of the sounds of the heart, he does not hesitate to communicate freely his own impressions. He seems impatient, however, to break away from the discussion of abstract questions, and gain from experience all the results he can to enlighten the dark points of cardiac pathology. He endeavours to affix a practical value upon the alterations of the sounds, reducing them to exponents of the vital and mechanical condition of the whole organ. In these are found "the key of cardiac pathology; for no matter what the affection may be, its symptoms mainly depend on the strength or weakness, the irritability or paralysis, the anatomical health or disease, of the cardiac muscles." "It too often happens, when the existence of a valvular disease is determined, that great labour is expended in ascertaining the exact nature and seat of the affection. Long and careful examinations are made, to determine whether the disease exists at the right or left side of the heart; whether it be a lesion of the mitral, tricuspid, or the semilunar valves; a contraction or dilatation; an ossification; a permanent potency or warty excrescence. Now, though in some, we might say in many cases, these questions may be resolved with considerable accuracy, it is also true that in a large number their determination is of comparatively trifling importance; and the two great practical points to be attended to are, first, whether the murmurs really proceed from an organic cause, and next, what is the vital and physical condition of the muscular portions of the heart; for it is upon these points that prognosis and treatment must entirely depend." Dr. Stokes furnishes at the end of this chapter a very complete "recapitulation" of the important points discussed or established in it; and, in an appendix, gives in full, the views of Skoda as to the causes of the sounds of the heart, since they agree to a certain extent with his own.

Diseases of the Muscular Structures of the Heart.—We pass over the pages devoted to a consideration of the simple and complicated dilatation of the heart to arrive at the chapter on *fatty degeneration*, which is one of the most instructive, interesting, and complete in the work. This disease is divided by Dr. Stokes into two forms, in the first of which fat is found under the subserous cellular tissue, and envelops the heart, and in the second the true muscular structure of the heart is converted partly or wholly into adipose matter. The distinction between the two forms is not considered as rigorous, since the last described may ensue upon the first, and, in a practical point of view, the history and general symptoms are much the same. When, however, the disease exists to such an extent that but little of the muscular structure remains, rupture of the heart is more likely to occur. In addition to this fatty transformation, in some cases, free oil is observed in the blood, and also in the liver and bony structures; a fact of some importance, perhaps, in connection with the theory of the origin of the disease, and, perhaps, also with those cases of unusual inflammability of the body which have given rise to the notion of spontaneous combustion. This circumstance has received particular attention from Professor Smith, of Dublin, who has made also some valuable observations upon the development of air in the veins before death and after it, but previous to putrefaction. These observations have been confirmed by Dr. Stokes.

The general diagnosis of fatty degeneration is founded upon the signs of a

weakened heart, the occurrence of attacks of pseudo-apoplexy, and a peculiarity in the respiration appearing to depend upon a deficiency of power in the right ventricle. Of these symptoms the last two are the most interesting. Those referable to the brain are explained by Dr. Stokes to consist in attacks somewhat resembling those of apoplexy, but differing also from them in the frequency of their repetition, the rarity of paralysis, and the fact of danger from antiphlogistics and benefit from the use of stimulants. The attacks are sometimes apparently purely syncopal, but in the more decided cases are preceded by lethargy, loss of memory, and attended by sudden coma. The peculiar sighing respiration, which the author next describes, he says that he has never seen except in this disease. "It consists in the occurrence of a series of inspirations, increasing to a maximum, and then declining in force and length until a state of apparent apnoea is established. In this condition, the patient may remain for such a length of time as to make his attendants believe that he is dead, when a low inspiration, followed by one more decided, marks the commencement of a new ascending and then descending series of inspirations." Coexistence of a certain amount of alteration of the aortic valves is common, although not uniform, so that the combination of the slow pulse, a feeble impulse, and a diminished first sound over the left ventricle, attended with a single murmur while the second sound remains clear, will be sufficient for the diagnosis of the disease in many cases. An excellent and slightly philosophical chapter on the treatment of the organic diseases of the heart comes next in order. No one can fail to derive benefit from its perusal. Our author's observations upon "the condition of the heart in typhus fever," and upon "displacement of the heart," are so familiar to most of our readers, that it is not necessary for us to do more than mention the fact that the results of them are here introduced in full.

Deranged Action of the Heart.—The nervous diseases of the heart are divided into those which appear to be simply nervous, and those in which there is some structural disease. Dr. Stokes thinks that *angina pectoris* has probably never occurred in a heart free from organic disease. He does not, therefore, regard it as a purely nervous affection, but says that it is usually found connected with some form of weakened heart. Obstruction of the coronary arteries as a cause of angina, he considers not effective except, perhaps, remotely, by inducing atrophy with fatty degeneration. The author's remarks upon the various forms of nervous disturbance of the heart, as occurring in anæmia, hysteria, gout, and induced by derangement of the stomach, or the use of narcotics, close his account of the diseases of the heart.

Aneurism of the Thoracic Aorta.—Dr. Stokes denies that there are any unequivocal physical signs of this disease. He says that, as a general rule, the sounds are more and more similar to those of the heart, according as the seat of the disease is near the origin of the aorta, but "there are no proper signs by which the pulsations of an aneurism within the chest can be distinguished from those of the heart—none deserving the name of unequivocal." The diagnosis often depends upon making out two centres of pulsation, in connection with other acoustic signs. Aneurismal sounds are divided into those attended with murmur and those in which there is none whatever. Respecting the cause of this variation, Dr. Stokes says, that nothing that can be considered exact is yet known, but the certainty of the frequent absence of murmur in undoubted thoracic aneurism is attested by his own experience. He says, however, that the absence of murmur is not explained by the freedom of the aneurismal sac from compression, nor is it due to the absence of disease of the aortic valves. Cases illustrative of these opinions are given. The general and functional evidence of the existence of aneurism of the chest and its diagnosis from other intra-thoracic tumours are fully considered. It is in such discussions that the wonderful clinical diagnostic talent of the author is conspicuous; one cannot too much admire the skill and perspicuity with which the most complicated set of symptoms and physical signs is analyzed and explained.

It is needless to follow the author in his examination of the pathology and treatment of this disease and of abdominal aneurism; a consideration of which, founded chiefly upon Dr. Beatty's case, closes the volume. A treatise like this,

replete with facts, observations and important opinions, upon a subject which has occupied the most acute and practised minds in the profession, can hardly need the imperfect indication of its value which we have given, but which, we nevertheless hope, will induce many to consult it, who may desire a reliable guide in the study of the diseases of the heart and aorta.

M. S.

ART. XV.—*Practical Observations on Gout and its Complications, and on the Treatment of Joints stiffened by Gouty Deposits.* By T. SPENCER WELLS, Fellow of the Royal College of Surgeons of England, Member of the Royal Institution of Great Britain, of the Pathological and Epidemiological Societies, &c., late Assistant Surgeon in Malta Hospital. London: 1854. 12mo. pp. 288.

IN this short treatise, Mr. Wells has presented a very sensible sketch of the pathology and treatment of gout, and its most frequent complications. Although the main object of the author is to direct attention to the iodide of potassium as a remedy exerting a direct chemical action on gouty deposits, and hence, one calculated to restore to their former healthy condition those tissues of the body that have become the seat of such deposits, the work will be found to contain many interesting hints in reference to the nature and cause of gout; the manner in which it is modified by rheumatism and syphilis; the several varieties of internal or latent gout; the forms under which the disease manifests itself in the female; and the habits, diet, exercises, climates, and remedies adapted to prevent, mitigate, or cure the disease, and to remove its usual sequelæ.

Gout, the author refers to morbid chemical changes produced in the blood by derangement of the process of nutrition; in consequence of which changes certain substances are formed in the blood not found in it in healthy subjects; or, if found, only in very minute portions. To clear the blood of these morbid matters, an effort is made for their elimination, in a state of more or less perfect solution, through the ducts of the excretory organs, or by their deposition upon or within the tissues of the joints, or among the interstices of the fibrous tissue in whatever part of the body it may be found.

"In the present state of organic chemistry," remarks the author, "the exact chemical constitution of the substances found in the blood of gouty persons, and poured off from the blood by means of the skin and kidneys, which would not be so poured off in healthy persons (because not existing in the blood at all, or only so existing in a minute fractional proportion, there is either no possibility of their appearance, or no necessity for their elimination), has not been accurately ascertained. It has been too common to accept assertions which have not been confirmed by trustworthy observations, and the dangerous error has been too often committed of making practice accord with theoretical views, the truth of which has not been positively established. No chemist, known to be experienced in the processes of organic analysis, has made a sufficient number of careful examinations of the blood and secretions of gouty persons, to demonstrate clearly how those fluids depart from their healthy condition, and in what state their unnatural contents are found in different forms of gout, and in the same gouty patient at different periods. This is a subject which still requires to be worked out by careful, trustworthy and experienced chemists, who will assist the medical attendant, and perform for him a duty for which his occupations, habits and education but seldom fit him."

"The chemical characters of the secretions and excretions of gouty patients have been more fully investigated than those of the blood itself. Still, much remains to be accomplished, although the general doctrine may be considered as established, that the blood contains certain substances it should not contain, that these substances must be either thrown off from the blood in the secretions or excretions, or be deposited in or around the joints, or continue to act as a poison in the blood, to circulate with it, and disturb, to a greater or less extent,

the performance of the functions of every organ, the physical characters and chemical composition of every tissue."

Microscopical observations have convinced Mr. Wells that the composition of the gouty deposits in or around the joints, or in some one of the fibrous tissues, varies very much in different persons, and in the same person at different periods.

"When," he remarks, "there is general plethora, or an inflammatory tendency, or when the system is supplied by active digestive organs with superabundant nutriment, lithic acid or the lithates, at times, abound in the urine, at others are deficient; and the lithates, with albuminous matter, compose the gouty concretions. When, on the other hand, the general health of a gouty person has been broken up by long-continued suffering, by mental anxiety, or any other depressing cause—when the digestive organs cannot prepare nutritive matter of good quality, or in sufficient quantity to supply the wants of the system—then the body feeds upon itself; its own half-dead tissues are its food; the phosphates form in the urine, and are deposited around the joints."

As the causes of the derangement of the processes of hæmatosis and nutrition which give rise to the chemical changes in the blood productive of gout, our author enumerates, 1. Hereditary tendency; 2. Undue, or irregular development of the nervous system.

"When," he remarks, "the brain is irregularly developed, irregularly exercised—when it is left in idleness, more or less complete, for one season of the year, and overworked in the other—when the intense study, close attention, violent passion, and alternations of excitement and depression inseparably connected with the practice of gaming, are nightly persisted in—when the very composition of the brain is altered from the circulation of blood through it which has been but imperfectly oxygenated by the air of crowded public assemblies, it cannot regularly develop nervous power; it cannot regularly distribute this power throughout the body. The spinal and sympathetic systems participate in the disturbance, and the consequence is, that the nutritive and absorbent vessels cannot act with precision—their balance is lost—they do not deposit or remove what they ought, and the blood becomes charged with superfluous or impure matter, which must be got rid of in some unnatural manner; by undue excretion, or by deposit in some unusual situation. The tissues and organs, instead of being renovated and perfected, remain partly composed of matter which has been, or ought to have been 'used up,' and they are consequently unable to fulfil their several offices in a proper manner. The derangement of one organ increases that of others. The whole system is deranged, and the evil which was the first cause of imperfection and failure, is increased by its own effects. Thus, in a continual series of reactions, general alteration of the whole blood is produced. Its chemical composition and physical characters are altered. Derangement of every part which is dependent upon a proper supply of pure blood for perfection, and even vitality, ensues. The alteration in the whole of the blood of the body resulting from a loss of balance between the nervous and circulating systems not only concurs with other causes in producing gout, but I believe it to be an important, if not essential element in its production."

The undue, irregular, or imperfect assimilation of nutriment by the digestive organs, in consequence of overfeeding or improper food, is the third cause enumerated by Mr. Wells as influencing and deranging the processes of nutrition, and bringing about that abnormal condition of the blood upon which gout depends. A fourth cause is, the habitual use of alcoholic drinks, whether spirituous, fermented, or vinous. A fifth, the inhalation of a stagnant and impure air; a sixth, deficient, or irregular muscular exercise; and a seventh, the want of attention to the freedom of cutaneous exhalation, from the neglect of daily and complete ablution of the entire surface of the body.

In its general outline, there can be no doubt of the correctness of the pathology of gout laid down by our author. It is the one, with very slight variations, now generally adopted, and which best accords with what we know of the character, phenomena, progress, and sequelæ of the disease. We cannot follow Mr. W. in his explanation of the manner in which the excess of lithic

and other acids in blood is produced; whether the changes of the old tissues into urea or lithic acid takes place in the extreme capillaries of the tissues, or the excess of acid occurs during the process of the sanguification of food. We have, as yet, too limited a series of exact observations and experiments bearing directly upon this question to enable us to arrive at any satisfactory solution of it.

In concluding his remarks on the pathology of gout, the author says:—

“It does not follow because the most marked chemical changes recognized in the blood and excretions in gouty persons are those connected with undue development of lithic acid, that this development is the only change which really takes place, or is the sole result of the operation of the various causes which derange the processes of nutrition. On the contrary, it is difficult to conceive that such causes can operate, for any length of time, without affecting every organ in the body. Whatever may have been the first departure from a state of health, so soon as the organs of secretion and excretion are supplied with impure blood their functions must be deranged. The kidneys have to excrete superfluous nitrogen; they do it for a time, then become incapable of doing it completely, and other organs are called upon to compensate or assist in its elimination, with more or less disturbance of their own proper functions. The liver receives superfluous carbon in the food, and must excrete it, or the elimination of carbon by the lungs being interfered with, the liver must compensate for deficient pulmonary exhalations. It does so for a time, then becomes deranged, and the kidneys are called upon to do what the lungs or liver ought to have done. They throw off large quantities of carbon in the colouring matter of the urine, or in the form of hippuric acid, but they cannot do this without derangement of their own proper functions. In the same way, when the excretion of nitrogen by the skin is more or less suspended, the kidneys must compensate for the deficient cutaneous elimination. They do so, but suffer for it. Thus, whatever may be the primary cause, the general effect in the end is the same—an impure state of the blood is produced by the imperfect performance of some of the various processes of nutrition; the most marked, and only distinct impurity hitherto recognized being an undue accumulation of certain compounds rich in nitrogen.

“The only nitrogenized substances hitherto known to have been detected, are urea and lithic acid, or its compounds; but I am convinced, from observation, that these are by no means the only forms in which nitrogen may be found in excess, for I have observed, in two cases, that when a few drops of the serum of the blood were allowed to evaporate on a piece of glass, and were examined under the microscope, that yellowish crystals, neither resembling the ordinary salts of the serum nor the lithates, were observed. In one of these cases, I was able to obtain about six ounces of blood, in 1849, and my friend Dr. Meyer examined it. He informed me that, on treating the serum with alcohol and chloride of zinc, he obtained a precipitate which partly dissolved in boiling water, and left, on evaporation, crystals of a highly nitrogenized substance, which I have since been convinced must have been kreatine or kreatinine. I could detect no similar matter in the urine in that case. It was low in specific gravity, varying only from 1010 to 1015, but otherwise normal. I have also found that, in some gouty patients, who suffer habitually from hepatic derangement and hemorrhoids, that carbonized substances abound in the blood, and may be detected in the urine in the form of hippuric acid, or hippurate of ammonia.”

The observations of the author on gout as modified by rheumatism and by syphilis, are particularly interesting; they do not admit, however, of a very satisfactory analysis.

In the chapter on the Morbid Anatomy of Gout, Mr. W. presents a number of facts and observations which show that in all cases of gout of any duration, there takes place a deposit of saline or earthy matter in or upon the fibrous tissues—either externally in the neighbourhood of joints, or internally in the fibrous envelops of the brain, spinal cord, nerves, and organs of the senses; in the fibrous coats of the arteries, and the fibro-serous membranes within and around the heart; in the fibrous tissues of the lungs and air-passages; the

aponeuroses of the muscles of the head, chest, and abdomen, and the fibrous coat of the testicle; the lesions resulting from these several deposits giving rise to various latent and irregular forms of the disease.

From the facts that have been accumulated, the author believes the general law is established that, as in strong persons certain derangements in the processes of nutrition lead to deposits of the lithates in the fibrous tissues about the joints, so in delicate or enfeebled persons, the same derangements lead to deposits of the phosphates in the fibrous tissues of internal organs.

The chapter concludes with some observations on the forms of latent or irregular gout, depending on the efforts of nature to throw off gouty matter from the blood by the kidneys. In these forms of the disease, in some cases the kidneys, in others the ureters, bladder, or urethra, are the seat of the most prominent symptoms.

Gout, as it occurs in the female, is the subject of the fifth chapter. The various forms in which the disease may be recognized in women are described as gouty dyspepsia, gouty hysteria, derangement of the general health leading to general and local nervous affections, irritable uterus, leucorrhœa, abortion, &c.

The natural treatment of gout, according to Mr. W., consists in such a regulation of diet and regimen as shall be adapted to promote the healthy performance of all the functions concerned in hæmatisis, and in the nutrition of the system. The beneficial influence of friction of the surface, as a means of promoting the healthful functions of the skin, is pointed out. Of all the forms of passive exercise it is, perhaps, the best.

"Sir William Temple said: 'No man need have the gout who can keep a slave to rub him,' and perhaps he did not greatly overrate the value of his favourite remedy. It was much employed by the ancients, whose attention was more directed than ours has been, until very lately, to the development of the muscular powers of the body, not only as a means of attaining bodily strength and agility, but in order to hasten convalescence after disease, to remove stiffness following wounds, injuries, or long confinement, and in the treatment of various constitutional diseases. But it has been too much neglected by the moderns, for I am convinced, by observation, that if well performed, persevered in for a considerable time, and applied systematically over the whole body, it may almost be made to supersede the necessity for exercise. It leads to very free circulation through the vessels of the skin, and an agreeable sensation of warmth which lasts many hours; and, by equalizing the distribution of the blood throughout the body, and leading to free inspiration, tends to prevent local congestion and oxygenates the blood."

Equally beneficial with friction, and, when used in conjunction with it, productive of the most decided advantage in the prevention of gout, is a regular course of bathing. It is somewhat remarkable that Mr. W. has, in some degree, overlooked this in his account of the natural cure of the disease. It is true, he tells us that:—

"The habits of life to be recommended to gouty patients in the intervals between attacks, are to rise early, to take a shower-bath, or be rubbed by a wet sheet, afterwards by a dry one immediately until warm; then to take some gentle exercise in the room by dumb-bells, or any motion of the limbs tending to expand the chest, and to drink freely of pure water."

It is not, however, all constitutions and conditions of health to which the shower-bath or cold sponging is adapted. Many of those who are predisposed to, or who have already suffered from an attack of gout, will derive far more benefit from the daily use of the warm or tepid bath, with friction of the surface, than from the external use of cold water in whatever way applied. For a full and satisfactory exposition of the rules to be observed by the gouty in bathing, we would refer to the very excellent work of Dr. John Bell, of Philadelphia, on baths, and the watery regimen.

Some judicious remarks are made by Mr. Wells on the cold water cure in its adaptation to gouty patients.

In the eighth chapter, we are presented with the author's experience as to the proper medicinal treatment of gout and its complications.

In regard to *bleeding*, he has, he remarks, never seen a gouty patient who bore the loss of blood well, but very many who have been injured very seriously by it. *Purgatives*, he believes, are often necessary, but are to be used with great caution. In many cases it is absolutely necessary to have the bowels cleared from irritating substances, but this must not be done by drastics or saline purgatives. The action of *sudorifics* Mr. W. considers to have a most powerful curative effect in gout, but it is an action which can, he believes, scarcely ever be attained with any certainty by mere medicine. Unless there be something in the state of the circulation, or in the condition of the nervous system, indicating the propriety of giving antimony or opium, alone or in combination, he is inclined always to trust to the use of diluents or of external measures, as the hot air-bath, or the vapour-bath, to induce perspiration. Some very excellent remarks are presented by the author in regard to the use of *diuretics* in gout, as well as on the employment of chemical solvents as palliatives, while we are endeavouring to remove the state of system on which the formation of the abnormal acids in the blood, and the deposits in the tissues caused by its presence, depend.

"Of all chemical solvents," Dr. Wells states, "I am disposed to regard the iodide of potassium as the most useful, it having so great a solvent power on the lithate of soda, which is the most common impurity in the blood of gouty patients. A concentrated solution of the iodide dissolves the lithate of soda very readily out of the body, and to a much greater extent when the lithate is recently prepared and the solution is warm; but it has very little power of dissolving pure lithic acid. I have given it very extensively for the last thirteen years in almost all forms of gout, except during the acute attack, and in almost every case with the most encouraging results. I have tried it in doses of from eight grains three times a day to one grain daily in divided doses. I have had patients who have continued the latter small dose for several months, and after carefully watching the effects of discontinuing its use and returning to it, I have been convinced that the improvement in health which accompanied and followed its use, was really connected with, or dependent on, the use of even so small a quantity."

Tonics, according to Mr. W., become occasionally of great use in the latter stages of chronic gout. He speaks very favourably of the citrate of iron and quinia, in doses of five grains three times a day, or five grains of the citrate in an effervescing draught, with carbonate of ammonia and lemon juice. The former preparation he recommends especially in old people, when the action of the heart is feeble and irregular. *Mineral waters* partake, he remarks, of the characters of diuretics and blood depurants, chemical solvents, and tonics, some of them having also a purgative action. *Colchicum*, Mr. W. believes to have, unquestionably, a specific power over gout; it not only relieves the pain and inflammation which accompany an acute attack in a joint, but also removes the symptoms of the disease in other parts of the body. This relief, he remarks, is certainly not procured, as many have supposed, in consequence of some sensible effect, as vomiting or purging. In the cases in which it is most useful, the pain is relieved and the general irritation removed without any other perceptible effect.

"The fear," says Mr. W., "that the relief colchicum affords is only obtained at the risk of future increase of disease, and of the irregular or internal affections of which the gouty eventually die, has doubtless arisen from the use of large doses, which really have a most injurious effect by lowering all the powers of the system, and is well founded if such doses be given, but is altogether groundless if the remedy be used in doses small enough to afford relief without producing any other effect." By far the best preparation of colchicum, according to Mr. W., is a tincture of the flowers.

The remarks of the author on the application of the general principles laid down by him for the treatment of gout to the circumstances present in the acute and chronic attacks of the disease—to its various complications with rheumatism, syphilis, &c., and to its several internal or irregular forms; as well as his directions for the management of joints stiffened by gouty deposits, are concise but judicious, and may be consulted with profit by all who are lia-

ble to be called to attend upon patients suffering under the disease or from its sequelæ.

To the treatise are appended four essays by its author, which appeared originally in the *Medical Times and Gazette*—namely, On the Treatment of Ulcers by Galvanism; On the Cure of Squinting by Prismatic Spectacles; An Account of a New Ophthalmoscope; and, On the Use of Chloroform Inhalations in the Palliative Treatment of Phthisis.

D. F. C.

ART. XVI.—*Statistical Reports on the Health of the Navy (British) for the Years 1837 to 1843, including Seven Years. Part II., East India Station. Presented to Parliament, June 1, 1853. Folio, 94 pp.*

As might be inferred from the title, this publication presents a great mass of information which will be examined with interest by the medical inquirer, and more particularly by those of the profession attached to the military and naval services. The valuable tables presented show, among other things, the total number of cases of all diseases and injuries, specifying those sent to hospital, invalided and dead, during a series of seven years; the number of cases of principal diseases and maladies depending upon specific causes that occurred in the different ships; the total number of cases of all diseases and injuries for seven years, the number sent to hospital, invalided and dead, with the ratio of each per 1,000 of mean strength; the annual mean strength, with the annual number of cases of all diseases and injuries; the total number of cases of the principal diseases, the number invalided and dead, with the ratio of each per 1,000 attacked, for seven years.

One of the most interesting features of the report is the attempt made to reduce practical results from statistics by exhibiting the effects attributable to different modes of treatment pursued in different ships. The success was but partial, and the attempt was abandoned in consequence of several difficulties, among which was that of not finding two or more vessels, in which the men had for a sufficient length of time been placed under circumstances which were precisely similar with respect to the influences operating on their health, together with all the necessary information as to their exposure on shore or in boats, their habits of living, the condition of the decks, modes of ventilation, &c. It was, however, ascertained, "that the exhibition of any of the more popular remedies on the heroic plan was not attended with the most favourable results; for, though when so given they might appear to succeed in one case, there were others in which they failed. As in the fevers which prevail on the western coast of Africa, where a more rational mode of practice has been introduced, it was found that bloodletting did not lessen the intensity of morbid action, or relieve what has been called congestion; neither did calomel, even when 'pushed' to an extravagant extent, arrest the onward course of the malady, or prevent the recurrence of febrile paroxysms, even though ptialism had been induced."

The following extracts present condensed statements of the nature and treatment of the febrile diseases to which the naval and military forces were subjected during their active service on the Chinese coast in the year 1843. The reporter remarks that—

"Like the preceding year, the details for the present furnish the most ample proof, were it required, of the unhealthy nature of the Chinese coast from the equator up to the thirty-third degree of north latitude; although beyond that, even up to the most northern limits of the Yellow Sea, there is no reason to suppose that it is more healthy. Everywhere a humid atmosphere overhangs a marshy soil, teeming with a profuse and vigorous vegetation, stimulated to exuberant growth by strong and offensive manures—conditions which, within the tropics, generally, if not universally, prove destructive to the health of

men coming from regions north of the fiftieth parallel of latitude. The shores and alluvial banks of tidal rivers, on the coast of China, differ in some respects from similar localities on the coast of Africa, inasmuch as the former are extensively cultivated, and yield their annual produce in obedience to the will of man; the latter are still in their natural state, bearing the same wild, indigenous vegetation that has covered them for ages, or, it may be, from times coeval with the creation of the human race. In both, remitting and intermitting fevers prevail; but as regards those which occur in China, there is this difference to be observed, that they are almost invariably accompanied by diarrhœal and dysenteric attacks; while in Africa, although diarrhœal affections sometimes precede an eruption of fever, they seldom or never assume a dysenteric character, and they generally cease when the fever becomes general, or acquires epidemic force. How far cultivation, and the difference in the vegetable produce covering the soil, may be influential in the causation of disease in these two regions, is a question which cannot be satisfactorily answered.

“Forty-three vessels were employed for a longer or shorter period on both divisions of the station during 1843, with a mean force for the year of about 6,450 men.

“With respect to febrile diseases, there occurred, of all kinds, 3,008 cases, being in the ratio of 466.3 to the 1,000 of mean force—a considerable reduction as compared with the preceding year, yet high as regards the average of a series of years, previous to the commencement of the war. There were forty deaths, which gives a ratio of 6.1 to the same amount of force, showing that these maladies were not only of less frequent occurrence, but that they were less fatal. In the medical returns, these 3,008 cases have been placed under the heads of intermittent, remittent, continued, typhus, and ephemeral fever, distinctions which might lead to the inference that different kinds of fever had prevailed throughout the force—but such does not appear to have been the case. Originating from causes presumed to be identical on all parts of the station, unless the first or second paroxysm proved fatal, they naturally presented symptoms which, in the main, were common to them all: as what has been called the continued, generally ran into the remittent, and the remittent into the intermittent, although there were rare instances in which this order was reversed, the two latter apparently, from an increase of morbid action, having assumed or passed into the continued type. A large proportion of, or rather nearly all, the cases of intermittent were the sequelæ of fevers which had first appeared in the continued or remitting forms; still there were a few cases which seem to have been intermittent from the commencement, although it is probable they were connected with preceding attacks, which were not observed, or had been forgotten. The term typhus has been applied to twenty cases; it does not appear, however, that they presented the symptoms peculiarly characteristic of this form of fever, though symptoms usually denominated typhoid were noticed: twenty-seven cases of continued and remittent, that is, of primary attacks, and twelve of intermittent, terminated in death; the latter were all cases of confirmed ague, which had long resisted every kind of medical treatment.

“Making every allowance for different modes of treatment, the disease appears to have prevailed with more severity in some vessels than in others, those which were most exposed in unhealthy localities, or in which there existed defective hygienic arrangements, presenting the most intractable cases. In several vessels, in consequence of secondary attacks, the number of cases, placed on record far exceeds the number of men borne on the books; the *Childers* and *Dido* are instances of this kind; but, in point of fatality, the crew of the *Cornwallis* were the greatest sufferers. In this vessel there were 313 cases of intermittent, and 132 of remittent, placed on the sick report during the year: by far the greater number of the first-named occurred amongst men who had suffered previously, more especially while serving in the *Yang-tse-Kiang*.”

“With respect to the treatment of these maladies, the surgeon remarks, ‘that bleeding in the cold stage of intermittent was had recourse to in many instances with marked and decided benefit, particularly if this initiatory step were fol-

lowed by calomel purges and moderate doses of quinine.' Bleeding was also had recourse to in remitting fever, together with nearly all the usual auxiliaries, such as cold applications to the head, blisters, calomel purges, the preparations of antimony, and, when a remission took place, quinine. Under the exhibition of these remedies, the results were not very satisfactory, as eight cases of the former and ten of the latter terminated in death. In some instances, it was supposed the treatment changed the remitting fever into ague. It is further observed, that, 'as compared with the remittent fevers that occurred in the Yang-tse-Kiang during the preceding year, these were much more amenable to treatment; the remissions being more distinctly marked, quinine could be administered with greater advantage: of the former, there occurred 37 cases and 4 deaths; of the latter, 132 and 10 deaths; the mortality for the preceding year being nearly a third greater than for the present. But, on the other hand, the mortality from all kinds of fever for the present year exceeds that of the former in about the proportion of 4 to 2½ per cent. It may be fairly doubted whether any benefit was derived from the bloodletting; at all events, it could only have been of a temporary nature, as the mortality from fever and ague in this vessel far exceeded that in other vessels where the treatment was less 'active,' although the fevers were the same, and contracted in the same as well as in similar localities. In estimating the value of any remedy, such, for example, as venesection in the above instance, it seems difficult to conceive how its effects could be ascertained separately from those of several other remedies which were used at the same time.

"In the Agincourt, there occurred 14 cases of intermittent, 47 of remittent, and 52 of continued or synochal fever: one of each of the former was invalidated, and two of the latter died. The most severe cases of remittent were contracted at Hong Kong by a party of marines who mounted guard over the same stores with the men from the Cornwallis; the earliest appeared on the 9th, and the last on the 30th of July. Every man of the guard who slept on shore was attacked; whereas there did not occur, at that period, a single case of a similar nature amongst the rest of the crew. Out of 75 men ordered on this special duty on the 25th of June, 47 were attacked with fever, and the remaining 28, who escaped, suffered severely either from diarrhœa or dysentery. The disease was not developed in any instance until after they had been fifteen days landed. The remedial measures employed in these cases were emetics at the outset, then a mercurial purge, and, subsequently, James's powder, saline medicines to keep the bowels open, and a dilute solution of acetate of ammonia; with nitrous ether as common drink. Bleeding was not employed in any instance; but it was thought necessary in many cases to 'push the mercury to ptyalism'—a mode of expression which means that it was not given with a sparing hand.

"It is remarkable that in the Cornwallis, in which—"when high arterial action and headache were present"—it was thought necessary to resort to active measures, such as bleeding, 'to reduce the disease to a manageable form,' the number of deaths from remitting and continued fever, namely, primary attacks, was not far from being four times greater than in the Agincourt, in which blood appears to have been only once abstracted by leeches. In making these comparisons, it is also at the same time to be borne in mind, that the crew of the latter vessel had not completed one year on the station, and had never ascended any of the great rivers; whereas the men of the former had been upwards of three years actively employed on all parts of the Chinese division of the station, and had suffered most severely both in the Canton and in the Yang-tse-Kiang rivers; it may, therefore, be readily imagined that they were not only less able to withstand the debilitating effects of fever, but less likely to be benefited by depletory measures, than were the crew of the newly arrived ship, amongst whom it was not considered to be necessary."

"In July and August, while the solar heat was great, parties of men from the Cambrian were occasionally exposed to its influence while watering the ship; they were also exposed to the cold night-air and to dews, in consequence of the watering-place being at a distance, which required them to be absent all night. While thus employed, they took cinchona bark, in spirits, as a prophyl-

lactic, apparently with good effect; for although they were employed floating the water-casks to and from the boat during the hottest part of the day, with the upper part of their bodies exposed to the full force of a mid-day sun, while the lower was immersed up to the middle in the sea, yet none of them contracted any serious illness. The surgeon, indeed, has remarked; that if it were not for liberty to go on shore, together with the intemperate use of spirituous liquors, and of solid animal food, such as 'ducks, eggs, hams, pork, and poultry,' the crew would have enjoyed an almost perfect immunity from every kind of sickness; and he has further stated, that 'if inebriation could be prevented, there would be little for a medical man to do on a foreign station, and the ships would be more effective than is generally the case.' 'I have never,' he adds, 'seen a more salubrious climate than the climate of this part of the coast of China; and I am perfectly satisfied that if alcohol were not in existence, there would not be many advocates for malaria' (as a cause of disease). There is a certain amount of truth in the preceding remarks as regards the influence of intoxicating liquors; but as to the supposed salubriousness of the coast of China, there are too many proofs to the contrary. And although intemperance was unquestionably, in numerous instances, the primary source of disease, still there is no reason to suppose that it was productive of a tithe of the various maladies which prevailed universally throughout the naval force employed on the Chinese waters. The immoderate use of the several kinds of food mentioned may have had some influence; still there does not appear to have been, in this vessel, any increase of those diseases that generally result from repletion or continued over-eating; and, as the crew of the *Cambrian* suffered but little compared with the terrible destruction of health and life caused by other diseases in vessels in which the men suffered from the want of nutritious animal food, it is to be regretted that these good things were not more equally distributed to the whole force, whether naval or military."

May we ask, where, on the shores of the Western Pacific, is to be found that terrible scourge of seaports on the Atlantic, the yellow fever. Has it to make its way to the Chinese ports, as it has recently done to those of the Southern Atlantic, on the Amazon, and La Plata?

Among the most formidable maladies with which the surgeons had to contend, were bowel complaints, dysentery, diarrhoea, cholera morbus, and, worst of all, Asiatic cholera.

The following passages will afford some interesting details relative to the characteristic and mooted points connected with the treatment of the latter disease:—

"In the *Blonde*, while lying at the same anchorage with the *Belleisle* and *Cornwallis*, near Ching-Kiang-foo, close to the position occupied by the 98th regiment, in which the disease raged with destructive force, only one case occurred. For this exemption the surgeon states it would be difficult to assign a cause; he, however, hazards the opinion, 'that it might depend upon the non-depletory plan adopted in the treatment of fevers, which, being of an asthenic form, bleeding was contra-indicated.' The physical powers of the men were thus, he supposed, better able to withstand the inroads of any disease occurring subsequently, while they were less susceptible to the variations of temperature. This vessel moved from Ching-Kiang-foo up the river to Nanking on the 6th of August, and remained there until the 1st of October; she then began to move down the river on the morning of the 2d, when five other cases occurred, one of which proved fatal. Four of these, being at the time under treatment for intermittent fever, were not entered on the sick list for cholera."

"Fourteen cases of cholera occurred in the *North Star*, while she lay at the entrance of the Woosung River. The first appeared on the 5th, and the last on the 25th of August. Eight of these the surgeon has designated as 'of a truly malignant character.' 'At the time the disease made its appearance, the ship's company had certainly arduous duties to perform, many of them being employed on boat service night and day; yet only two or three of the men attacked with the disease in its worst form had been employed on these services.' Bloodletting, he states, appeared to have more or less an excellent effect in all; by this means, reaction in nearly every instance was produced,

and the external heat restored; the surface, in some cases, even at death, was preternaturally hot. In one of the worst cases, when the pulse began to get contracted and weak, thirty ounces of blood, at the rate of about ten at a time, were abstracted with the most marked effect; generally, however, the reaction was but temporary. Now, judging from the results, the advantages gained by the above mode of practice seem to have been of a very questionable nature. Of the effects of any remedial measure, we can only speak with certainty after long experience, by repeatedly contrasting its effects with those of other remedies; and likewise by contrasting the results obtained by the exhibition of one specific remedy, with the results observed in cases treated without the employment of any remedial means whatever. In the few cases of cholera that occurred in this vessel, nearly the same mode of treatment was adopted, the aim being in all to lessen what has been termed congestion; consequently there were no means of judging how far the measures were beneficial or detrimental. Out of fourteen cases, or rather, according to the surgeon's report, out of eight of a truly spasmodic character, six proved fatal—a rate of mortality, it is to be presumed, which effectually precludes the possibility of attributing any beneficial influence to the depletory plan of treatment. In five of the six fatal cases recorded, blood was taken from the arm to the extent of from ten to twenty-six ounces; but in no instance, judging from the symptoms detailed, does it appear to have afforded the slightest permanent relief; the symptoms did not alter, and if they had altered, it would have been difficult to ascertain to what the alteration was due, as hot brandy and water spiced was given both before and after the bleeding. In one of the cases, after twelve ounces of blood had been taken from the arm, an attempt was made eight hours afterwards to take away more; but 'no blood could be obtained, either from the veins of the arms or from the temporal artery;' the pulse was then imperceptible at the wrist, 'the respiration full and loud,' the features much contracted, 'the face and neck of a leaden hue; the body and lower extremities warm, but the neck and arms cold and clammy.' Two hours afterwards the patient expired.

"It is not necessary further to follow up the detail of these cases; sufficient has been stated to show that the bloodletting had none of the excellent effects attributed to it."

"On careful perusal of all the medical reports from the squadron, it appears that in every vessel employed in the Yang-tse-Kiang, from Woosung to Nanking, between the middle of July and the middle of October, cholera or choleraic diarrhoea broke out, while not a case of either form of the disease appeared in vessels employed on other parts of this division of the station, with the exception of those which arrived at Chusan from the Yang-tse-Kiang. On the Indian division of the station it appeared in vessels touching at Bombay, Calcutta, and Singapore, the disease being prevalent in each of those places at the time of their visit.

"How far medical treatment lessened the mortality from this disease, or whether it influenced it at all, is a question, if we are to judge by the medical reports, that will not be easily answered! for there were not two medical officers in the squadron that agreed as to the same remedial measures; while, as a general rule, each considered his own, even although every second or third case proved fatal, to have been attended with some good effect. Another reason which tends to invest this question with difficulty, is the various degrees of severity with which the disease appears in different communities in different places, and in similar communities in the same places. In several of the vessels in the Yang-tse-Kiang, in the *Driver*, *Hazard*, *Harlequin* and *Jupiter*, for instance, the disease could hardly be recognized separately, a few of its leading symptoms, such as spasm, and profuse watery evacuations, having been merely temporarily, so to speak, engrafted on the already existing cases of diarrhoea; while in other vessels, as in the *Belleisle*, *Cornwallis*, and *North Star*, it displayed all its worst characters.

"At the period now under consideration, bloodletting appears to have been much in vogue all over India; and however contrary to common sense it may now appear, still it would be difficult to prove that it was not in some instances attended with benefit, while there are others in which it evidently only hast-

ened the fatal result, inevitable, perhaps, without it. The state which seems to have been most dreaded, and against which this most formidable remedy was employed, was congestion—the accumulation of dark blood in some part of the body, probably in the large veins of the thoracic and abdominal cavities, or in the vessels of the viscera themselves. How it was ascertained that the organs contained in these cavities were really in a congested state has not been mentioned; neither has the nature or the action of the primary cause effecting these accumulations or stagnations of the vital fluid been explained, nor as to how they gave rise to spasm, the flow of immense quantities of fluid from the intestinal canal, and, in the end, to death. The mere mechanical obstruction offered by these congestions did not interrupt respiration or the action of the heart; neither can we suppose they interfered with the secretion of bile or urine; and as none of the tissues of any of the organs, however much they may have been loaded with blood, were found after death to be disorganized, it is difficult to imagine why simple congestion should have been feared, and equally difficult to imagine how the abstraction of blood could be supposed to remove the primary cause effecting the obstruction of the circulation, or (unless by diminishing the quantity) relieve the overburdened organs of their surplus blood. The purpose for which blood was abstracted, appears to have been to excite reaction; but, if it really ever had this effect, it seems as if we might suppose that the increased force of the arterial current would likewise increase for a time the venous congestion, and thus the purposed remedy would add to the evil it was intended to remove.”

The reliance to be placed in the observations made, and results exhibited in the report, may be estimated by the fact that the English squadron on the East India station, for at least ten years previous to 1840, seldom consisted of less than ten, or more than eighteen vessels, bearing a yearly average of from 1,500 to 2,000 men. During the war with China, the forces were increased to 7,000 and upwards.

G. E.

ART. XVII.—*History of the Yellow Fever at New Orleans, La., in 1853.* By E. D. FENNER, M. D., one of the Physicians to the New Orleans Charity Hospital, &c. &c. New York: 1854. 8vo. 84 pp.

WHEN a disease breaks out in any locality, it would appear, at first sight, to be a very easy matter to settle the question as to its origin—whether from some cause existing in the locality itself, or a *materies morbi* introduced from without. And yet we find that, in reference to the occurrence of yellow fever in this, as well as in other countries, the question of its domestic origin or importation from abroad, is still a subject of much and often angry controversy. This, however, we apprehend, arises less from the intricacy and obscurity of the question itself, or from the want of sufficient data for its satisfactory solution, than from the one-sided view that has been taken of the circumstances connected with the several visitations of the disease. It is, unfortunately, too often the case that a few badly interpreted facts are seized upon, while others, of even greater significance, are either entirely overlooked, or, by a forced construction, are made to conform to the conclusions deduced from the incomplete or erroneous data that have been first adopted.

A vessel arrives at one of the wharves of a city from some foreign port—from one, perchance, within the yellow fever zone, and within a few days one or more of her crew are attacked with the disease, while shortly afterwards numerous cases occur in individuals residing in the immediate vicinity of the spot at which the vessel lies, and perhaps in others more or less remote. With many, such facts afford unquestionable evidence of the importation of yellow fever, which conclusion is almost invariably that adopted by the community at large, who never investigate closely or reason accurately, especially on questions relating to the etiology of disease, and whose mind it is always difficult to disabuse of any error it may imbibe.

The arrival of a vessel from a yellow fever locality, and the subsequent occurrence of that disease among her crew, and in those residing in the immediate vicinity of the wharf at which she lies, are not, of themselves, sufficient to prove the importation of the fever. Upon a closer investigation we shall, perhaps, find that the occurrence of disease on board the newly arrived vessel took place subsequent to her entrance into a locality where all the causes that are acknowledged to be capable of producing yellow fever already exist, and where, on other occasions, it has broken out without the presence of any vessel from either a healthy or unhealthy port, or the introduction from without of any source whatever of infection. It is, therefore, as reasonable to suppose that the crew of the vessel contracted the disease after her arrival, from the local causes there existing, as that by her means it was introduced amid the persons on shore simultaneously or subsequently attacked.

It will, upon further inquiry, be found, perhaps, that the greater number of the persons attacked had no communication, either direct or indirect, with the suspected vessel, and were out of reach of any poisonous emanations from her hold; while, at the same time, no fact connected with the occurrence of the disease countenances the supposition of its having been communicated to them, by those primarily attacked.

Carrying the investigation still further, a most important circumstance may, peradventure, appear, namely, that new cases of the fever continue to occur in the same locality many weeks, if not months, after the removal or departure of the vessel to which its origin was attributed; or, perhaps, the still more startling fact will be discovered of the occurrence of sporadic cases previously to the arrival of the suspected ship.

That in all those portions of the United States where the yellow fever has occurred—and the remark may be extended to other countries—there exist local causes, sufficient, under particular circumstances, not yet well understood, to produce the disease independently of any foreign sources of infection, is a fact very generally admitted. In too many instances, the possibility of its domestic origin has been too clearly demonstrated to admit of any doubt; and in no case in which all the circumstances of its outbreak have been carefully investigated, has any valid evidence been discovered to countenance the supposition of the importation of the fever from abroad.

It is possible, we admit, that the arrival of a foul vessel, or one with damaged articles of certain kinds on board, during a season of extreme heat, may give rise to yellow fever. In such a case, however, the disease will be confined entirely to the crew of the vessel and those who have direct intercourse with her, or to such as reside so near to the wharf at which she lies as to be within the influence of the poisonous emanations from her hold, while new cases of the fever will cease to occur on shore so soon as she is removed. We cannot conceive of the possibility of an epidemic of yellow fever being induced in any locality, and there continue to prevail for weeks and months, simply by the arrival of an infected vessel.

That the yellow fever which prevailed during the summer and autumn of 1853, within a very limited district of this city, was produced by local causes alone, is a fact established by the most conclusive evidence; and that the same is true in relation to the more wide-spread epidemic which, at the same time, ravaged New Orleans and its vicinity, Dr. Fenner has, in our estimation, very clearly proved in the work before us. He has, with great industry and candour, and apparently with the utmost care, investigated all the circumstances calculated to throw light upon the origin of the disease in New Orleans, and has, we think, very clearly shown that they are adverse to the supposition of its importation from abroad, while they indicate with great clearness its dependence upon causes existing within the city itself.

It is not our intention to present an outline of the history of the epidemic yellow fever of New Orleans of the last year, as given by Dr. Fenner, nor any abstract of the evidence he adduces bearing upon the question of its domestic origin. We prefer that the reader who feels an interest in the subject should study the facts collected by Dr. Fenner in detail, as the best means of enabling

him to judge of their relevancy, and of the correctness of the conclusions which the author deduces from them.

The character of the facts, and of the deductions the author has made from them, will be seen by the following general summary, which appears on page 72 of the history under consideration.

"1. We have shown that, when the pestilence broke out, the condition of the city in respect to *cleanliness* was so bad as to be an object of *public notoriety*. Indeed, it was in such a state that, if it had given rise to Egyptian plague instead of yellow fever, it should not have surprised any one. The only surprising thing is, that, with so much filth of all kinds as is always to be found in this city, we do not have an epidemic every year. The fact that *we do not*, has even led to the public expression of the strange opinion, that the public filth of the city, instead of *originating* yellow fever, absolutely *protects* us from it in no small degree.

"2. We have shown that yellow fever appeared much earlier this year than usual, and that the *first cases* occurred in persons who had been in the city but a few days, in one instance six days, and in another thirteen. These were unacclimated persons, who had just arrived from Europe, and came over in vessels direct from Liverpool and Bremen, without stopping at any place where yellow fever was prevailing.

"3. We have shown that no vessel that arrived from any *infected* port, whether in South America or the West Indies, brought any cases of yellow fever; had any on board them whilst they were here, or gave rise to any cases previous to those which must have originated in this city.

"4. That within a very few days after the occurrence of the first cases, others were seen in different and remote places, having no sort of connection or intercommunication that we were able to trace after a careful inquiry. That shortly afterwards other cases occurred in still more remote and separate localities, under like circumstances.

"5. We have shown that the disease was the same that prevails here to a greater or less extent every year, differing only in the extraordinary degree of its malignancy.

"6. We have shown that all the varieties of endemic fever which prevail throughout the southern country were seen here in the midst of the epidemic yellow fever, and they were seen to interchange types just as is observed among the fevers of the country."

To this summary Dr. Fenner appends the following comments:—

"From these facts I am led to the inevitable conclusion, that the disease we have been considering originated from local causes existing in this city, aided by an extraordinary constitution or condition of the atmosphere at the time.

"I am not prepared to maintain that the putrefying filth which is to be found to a great extent in this city at all times, is the *sole cause* of yellow fever; for, if it were, we should certainly have an epidemic here every year—but I do believe it is almost a *sine qua non*, and that if it were effectually removed, and the whole city well paved, it would effect more towards the *prevention* of yellow fever here than anything that can be devised. This opinion is surely well supported by the established facts that *yellow fever always breaks out and rages worst in the unpaved and least improved parts of the city*, whilst the *well paved and best built portions generally suffer least from it*.

"It is often stated, and believed by many, that yellow fever always breaks out first among the shipping. *This is not true*, as I have repeatedly demonstrated in my previous writings on the subject. It is firmly believed by many persons that New Orleans is indebted to its naval intercourse with the West Indies, Vera Cruz, or Rio Janeiro, for all the yellow fever that is ever seen here. In opposition to this opinion stands the stubborn fact, that this intercourse has been continued, with but slight interruption, for half a century past, whilst we have only had epidemics of yellow fever on an average of about once in every three years. And furthermore, I have, upon several occasions, carefully noted cases of this disease which were imported from Havana and Vera Cruz, and although they were placed in apartments where they were surrounded by per-

sons who had never had yellow fever, I never knew the disease to be communicated in a single instance.

"The great epidemic of this year certainly broke out first in New Orleans, but the peculiar constitution of the atmosphere, upon which I must think it in a great measure depended, extended to a greater circumference than was ever known before in this region. Still, it had its bounds, and nearly all places within those bounds which had local causes of disease capable of generating yellow fever, with the aid of this extraordinary constitution of the atmosphere, did have it. The range of this yellow fever atmosphere, although so wide, was as well defined as the narrow locality in the city of Philadelphia, in which the disease originated and was confined last summer."

We shall notice in this connection the general statement of Dr. Fenner in relation to the meteorological condition of New Orleans preceding and during the prevalence of yellow fever, and his remarks on the close of the epidemic, both of which occur in a previous portion of the history.

After giving an abstract of the state of the weather from the register kept by Dr. E. H. Barton, of New Orleans, Dr. Fenner remarks:—

"From this abstract we learn that the winter (of 1852–53) was quite cool and dry, but without any very cold weather. I did not see any ice in the open gutters of the city.

"The spring was quite variable; alternately hot and cold, and rather dry. Vegetation remarkably backward.

"From the 12th to the 20th of May the days were *very hot*, whilst the nights were so cold as to render one or two blankets necessary to comfort. The thermometer rose to 88°, which is within one degree of the highest point in July. On the 21st there was a sudden change, the weather becoming quite cold. This continued for two or three days; and *it was about this time*, as well as I have been able to ascertain, *that the few cases of yellow fever appeared in the city.*

"The summer was hot, though *not unusually so*. It will be seen that the *average* of the thermometer was above 79°, which some contend is necessary for the prevalence of the yellow fever as an epidemic. The summer was likewise *quite wet*, though not as much so as others in which there was not so much yellow fever."

From this statement it would seem that the combined influence of heat, moisture, and filth, are not alone sufficient to produce an epidemic of yellow fever.

Treating of the decline of the epidemic of 1853, Dr. Fenner remarks:—

"October 13. The Board of Health announce publicly, that *there is now no epidemic in the city, and that absentees and strangers may come in with safety*. This announcement was welcomed with joy, and from this time people rushed in from all quarters. A few were attacked after their arrival here, but not severely, the malignancy of the fever having been greatly mitigated.

"I stated, in my memorandum of the 17th September, that I considered the epidemic was really over then, although there was no public announcement to that effect. My opinion was based, not *alone* upon the great diminution of cases, but also upon *the change of type* that was visible in the new cases then occurring. Fevers of the *pernicious, intermittent, and remittent* types became more common than yellow fever. In fact, the yellow fever type was gradually giving place to the remittent and intermittent. This is what always occurs, as I have completely demonstrated in my previous writings on the subject.

"The number of deaths from yellow fever, for the week ending September 17, was 221. Now, if we refer to the period when the deaths first got up to a similar amount, we shall find that it was the week terminating the 16th of July, when the deaths were 204. Thus we have *two months* as the period during which it may be said *yellow fever prevailed as an epidemic*. This again corresponds with the observations of the past. We have more or less yellow fever in this city every year, but, after a constant residence here of twelve years, I must say that I have never known it prevail *to an epidemic extent* for a longer period than *sixty or seventy days*."

"A popular error, of extensive prevalence and long standing, respecting yel-

low fever, is, *that an epidemic can alone be extinguished by frost.* I demonstrated this error in my histories of the epidemics of 1847 and 1848, and the events of the present year fully confirm the correctness of my observations. In 1847, the epidemic ceased long before the appearance of frost, yet sporadic cases were seen to a considerable extent to the end of the year. On the present occasion, the disappearance of the epidemic was published by the Board of Health on the 13th of October, whilst the *first frost* was announced on the 25th of October, and that was only observed on the outskirts of the city. On the 31st there was a much heavier frost, but by this time a large number of persons had returned to the city. It is true that some of them were attacked with the fever, as before stated, but the cases were generally mild, and easily managed. We shall doubtless continue to see scattering cases till the end of the year. The general remote cause of all the endemic fevers seen here, which, in its most virulent or powerful state, produces an epidemic of yellow fever, gradually undergoes modification in the progress of time; it becomes less virulent or powerful, and in proportion to the degree of this change will it produce the milder types of fever." "The prevailing *fever cause*, which in August produced *yellow fever* in nine-tenths of the cases which occurred, had become so much weakened in October as to produce mostly the *remittent and intermittent types*; yet the relationship between them was still so close, that if the latter were not promptly cured, they ultimately presented the worst features of the former."

It is now very generally admitted, particularly by the leading American physicians who have studied the yellow fever under circumstances the best adapted to lead to accurate conclusions on the subject, that the disease is non-contagious—that when a patient affected with it is removed beyond the locality where it prevails endemically to a healthy situation, he will not communicate the fever to those who surround him. That this is generally almost universally true in reference to yellow fever is strongly insisted upon by Dr. Fenner—and yet we find him making the following unaccountable admission:—

"I am not prepared to maintain that yellow fever is *never* communicated, *per se*, from person to person. On the contrary, I can readily conceive that, like cholera, typhus, and dysentery, it may sometimes display infectious or contagious properties to a limited extent. I have *never seen* an instance of the kind; but cases have been reported to me, upon such *reliable testimony*, that I could see no other way of accounting for them than direct communication."

"Again," says Dr. F., "yellow fever may be contracted in an infected district, and conveyed in the person of the sufferer to a place quite remote."

Now we find, in the history which Dr. F. has given us of the epidemic which prevailed in New Orleans and its neighbourhood, during the summer and autumn of 1853, no facts to warrant the foregoing admissions, which are, also, in direct opposition to the experience of those who have carefully observed the disease in other epidemics occurring in other localities. It is true, Dr. F. endeavours to weaken the force of his admissions by showing that the contagiousness of yellow fever is a rare and accidental concomitant of the disease. "Admitting it as true," he remarks, "that yellow fever is capable of regenerating itself in the bodies of the sick, and of thus extending from person to person in close proximity, let us pause a moment and examine *the value* of this fact. At all the places where yellow fever prevails in the United States, it appears at *distinct intervals* of different lengths. It breaks out and rages for a season, disappears entirely, and, after a lapse of time, returns again. When it reappears, how often can it be traced, with *reasonable probability*, to infected vessels, goods, or persons? If this can be done in a *considerable majority* of instances, it will be sufficient to make a *rule*, and the instances in which such a connection cannot be fairly traced, must be set down as *exceptions* to the rule. Now I maintain, after careful observation of the outbreaks of yellow fever at New Orleans for the last twelve years, that no such connection could be reasonably traced in any instance. Nor has it been traced in a satisfactory manner in Mobile, Natchez, Vicksburg, or any other place in the United States where this disease has often prevailed. Hence, the *great majority* of the physicians, and others who have had the best opportunity of observing, have come

to the conclusion that epidemics of yellow fever *do not usually, if ever*, originate from this source. This is also the uniform conclusion, with but *few exceptions*, of all those who have seen most of the disease in all parts of the world where it prevails, as any one can see who will examine the volumes of testimony that have been published on the subject."

Again, in speaking of the conveyance of the fever by those affected with it to places remote from that in which it was contracted, Dr. Fenner says:—

"In such instances"—that is, the removal of the sick to unaffected districts—"which are very frequent, and doubtless familiar to thousands of persons in this region, *how often* is the disease communicated to those who come in contact with it? I will venture to say, *not as often as once in fifty cases*. Many would say, not once in a hundred. Two highly intelligent gentlemen of Memphis, have recently told me that probably not less than seventy-five cases of the malignant yellow fever of this year were landed from the steamboats at that place, and there died, or recovered, without communicating the disease in a single instance. Dr. Jewell, of Philadelphia, informs us that eleven cases of yellow fever were admitted from the infected district into the Pennsylvania Hospital, and three into the Blockley Almshouse; they were mixed promiscuously with other patients, yet the disease was not communicated in a single instance. Dr. Ball, of this city, who on behalf of the Howard Association, visited the village of Providence when the epidemic was at its height, tells me of several instances in which the disease was taken back to the plantations without being communicated. I will not weary the reader with instances of the kind.

"If, then, it be true that yellow fever is not communicated from one person to another by immediate presence or contact, independent of other influences, oftener than *once in fifty or a hundred exposures*, it would certainly make but a *rare exception* to a general rule; and this I believe to be about the true and real value of the now popular and warmly advocated doctrine of the *contagiousness of yellow fever*. It is equally applicable to *fomites* of all kinds."

All that we know of unquestionably contagious diseases, proves, that their capacity to propagate themselves by a specific poison, generated in the systems of those affected with them, is their invariable characteristic. We know of no facts that countenance, in the slightest degree, the idea that a disease, admitted to be, ordinarily, of malarious origin, and which, we are assured, is ever known to spread in a healthy district into which those labouring under it are introduced, may occasionally assume a contagious character. The fact, which cannot well be contested in the face of the mass of incontrovertible testimony which has been accumulated in its support, that the yellow fever originates in local morbid causes, altogether independent of any contagious principle derived from those already affected with the disease, and strictly limited to districts more or less circumscribed, is, to us, conclusive evidence that under no circumstances does it assume a contagious character. The few instances that have been reported in which the fever has appeared to be communicated by the sick to the well, or conveyed by the infected to distant and healthy localities, are all capable of an easy explanation, without the necessity of resorting to the absurd notion of a contingent and occasional contagiousness.

In regard to the characteristic features of yellow fever, so strongly insisted upon by many writers on the disease, Dr. Fenner remarks:—

"The general impression derived from reading descriptions of yellow fever is, that it is a *violent fever of a single paroxysm, lasting about seventy-two hours, and presenting strongly marked characteristic symptoms, by which it may readily be distinguished from all other types of fever*. I have not found it so; nor have I yet found the man who could always say correctly whether a case, examined *per se*, was yellow fever or not. I have already shown that differences of opinion were expressed about the first cases that appeared this year, and have only to add that the same thing occurs here every year. The truth is, yellow fever is so closely allied to various remittent and intermittent fevers, that no uniform and reliable distinction can be drawn between them in the early stages; and the only way we get at the fact that yellow fever is prevailing, is by observing the *final results*, whether in *death* or *convalescence*; and the former

is by far the most conclusive of the two. Even in such an epidemic as this, thousands of cases occurred which no one would have thought of calling yellow fever, if it had not been generally known that many of the same character and appearance had terminated in fatal black vomit."

Dr. F. notices as a feature of the epidemic of 1853, a *tendency* to relapse, which presented itself in many instances:—

"After the customary subsidence of the fever at the end of the third day, there would be a period of calmness, lasting from twenty-four to forty-eight hours, and then the fever would kindle up again, and last for one day or more. This secondary fever was often very dangerous, though it frequently terminated happily. The interval of convalescence after the first attack was sometimes so long as to appear more like a *second attack* than a *relapse*. From two to four weeks have been known to elapse between two distinct attacks of the fever in the same person. Heretofore, those relapses, or second attacks, were not at all common."

In regard to *second attacks* of yellow fever—that is, in persons who have had the disease in former years, we are told by Dr. Fenner, that—

"There can be no doubt that if a person have a plain attack of yellow fever during the prevalence of a *severe* epidemic, there will be but small probability of his ever having it again, provided he remains in the same place; yet the rule is by no means *invariable*. If one has it, however, during a *mild* epidemic, or when there are only a few sporadic cases, his subsequent exemption will not be near so great. Thus, there were numerous attacks this year among persons who had other attacks since 1847, but not many among those who had it that year, or any previous strong epidemic."

Dr. F. notices a number of second attacks that occurred during the epidemic of 1853, either under his own observation or that of his medical friends; in many of these cases, however, the fact of the first attack is based solely upon the patients' own statements, upon which, in all instances, the fullest reliance cannot be placed.

In reference to the supposed immunity enjoyed by the *creoles* or *natives* of *New Orleans* from an attack of the fever, Dr. Fenner states, that—

"One of the most extraordinary features of the epidemic of 1853, is presented in the fact that the natives of the city, *both white and coloured*, have suffered severely, and many of them died of it. This is generally admitted and beyond dispute. Children who were born since 1847 have suffered most; but many born previous to that time likewise suffered, and some of them died of black vomit. I saw two quadroon girls, sixteen and seventeen years of age, who were natives, and had never lived anywhere else—they had the fever severely, but recovered. A child, aged five years, in the same family, also had it. I attended a quadroon boy, about eight years of age, who had a severe attack, attended with hemorrhage from the gums. I have already mentioned the case of a white infant, only *five weeks old*, that died of black vomit. It may be stated generally that all children and young persons in the city, who had never had yellow fever previously, were attacked this year. Of course, there were some exceptions to this, as there are to every general rule.

"I am informed that the *creoles* on the coast, above and below the city, suffered much worse from yellow fever than those in the city, many adults having died of the disease."

It appears that during the epidemic of 1853, *unacclimated* negroes, or those who had never had yellow fever before, were equally as liable to attacks of the disease as the white population, but the disease was not so severe and fatal in them as in the whites generally.

"This is equally true," Dr. F. remarks, "of the mixed races generally. It is a well-established fact that there is something in the negro constitution which affords him protection against the worst effects of yellow fever; but what it is, I am unable to say. During an epidemic, he will take the fever almost, if not fully as readily as the white, but it will be altogether milder and less dangerous in its tendency. In short, it will correspond more exactly with the bilious remittent fever that prevails in the country, and requires precisely the same treatment. And yet this type of fever in the city negro must be pro-

duced by the very same cause that gives rise to malignant yellow fever in the white race. Occasionally, we see the hemorrhagic diathesis of yellow fever displayed in the negro—but it is by no means common. The least mixture of the *white race* with the *black* seems to increase the liability of the latter to the dangers of yellow fever; and the danger is in proportion to the amount of white blood in the mixture. Very few negroes ever die of yellow fever in this city; but I learn that a considerable number have been lost on the plantations this year. The cause of this may readily be imagined, when it is recollected that yellow fever never prevailed on the plantations before, and of course most of the physicians were not familiar with its treatment.

“I consider the danger to negroes from yellow fever to be no greater than that from *bilious fever* in the country, and *not half so great as from congestive intermittent.*”

Dr. Fenner estimates the chance of *recovery, when well-marked black vomit* has occurred in a case of yellow fever, to be but little better than one in a hundred. He nevertheless presents quite a respectable list of such recoveries, which during the epidemic of 1853, occurred under his own observation and that of other physicians of New Orleans.

We close our notice of Dr. Fenner's history by the following extract, exhibiting his experience, during the late epidemic, of the “abortive treatment” of yellow fever by large doses of quinia, so highly extolled in some of his previous writings:—

“In 1847, and ever since, till the present season,” he remarks, “I have been able *promptly to cut short* nearly all the cases of yellow fever I was called to treat in this city; and there was but one year (1851), in which there was not a good deal of it to be treated. Several other highly intelligent physicians of this city have done the same. Our chief remedies for this purpose were large doses of quinia and opium, given at the onset of the fever. In reporting my satisfactory success with this treatment, I admitted that the disease treated, although certainly yellow fever, was of rather a milder type, and that it remained to see whether this abortive treatment would be equally successful in a more malignant epidemic. The opportunity has been presented this year, and I must make a candid statement of the results of my experience.

“When this epidemic broke out, I was one of the visiting physicians to the Charity Hospital, and soon had ample opportunities to test the abortive treatment by large doses of quinia and opium. It did very well in many cases where there was a *fair opportunity to apply it*, but I soon discovered that it did not display that controlling influence over this fever which it had done over all the yellow fever I had met with for six years previously. I then fell upon a more moderate use of the sulphate of quinia, and finally gave preference to the *ferro-cyanuret*, in combination with blue mass, and without opium or morphia. I was pleased with the results, and pursued this course to the end of the epidemic.

“I do not recollect to have treated any case of yellow without giving quinia in some form, and am willing to compare results with any physician in the city. I still believe it to be one of the most, if not *the most valuable* of all our remedies in yellow fever—in short, that it is just as valuable in this type of fever as it is in bilious remittent. Many physicians tell me that they found the sulphate of quinia to fulfil their expectations this year as well as usual, whilst others report quite differently.”

D. F. C.

ART. XIX.—*Woman: her Diseases and Remedies. A Series of Letters to his Class.* By CHARLES D. MEIGS, M. D., Professor of Midwifery, and the Diseases of Women and Children, in the Jefferson Medical College at Philadelphia, &c. &c. Third edition, revised and enlarged. Blanchard & Lea, Philadelphia, 1854. 8vo. pp. 672.

WHATEVER fault may be found with those peculiarities of style in which Dr. Meigs delights to indulge, and however much a few startling generalizations in pathology and therapeutics that are advanced by him may be repudiated as unsound, still, the work before us ranks confessedly among the best modern treatises on the diseases of the female—presenting, in general, correct delineations of their symptoms, sound views in regard to their seats and character, and clear directions for their management founded upon extensive experience and close observation.

In the edition just issued, the work has evidently undergone a very thorough revision, while many important additions have been made to different portions of it, calculated to render it more complete, and thus materially to augment its value as a practical treatise on the diseases of woman. We sincerely trust that the life of the author may be spared to superintend many future editions, each of which, we feel assured, will be more acceptable in their teachings than the preceding, and, we should also hope, less exceptionable in style and language than the one before us.

D. F. C.

ART. XX.—*Handbook of Chemistry, Theoretical, Practical, and Technical.* By F. ABEL, Professor of Chemistry at the Royal Military Academy, Woolwich, &c., and C. L. BLOXAM, formerly First Assistant of the Royal College of Chemistry, with a Preface by Dr. Hoffmann, and numerous illustrations on wood. Philadelphia, Blanchard & Lea, 1854. 8vo. pp. 631.

THE progress of Chemical science, and the increased attention which it has attracted, have rendered necessary the publication of treatises devoted exclusively to peculiar departments. That of Analytic Chemistry has heretofore been treated in works which, to a considerable extent, presuppose an acquaintance of the student with general and theoretical Chemistry, and have hence noticed in detail only the methods by which substances should be examined, and the constituents detected and fully determined. Feeling, from experience, the want of a single work in which the “*result* only of a long chain of reasoning” should be presented to the student as a means of enabling them to acquire, at the least expenditure of time, a sound basis for the education of the professional chemist, they have been induced to bring this handbook before the public. The testimony of Dr. Hoffmann to the abilities of the authors is a sufficient recommendation, and will be found fully borne out by an examination of the work itself.

The first chapters give a short account of specific gravity, equivalents, affinity, nomenclature, notation, and the physical condition of bodies. Then follows a full account of chemical manipulation, in which minute descriptions are given of the kinds of apparatus best adapted to the various processes, the different instruments necessary in the construction and fitting of the various forms, and the precautions necessary in construction and manipulation, comprising all that the student will be called upon himself to prepare or use, together with occasional illustration of important points by instruments of less common character, and containing in fifty-four pages matter, which, in all its details, should fix the full attention and appreciation of the student as an essential preliminary to the acquisition of careful habits, and dexterity in practical operations.

The elements, and many of their compounds, are then noticed so as to con-

vey all the knowledge necessary of general and theoretical chemistry without entering upon unnecessary detail. As it is necessary that the analytic chemist should have some knowledge of the methods of preparing objects which he may be called upon to examine, the processes used in arts and manufactures have been given, but without entering into mechanical details unless having some particular bearing on the results as found in commerce.

The concluding portion of the work is devoted to qualitative and quantitative analysis, in which the practice of the Giessen school is followed—modified, however, as experience in teaching has suggested, and in which is given evidence of judgment and ability in this branch of the science.

The American edition is issued in form and appearance well worthy of imitation in works of a similar class, and the facilities of the students have been enhanced by the introduction of appropriate wood-cuts of apparatus which, in the English, are only described, and would be constructed with difficulty, by one not familiar with the best forms of apparatus, without such aid as is here given.

ART. XXI.—*Elementary Chemistry, Theoretical and Practical*. By GEORGE FOWNES, F. R. S. Edited, with additions, by Robert Bridges, M. D., Professor of Chemistry in the Philadelphia College of Pharmacy. A new American from the last revised London edition. Philadelphia, Blanchard & Lea, 1853. 12mo. pp. 555.

THIS well-known work has, since the author's death, received the careful revision of Messrs. H. Bence Jones and A. W. Hoffmann, whose names are well known as connected with chemical inquiry. The original work has created for itself so firm a reputation, that it will be incumbent on us to notice only the recent additions rendered necessary by the constant progress of the science. These have been made principally in Organic Chemistry, but we may observe that the Inorganic department has not been overlooked—the nomenclature modified, equivalents corrected, processes added, &c., whenever necessary. In Organic Chemistry, the additions are of a very interesting character, the most prominent of which stand the alcoholic series, in which the results of the electrolysis of valerianic acid by Kolbe, and also of acetic acid, with the valuable views deducible therefrom, are clearly related, and the results which the study of the relations in composition of this class have elicited, and which will probably lead to important effects, are carefully developed in pages 393 to 396. New alcohols are added to those previously known, and many new and important compounds of the series, as the cyanates and cyanurates, carbonates, &c. are added. In the benzoyl series, we find benzoyl the radical of benzoic acid, and the valuable observations of Noad, Cahours, and Gerhardt, on the homologous compounds.

The Organic Acids are enriched by the observations of Pasteur on racemic, and Piria on malic acids. Organic bases have been enlarged to a considerable extent, not only by additional accounts of new natural bases and their salts, but also by an extensive notice of those of artificial origin derived from the different members of the alcoholic series, from coal tar, animal oil, aldehyde, and those containing phosphorus and antimony.

To the cyanogen compounds the nitro prussides of Playfair are appended, and the articles on litmus, cochineal, madder, &c. have been rewritten.

The above succinct statements of the more prominent additions will be sufficient to exhibit the attention which has been bestowed by the editors in their revision, and it will be found that the very few paragraphs which have been dropped from the original are such as more accurate knowledge has rendered necessary, and which "the author himself would have desired, if his life had been spared to science."

The American edition has also added to it valuable notes, and in paper and execution will bear comparison with those previously issued.

ART. XXII.—*Remarks on the Origin of the Yellow Fever which prevailed in Philadelphia, in 1853.* R. LA ROCHE, M. D. (Transactions of the College of Physicians of Philadelphia, Vol. II. N. S., No. IV.)

THOUGH the controversy, relative to the origin of the yellow fever epidemics, which have at different times prevailed in our principal Atlantic cities—whether it be of domestic origin or imported from abroad—has lost the bitterness of feeling which formerly characterized it; each opinion has still its advocates. The zeal of the partisan has now, however, given way to the anxious and honest search after the truth, and we cannot doubt, therefore, but that the question will be soon settled.

In our present number will be found an interesting account, by Dr. Porter, of the yellow fever which prevailed at Sullivan's Island, Charleston Harbor, S. C., in 1852, in which the author shows conclusively, we think, that the epidemic arose from local causes. Dr. Fenner has proved (see our present No., p. 184, *et seq.*) that the yellow fever epidemic at New Orleans last year had a similar origin, and Dr. LA ROCHE, in the interesting paper we are now to notice, maintains that the yellow fever which prevailed in Philadelphia last summer was due to a like cause.

Dr. Jewell, whose account of this last epidemic we gave an abstract of in our previous number (p. 543, *et seq.*), seems to incline to the belief that the disease was brought here by the barque Mandarin.

The grounds upon which the belief in question has been founded are the following:—

"1. A vessel, the barque Mandarin, left Cienfuegos, Cuba, for this port, on the 25th of June, all in good health, with a cargo of sugar, molasses, and cigars.

"2. At the time of her leaving Cienfuegos no epidemic prevailed there; but, according to the report of the captain, 'a few cases of smallpox and fever existed.' The crew lived on board while in port, anchored off the town, were seldom on shore, and, as far as the captain knew, none of them had visited the sick.

"3. The vessel arrived at the Lazaretto, July 12, after a passage of seventeen days; her crew originally consisting of twelve men. Of these, two died of fever during the passage. The first was attacked when eight days out from Cienfuegos, July 3, with fever, and died on the 7th. The second sickened on the 4th of July, and died on the 9th.

"4. At the time of arriving at the Lazaretto, the crew, numbering ten souls, were, on examination, found to be in good health. Nevertheless, it was considered prudent that the barque should be detained until thoroughly ventilated, cleansed, and fumigated. The bedding and clothing of the deceased sailors were destroyed, the vessel was whitewashed and fumigated in every part with chloride of lime, while the bedding of the crew were aired and their clothing washed. She was detained an entire day.

"5. The Mandarin reached the city on the 13th of July, and came to at South Street wharf. On the 16th, she was hauled up to the lower side of the first pier below Lombard Street, where she discharged her cargo. On the 20th, seven days after her arrival, she dropped down to the lower side of the first pier above Almond Street, where she remained until the 26th. From this she was removed, by the Board of Health, to the cove below the Navy Yard, whence, on the 28th, she was remanded to the Lazaretto, in order to undergo a more rigid and thorough purification. The crew having been previously discharged, the captain and mate remained by her, sleeping on board.

"6. There was no disease of a malignant type in the vicinity of where the vessel lay, 'as far as has been ascertained,' either before or during the time of discharging her cargo. No epidemic was prevalent in any part of the city. There appears to have been no cause for alarm until the cargo was out of the vessel, when it was noticed that a very offensive smell proceeded from her hold. Subsequently, but not before the vessel had been removed to Almond Street

wharf, the stench became intolerable, especially whenever the pumps agitated the bilge-water contained under the limber planks or flooring of the hold.

"7. The first case of a suspicious character which occurred in the neighborhood was on the 19th of July, six days after the Mandarin reached South Street wharf, three days after she was removed to the pier below Lombard Street, and the day before she left this last position for Almond Street wharf. The individual attacked drove a furniture car, and stood on the upper side of South Street. On the next day, the 20th, five new cases occurred, all in persons residing near, or frequenting many times a day, South Street wharf, or the vicinity. The next, or seventh case, was the mate of the Mandarin. He was attacked on the 21st. Other cases occurred that day and the following, all of which were traced to South and Almond Street wharves, and other neighboring localities. On the 23d—consequently, while the Mandarin lay at Almond Street—the captain was seized with the disease. He was removed to Clements's Hotel, Delaware Avenue, between South and Lombard Streets, where he recovered.

"8. From this time the fever gradually spread; but all the cases that occurred up to the 27th of July either resided or did business in the vicinity of South Street wharf. 'Nor is it known,' says Dr. Jewell, 'that any case originated below or south of where the vessel had been last moored, nor has any case come within our knowledge north of Lombard Street.' Subsequently, however, the disease extended and assumed the character of an epidemic, occupying, with few exceptional cases, a locality extending from Union Street north, to Queen Street south (about 600 yards); Second Street west, and the Delaware on the east (200 yards). It did not cease before the middle of October."

"It appears, therefore," says Dr. La Roche, "from the foregoing statement, that a vessel arrived from a port of Cuba, where, at the time of her departure, there did not prevail any epidemic of malignant disease, but only a few cases of smallpox and fever; that a few days after her departure, two of her crew died of fever; that the bilge-water was in an impure state, and emitted an offensive smell; that when she reached the wharf, the vicinity of the place, as well as the city generally, enjoyed the usual ratio of health of the season; that a few days after her arrival, the yellow fever broke out in the vicinity of the place where she lay; that the disease, during some days, remained circumscribed within a very short distance of the spot where it first broke out, and that, with a few exceptions, it continued to prevail in the neighbourhood of South Street wharf, occupying an area of two or three squares in a northern and southern direction, and of a third of that distance westwardly."

An attentive examination of all the facts, satisfies Dr. La Roche, that the barque Mandarin had no agency in the production of the fever; "that the latter was due to the operation of local causes situated in the vicinity of the place where it broke out and prevailed; that these causes were similar to those which have given rise to the same disease at other times, here and in other situations, and that the fever would have prevailed just as surely had no such vessel arrived from the West Indies."

"In the first place, there is no proof—nay, there is no reason to believe"—Dr. La Roche remarks, "that the sailors who sickened and died on the passage, admitting that they had the yellow fever, a circumstance which has not been proved, took the disease from causes existing in the vessel. It is just as likely their sickness was due to a febrile poison inhaled by them while at Cienfuegos. We are told that, though no epidemic was prevailing at that place at the time of their departure, sporadic cases had occurred. They generally do so at that season of the year in all the West Indian ports among unacclimatized strangers and especially sailors. The cause of fever, therefore, was in operation in the town; and we know that the crew of the Mandarin were, if not *often*, at least *sometimes*, on shore, where doubtless they indulged in *sprees*, as sailors are very apt to do. What prevents those who suffered at sea from having imbibed the seeds of the fever when they landed? Or, supposing them not to have landed, what prevents the febrile poison from having been wafted to the ship which was anchored off the town, and of course at no great distance from the latter,

and certainly not beyond the reach of malarial effluvia? Such things have often occurred in tropical ports where there are no wharves or quays; where vessels are all anchored off the town, and where, unless the distance is great, or some intervening cause exists, the men are liable to receive the infection although carefully refraining from going ashore. Add to this, that the men, as I have lately learned, slept not in the fore-castle or other parts below, on account of the oppressive heat that there prevailed, but in the longboat; and that those that died were only taken below after they had sickened. Now, of all places on shipboard, the longboat is about the last where the yellow poison could be expected to be formed or to lurk. As well might we suppose it to be formed in the rigging.

"The period at which those men sickened cannot be urged in disproof of the opinion here suggested; for it is not uncommon, especially when the epidemic influence is not very violent, to find the disease breaking out on the 8th and 9th day after exposure, as was the case in the instances in question, and even later. The very circumstance of the two men taking sick in such rapid succession, would lead me to infer they had imbibed the seeds of the disease before leaving Cienfuegos; for it is not probable, had the poison been formed and exhaled in the vessel, that the two cases would have occurred almost simultaneously, and that the disease would then have ceased to appear until some days after her arrival. The occurrence, of course, is possible. It appears to me, however, more probable, had the fever originated from local sources of infection existing on board, that the cases which occurred would have done so at a longer interval from each other, or, what might be still more natural to expect, that the disease would have continued to show itself off and on during the rest of the passage. So far, however, from this occurring, no one else on board appears to have suffered from sickness; for when the vessel arrived at the Lazaretto, and the men, ten in number, were examined by the physician, they were all found to be in good health; and the captain, before being permitted to proceed to the city, declared on oath, that no sickness, except that resulting in the death of the two seamen, had occurred during the voyage.

"This was on the 13th of July. On the 16th, the vessel having been hauled up to the lower side of the first pier below Lombard Street, the crew was discharged, the captain and mate alone remaining. None of the men so discharged were since ascertained to have died or sickened. Now, had the Mandarin really been in an infected state, the result here adverted to could not very well have obtained. On examining the records of vessels in which the yellow fever has truly originated and prevailed, matters are usually found to assume a different course. In all, the proof of this origin is established on something more substantial than the sudden appearance of two cases, preceded and followed by the perfect healthiness of everybody else on board. Nor is this all. Had the Mandarin been the focus of infection—had she contained the seeds of the disease in her hold, we might have expected to find those who were subsequently employed on board to suffer, in greater or less number, from the disease instead of escaping to a man, while others at a distance were attacked. Such certainly has not been the result in instances in which the poison was undeniably formed on shipboard. Now, what has been the case in respect to the Mandarin? We are told that none of the laborers employed in unloading her, and who must have been sufficiently exposed, in all conscience, to the action of the poison had it been elaborated and accumulated in the hold, took the disease. Furthermore, on the 26th, when the cargo had been discharged, when the hold had been opened, when the disease supposed to have been introduced by her, had already broken out, when, as it is stated, the noxious emanations which had been latent in the hold and under the limbers of the vessel had an opportunity to escape from their place of confinement, when, of course, the vessel must have been saturated with them, and when cases of fever are supposed to have originated from merely sitting some hours in her vicinity while she lay at Almond Street wharf;—on the 26th, I say, the Mandarin, by order of the Board of Health, was taken in charge by the Health Officer, who put on board a crew of five men, including the watchman. These men remained in her from that date up to the 29th of July, when she was safely

anchored inside the island of Little Tinicum, opposite the Lazaretto, and placed in charge of the quarantine-master. What was the result? Did any of these men suffer from the deleterious effects of the poisoned atmosphere of this vessel? No, far from it. 'Neither the Health Officer nor any of his men, nor did the watchman who remained on board until after her purification and return to the city, experience an hour's sickness.' At the Lazaretto, the Mandarin remained from the 29th of July to the 2d of August; several of her limber planks were removed. 'She was then scuttled and filled with water, which remained in her twenty-four hours. The holes were then plugged up and the water pumped out. Next, she was thoroughly whitewashed, and, in every part of her, chloride of lime was freely distributed.'

"All these operations—which, let it be said in passing, are not, under conditions of atmosphere such as those that existed at the time, very generally successful in eradicating the disease from a really infected ship—require considerable exposure on the part of the individuals employed in carrying them out; and yet we are told that, during this process of expurgation, 'the Quarantine Master, with several of the bargemen at the station, were on board and at work in the hold, cabin, and other parts of the vessel for several hours during each day—the weather excessively hot—without any inconvenience to their health.' On her return to the city, the 3d of August, she was moored at Noble Street wharf, Northern Liberties, where she remained until the close of the month; then was removed to the screw-dock, Kensington, for repairs, and on the 6th of September was taking in cargo between Arch and Race Streets, preparatory to a voyage to New Orleans. In these different localities, no cases could be traced to exposure to the atmosphere in or near the vessel. Indeed, as we are informed by Dr. Jewell, no sickness whatever followed in her track after she left Almond Street wharf on the 26th of July.

"A result of this kind could scarcely be looked for had the vessel been the focus of infection. It is one which could not be anticipated by those who have examined the history of vessels in which the fever poison originated. In such instances, the danger of infection is far greater to those who are on board, especially if employed below, than to those who remain outside; while in very many cases the danger is limited exclusively to the former; and, it is well known, that the operation of expurgation, when performed in yellow fever regions, and especially during hot weather, is one of great danger to all but Kroomen and negroes, to whom, on the Coast of Africa and in the West Indies, it is exclusively assigned. In the instance of the Mandarin, however, the danger was, as it would seem, the share of those who abstained from going aboard and remained at a greater or less distance, many of whom not only did not approach her, but did not, nor could, in any possibility, see her.

"Let it not be said that both the captain and the mate of the Mandarin, who remained and slept on board after the crew had been discharged, took the fever, of which the latter died; and that from this circumstance we are justified in concluding that the poison was generated in the hold of the vessel. These men were attacked some days *after* the disease had broken out in the vicinity; the mate on the 21st of July, and the captain on the 23d. They were not the first cases, as would probably have happened had the poison issued from the hold. They were, while attending to their duties and sleeping on board, exposed to the exhalations from the same local sources of infection which were exercising their baneful influence over the people of the neighborhood. Indeed, they were more exposed than the generality of others who were attacked during the season, for, like some of the other early cases, they slept on board, and consequently in the focus of the local infection; and it is well known that malarial exhalations are more active and dangerous at night, and are especially liable to affect individuals exposed to them during the state of sleep.

"Nor can we attach much importance to the circumstance that two of the early cases were those of the captain of a vessel lying in Lombard Street dock, next above the pier where the Mandarin discharged her cargo, and of the mate of another vessel close by, and that subsequently other cases occurred among the men employed in the first-mentioned vessel. There is no proof that these took the disease from the poison issuing from the Mandarin, any more than

from a morbid agent originating, as it has on former occasions, along the wharves. While they were falling sick above the place where that barque lay, others were sickening below, and without approaching the latter. In visitations of the yellow fever in this city and elsewhere, the local sources of infection, though at first circumscribed within rather narrow bounds, have seldom been found to remain so beyond a limited period. They soon cover a surface of a few hundred feet, and do not attain their full limits until some time has elapsed. We may very well understand, then, that cases can have occurred almost simultaneously at South and Lombard Street wharves, without being obliged to attribute some or all of them to the Mandarin. Even if we suppose the cause to have been limited to South Street wharf, where, as we have seen, the first cases and several of the succeeding ones presented themselves, we can be at no great loss to account for the cases on board the Mandarin and neighboring vessels; for we are told that the captain of the former, though sleeping on board of his vessel, took his meals at the Champion House, near the northwest corner of South Street and Delaware Avenue; and it is not improbable that he, as well as the other men who sickened close by, visited that same locality, and that they all perambulated the wharves at night. Captains, mates, and sailors of merchant vessels are not proverbial for their sedentary habits, and when their day's work is over are little apt to seclude themselves within the precinct of their vessel.

"To this I may add that little support is afforded to the opinion respecting the agency of the Mandarin in introducing the disease, by the facts that an individual from the country, who had visited the Avenue on Thursday and Friday evenings (21st and 22d of July), and sat for an hour or more on the wharf at Almond Street, where that vessel lay, took the disease on the 23d, and that the captain of the brig Reform, lying on the north side of the pier above Almond Street wharf, where the Mandarin lay last, shared the same fate on the 26th; for, independently of the little probability of these persons being morbidly affected by a poison issuing from a vessel which, from the day she left Almond Street wharf (on the 26th), was fully exonerated from the charge of having done any injury to a single one of the many individuals who worked, ate, and slept on board, we find that other individuals, about the same time, took the disease in places remote from the vessel, and without having been known to approach her. Thus, for example, Honora Stanton, who resided at No. 16 Little Water Street, above South, some four or five hundred feet from Almond Street wharf, was taken sick on Friday the 22d, and died on the 27th. If cases could occur at a distance from the vessel, and if, amid the large number of individuals who approached her during the time she lay at Almond Street wharf, only two took the disease, and if, besides, after she left that wharf no one sickened on board, we run very little risk in expressing a doubt as to her agency in the production of the two cases in question. The same causes that gave rise to those at a distance can well be considered sufficient to have occasioned those that occurred near Almond Street wharf, without referring the effect to a vessel, the infection of which is, to say the least, very problematical.

"In addition, let it be here remarked that, when the limber planks of the Mandarin were removed, no mud or other collection of filth was found concealed there. The hold of the vessel had, it is true, often been, as the captain admitted, in a foul condition. It is true, also, that when the cargo was out of the vessel, it was noticed that a very offensive smell proceeded from the hold; that after she dropped down to Almond Street wharf, the stench became intolerable, especially when the pumps agitated the bilge-water contained under the limber planks. And we are told, moreover, that in consequence of some defect in the pumps and of her being a tight vessel, the bilge-water could not easily be discharged, and as a consequence soon acquired a disagreeable smell. But all this does not necessarily make a sickly vessel, except under peculiar circumstances, and when carried to a greater excess than appears to have been the case in the instance of the Mandarin. All vessels that carry cargoes of sugar and molasses are more or less foul, for the time being at least, and there are few of them whose bilge-water does not emit an offensive smell, sometimes an in-

tolerable stench; and yet comparatively few become laboratories of febrile poison. Indeed, although under particular conditions of atmosphere, and in particular latitudes, foul ships are very apt to be unhealthy, they are far from being necessarily so. Nor is it less true that a condition of bilge-water, such as is stated in reference to the Mandarin, however often connected with the manifestation of disease, is not always so; cases having been adduced in which the foulest and most offensive water has proved innocuous; while disease has raged where the odor was hardly, if at all, perceptible.

"I may further call attention to the circumstance that the period at which the first reported cases occurred, must on reflection prove an obstacle to the idea of tracing the disease to the Mandarin. It must be remembered that this vessel reached South Street wharf on the evening of the 13th July; that she was removed to Lombard Street wharf on the 16th; that she there discharged her cargo and was removed to Almond Street on the 20th. It is not presumable, that before the vessel had reached Lombard Street, and the cargo had been discharged, the poison was lurking about her decks, cabin, forecabin, or longboat. Such things seldom occur even in ships more fully saturated than the Mandarin could possibly have been. Besides, the captain and mate, who, as experience showed, were not proof against infection, as well as the eight seamen who manned the barque, had been living on that deck, and in that cabin, forecabin, &c. with perfect impunity. The first cases, therefore, if they took the disease from the exhalations issuing from the vessel, did not and could not have done so while she was lying at South Street wharf. The effect must have been produced when the hatches were opened, the cargo discharged, and the hold exposed. Dr. Jewell himself acknowledges the fact; for he remarks that 'there appears to have been no cause for alarm until the cargo was out of the vessel, when it was noticed that a very offensive smell proceeded from her hold.' Now, if we bear in mind that the operation of unloading did not commence before the 16th—the day the vessel was hauled up to Lombard Street wharf—and could not have been effected in less than twenty-four or thirty-six hours, we shall perceive that the morbid effluvium, supposed to have been generated under the limber planks of the hold from the putrescent state of the bilge-water, and which heretofore had there remained in a latent state, could not have had 'an opportunity (acted upon by certain exciting causes, as heat and moisture) to disseminate itself,' and poison the atmosphere in the immediate neighbourhood of the place where the barque lay, before the evening of the 17th. If, with this fact before us, we turn to the date of the early cases, we shall find that the first occurred on the 19th, some thirty-six or forty hours only after the poison began to escape. On the next day, 20th, we have five cases. On the 21st, five more occurred. Here, then, we have an incubation of thirty-six or forty hours at most in one case; an incubation of sixty to sixty-five in five cases, and an incubation of from eighty-four to ninety in five more. The occurrence will appear improbable to those who have paid some attention to the subject, especially if they take into consideration the circumstance that the poison, which is supposed to have so quickly stricken down individuals situated at a greater or less distance from the vessel, was powerless as regards those employed in discharging the cargo, and who consequently were most exposed to its morbid effects. Such instances of short incubation are rarely encountered. Cases of course occur shortly after exposure—sometimes a few hours, or a day or two. They are mentioned in many works on the fever, and I have myself, in a recent publication, referred to several of a striking character. They are, however, generally observed only in places where the atmosphere is fully saturated with the poison, and in times of violent and wide-spreading epidemics. But, even under such circumstances, they are comparatively unfrequent and exceptional. More generally the process of incubation is of longer duration, requiring, according to some writers, from three to eight days, and in the opinion of the greater number, from five to ten. It has extended to three or more weeks, or to several months. Now it would be a rather extraordinary circumstance that, in the epidemic of last season, we should have had, at the outset, and in such rapid succession, so large a number of instances of unusually short incubations, and that the disease should have attacked the first suf-

ferer about a day and a half only after the poison could possibly have reached him, and the very next five only at the most twenty-four hours later. More consonant with our former knowledge on the subject, backed by what has been adduced against the supposition of the Mandarin having been the efficient agent in introducing the disease among us, is it to conclude that the individuals thus attacked had imbibed a poison generated elsewhere than in the ship, and which floated in the atmosphere of localities where the disease broke out and prevailed before the cargo was discharged.

"That causes capable of producing the effect, and similar to those which have again and again occasioned the yellow fever without the agency of a really or supposed infected vessel, existed in those localities, will not be denied by any one who inquired into the subject at the time. 'The docks along the Delaware line, between Lombard and Almond Streets,' says Dr. Jewell (writing in September), 'as usual contain large accumulations of offensive mud and other filth.' 'In addition,' continues the same writer, 'to the prevalence of the morbid atmosphere which we have clearly shown developed itself on board the Mandarin, but not until her cargo was discharged, and which so sensibly affected individuals on approaching her when she lay at Almond Street wharf, we must not for a moment conceal the existing causes in the immediate vicinity of South Street wharf, sufficient to justify the supposition of their agency in the development of disease of a malignant type, when subjected to the high thermometrical influence which prevailed throughout the months of June and July. Not the least mischievous of these causes, in the production of an unhealthy atmosphere, was the outlet of the sewer into the dock at South Street Ferry, belching forth continually putrid masses of animal and vegetable filth accumulating around its mouth, and exposed at low water to the rays of the sun, exhaling streams of unwholesome and poisonous gases into the surrounding air. Besides this agent, there was a most foul wharf at the upper side of South Street; a filthy avenue between Lombard and South Streets, without any properly constructed surface drainage; numerous damp confined cellars, subject to an occasional overflow by the ebbing and flowing of the tide-water of the Delaware, and various minor causes that might properly be added to the above category, fruitful in the production of atmospherical changes injurious to health.'

"At a still later period (October 5), Dr. Jewell reverts to the subject, and after taking some pains to show that the disease was not sustained by an overcrowded population, he remarks: 'The whole neighbourhood, however, may be considered as favorable to the production and nourishment of malarious fevers, in view of its proximity to the river docks, the open sewer at South Street wharf, the damp cellars, filthy alleys, and other local causes of disease, under such a long-continued high thermometrical atmosphere, as prevailed during the months of July, August, and September. That of July being 77.14 degrees, about two degrees above the average mean for many years; August, 76.76, or nearly four degrees above the common average for that month, according to Dr. Swift's review of the weather, published in the *Medical News*.'

"Such accumulations of organic refuse, I repeat, have often, when located in places of the kind particularly, and when aided by certain thermometrical and hygrometrical conditions of the atmosphere, proved the source of the evolution of the yellow fever poison. To no other cause could our epidemics of 1805 and 1819 be ascribed; for there was not at those two seasons, the most remote possibility of the disease being imported in any shape or form whatsoever. In the former, no vessel, likely to have rendered us this poor service, and exonerated our city from the dishonour of being 'the parent of such a pestilence,' had approached nearer than the Lazaretto; and it is well known that, when, in 1819, the disease broke out in the ferry tavern, on the upper side of Market Street, and in the contiguous buildings, there was not, nor had been from the commencement of quarantine, any vessel near them from the West Indies or any sickly port. Let it be remarked that, on those occasions, as indeed at every other return of the disease among us, the fact of its non-contagiousness was as satisfactorily made out as it was during the past season. Were it necessary, hundreds of instances, which have occurred in other portions of temperate regions,

might be adduced to show the local origin of the yellow fever from causes of the kind mentioned, and its independence of any exotic or imported poison. And the most strenuous and ultra advocate of importation in this city and everywhere else, must acknowledge that, in the West Indies and on the coast of Africa, the disease is due to exhalations issuing from localities very similar to those to which we have for years past been disposed to attribute its origin here. During this very last summer, eight cases of what I would consider well-marked yellow fever, occurred in Brandywine Village, which were indubitably the result of local causes. Instances in large numbers are on record of the disease breaking out and prevailing more or less extensively on shipboard under circumstances which leave no doubt as to its local origin there. Nor are such cases limited to vessels from the West Indies or the coast of Africa. They have shown themselves in some from cold climates, where they could not possibly have imbibed the seeds of the disease. They have occurred in port or at sea, during a cruise or passage, and far from any contaminating spot. It has not unfrequently happened, that while a vessel is severely visited by the fever, others situated near it in port, or at sea, or on the same cruising ground, have remained uninjured. The very poison said to have existed on board the *Mandarin*, is admitted to have been elaborated in her hold out of materials of the kind mentioned. And surely, if such has been the case in all the instances referred to, we cannot see why they may not be regarded as capable by themselves of having produced the effect in question last year on our wharves, especially when we bear in mind the atmospheric peculiarities of the season, and the great tendency manifested in most places south of this city and in tropical regions generally to the development of the disease. They certainly existed in greater excess there than under the limber planks and in the hold of that vessel; and, if the foulness of the latter can be supposed to have been the source of the febrile poison, it remains to show why the foulness on shore could not have given rise to a like effect."

Though the greater number of the cases, as Dr. La Roche remarks, "which prevailed during the season, from the 19th of July to the middle of October, were in the vicinity of the wharf where it originally broke out, and where it is supposed the *Mandarin* introduced and left the poison which contaminated the atmosphere, the disease showed itself at a considerable distance from the place, and without the possibility of its being traced there directly or indirectly. Dr. Jewell, in that part of his essay which was read before the College on the 7th of September, gives us an account of forty-four cases that had up to that period appeared. Of these, thirty-seven were traced directly to the infected district. In four, the origin was doubtful; and in three, no clew could be had as to where the disease was contracted. In other words, they could not be shown then, and cannot be proved now, to have taken the disease in the infected district, properly so called, *i.e.* within a short distance of where the *Mandarin* had lain; while they sickened too far from that district to permit us to think that the air contaminated by the vessel could have reached them, especially as we find that it did no injury to the intervening population. In his last communication (Oct. 5), Dr. Jewell states that the whole number of cases registered had amounted to 170. Of these, 147 were traced directly to the infected district; 22 were of doubtful or unknown origin, and one contracted the fever elsewhere. Of these 22, three occurred in September, in the upper part of the city, fully a mile and a half north of South Street, but not far from the Delaware front. The history of the first of these three cases could not be ascertained; but there is no reason to suppose the patient had approached the infected district. He resided in north Front, near Callowhill. The subject of the second case, was the wife of the first. 'This woman declared that, so far from visiting the vicinity of South Street, she had not even crossed the threshold of her own door for several weeks, having a family of small children around her. The room she occupied, the third story front of an unfinished warehouse, was very filthy, but large, and by no means confined. The whole upper part of the building was rented out, in rooms, to different families of the low order of Irish, everything around presenting poverty, rags, and filth.' The subject of the third case resided about one square north of the preceding, in Willow Street, two doors west of Front.

The individual, as we are told, 'unhesitatingly declared that he was not acquainted with the lower part of the city, did not know that he had ever been in the vicinity of South Street wharf; was a shoemaker, worked in Front Street above Noble, and was not in the habit of going anywhere else, but from his shop to his residence in Willow Street.'

"Here we have cases of unmistakable yellow fever arising far from the infected district, and in a locality where the atmosphere supposed to have been contaminated by a poison derived from the Mandarin could not have penetrated. Dr. Jewell, it is true, speaks of 'an epidemic influence stealthily creeping along the wharfs from the infected district, and which lent assistance to other causes in exercising a morbid agency on the individuals in question.' But he will have some difficulty in making us understand how an epidemic influence (the nature of which, by the way, he does not explain) could have travelled the distance of a mile and a half, and, in its transit, passing over hundreds of vessels, and thousands of people, without influencing any but three individuals residing at the end of its journey. More natural will it be—more consonant with the result of experience here and elsewhere—to conclude that the disease arose, in the instances above mentioned, from the action of a local febrile poison; and, setting aside the aforesaid mischievous intruder from South Street, join with Dr. Jewell in the opinion that the effect was due to a residence, on the part of the individuals seized, 'in the immediate vicinity of Willow Street wharf, where the culvert along Pegg's Run empties into the Delaware, which outlet, at low tide, is fully exposed, and where at all times there is a large deposit of putrefying vegetable and animal remains, and that, from the decomposition of these remains, there arose "a miasmatic constitution of the atmosphere," "from the inhalation of which those individuals contracted the fever, resulting in the death of two of them."'" Surely, to those who know what the results of the exposure of such remains to the action of a powerful sun commonly are—especially during a summer season such as that through which we passed last year, with an average several degrees higher than usual—the sickness of the individuals mentioned will not appear difficult to explain, without having recourse to an epidemic influence stealthily creeping along the wharfs, and whose existence is founded on purely hypothetical grounds.

"Of course, some will feel inclined to say that, in the supposition of the cases in question having arisen from the action of the local causes mentioned, it is difficult to understand how the disease, instead of spreading, should have been so restricted as to attack only three individuals, and leave the rest of a large population unscathed. To this it may be answered, that in every place where the yellow fever makes its appearance epidemically—whether it be in this or other cities—many more escape than are attacked, except in times of violent and wide-spreading visitations; that, in the instances before us, the cases occurred towards the latter end of September, and but a few days before the occurrence of frost, or cold weather;—therefore, shortly before the close of the epidemic—and when, consequently, the disease had but little chance of spreading; and that, explain them as we may, such instances of the limitation of the disease to a few cases, even in instances when its local origin cannot be doubted, are of rather frequent occurrence. Sporadic cases of yellow fever, in various cities and towns subject to the disease, have been noted from time immemorial. In some places, indeed, they occur almost annually, between periods of epidemics. Nay, I could, if necessary, show that, in localities where the fever has never or seldom assumed the epidemic character, sporadic cases are not unknown. Now sporadic cases are as surely the effect of a poison evolved from local sources of infection, as those that occur in epidemic seasons; and, if we admit this, we can very well admit, also, that the disease which broke out last year in the northern part of this city, towards the close of September—though affecting but three individuals out of a population of many thousands—may have been just as well the result of a poison exhaled from local sources of infection, as if it had spread to a large number, or invaded the entire district.

"It may be remarked, before closing this branch of the subject, that instances of the sudden appearance, in epidemic seasons, of cases of the disease at a more or less considerable distance from the infected district, without the possibility

of tracing them to exposure to the tainted atmosphere of the latter, are not unknown. Those of us who witnessed the epidemic of 1820 must remember, that after the disease had prevailed for some time in the parts originally and principally affected—first about Race Street wharf, and subsequently and mainly about the foot of Walnut Street and the vicinity—and when it was there on the decline, it suddenly broke out, on the 18th of October, in the Northern Liberties, a mile or more from Walnut Street. The cases were twelve in number, of whom eleven died. They could not in any way be referred to the morbid agency of the atmosphere of the localities where the disease had before been prevailing, and were satisfactorily traced to exhalations issuing from Pegg's Run, which at that period was uncultivated, and received the contents of the gutters of numerous streets and alleys, as also of two culverts, and the offals of several glue, starch, dressed skin, and soap manufactories, of slaughter-houses, privies, &c."

To the various reasons already assigned for the disbelief that the Mandarin was instrumental in introducing the disease, Dr. La Roche adds that *he has reason to believe that cases occurred prior to the 19th of July*, "which exhibited many of the characters of the fever about to prevail epidemically. Of one case I am positively certain. It was as well marked as any I heard of or saw during the course of the season, and as such deserves to be recorded in this place. I am indebted for an account of it to the attending physician, Dr. Keating.

'James Kiddell, æt. 47, Englishman, a measurer of mahogany, residing in Fourth and Spruce Streets. I was called to see him on the evening of Monday the 4th of July. He had been complaining since the 1st, and had felt so unwell as not to attend to his regular occupation.

'Symptoms when I first saw him: violent pain over the colon; intense burning in epigastrium; tongue very red and furred, rather contracted; expression of face anxious; extreme cephalalgia, slight nausea, and a constant desire to go to stool, attended with tormina and tenesmus; evacuations, when I saw them, fecal and abundant; heat of skin intense; pulse 120, hard and contracted.

'Tuesday, July 5, at 9 A.M. Passed a very restless night; skin very hot; pulse increased in frequency; burning in epigastrium very much increased; tenderness over the colon almost disappeared. Has had constant recourse to the close stool, having had seven evacuations since I saw him. Passages consist now of a sero-mucus tinged with blood and semifecal matter, but abundant in quantity. Cephalalgia and anxious expression of face, and nausea increased; but, with all this, the patient seems to have no uneasiness as to the result, and does not complain much of his sufferings.

'Evening. Pulse and general condition same as in the morning; more flush in the face; constant vomitings of a glairy mucus, some of it having chocolate-coloured grounds; stools more frequent, quite abundant, and consisting of a fluid of tarry consistence and dark purplish colour, resembling broken-up clots of blood; pain over the intestines, tormina, and tenesmus disappeared. Says he feels better.

'Wednesday, 8½ A.M. Face has the dusky flush of typhus fever; tongue red at the edges, brown and dryish in the centre; pulse very quick, not quite as strong. Has been vomiting a quantity of coffee-ground, chocolate-coloured fluid, and passing large masses from the bowels, resembling broken clots of blood mixed with feces and mucus. Dr. West saw him with me at 12 M. on Wednesday. Continued very much in the same way until Friday morning, when all his bad symptoms apparently gave way. Got up at 10 o'clock, stood before the glass and shaved himself. On that day, Dr. West and myself considered him convalescent. But at 9 o'clock P.M. I was called in haste, when I found Mr. Kiddell vomiting large quantities of claret-coloured fluid; his pulse was rapid and sinking, and he was constantly purging a dark-coloured fluid. Complained of no pain; brought up the fluid from his stomach without any effort. It seemed almost a regurgitation.

'Mr. K. died Saturday morning at 9 A.M., after having had two convulsive spasms. I saw him a half hour previous to his demise; he complained of no pain, and insisted that he was convalescing. His skin was now slightly jaundiced; the conjunctiva much injected; mouth filled with purple-coloured sordes.

A few hours after death the skin assumed a deep bronze colour. A *post mortem* was refused.'

This case, to say the least of it, Dr. La Roche justly remarks, "is highly interesting. For my part, I cannot but consider it as one of yellow fever. At any rate, it does not look like anything else, and certainly presents so many of the characteristic phenomena of that disease, that, had it occurred but a few weeks later, it would not have been viewed in any other light by the most experienced physician. The dysenteric complication is far from being an unusual one, and its occurrence is not sufficient to change the diagnosis. It constituted a prominent feature in the case of Mr. Crowell, which occurred on the 16th of August, and terminated fatally with decided black vomit on the 19th. In the instance before us, we have the peculiar headache and anxious expression of countenance, the frequent and contracted pulse, so common during the stage of reaction; the intense burning of the epigastrium; the red, contracted, and furred tongue; the chocolate, claret-coloured, coffee-ground discharge from the stomach; the tarry, dark-purplish, bloody stools; the jaundice; the bronzed appearance of the skin a few hours after death; and, on the third and fourth day, so sudden and complete a cessation of all the bad symptoms, and so remarkable a retention or return of muscular strength as to induce the physicians to regard the disease as effectually mastered. This assemblage of phenomena, so strongly characteristic of the yellow fever, is not encountered in any other febrile affection; and I can have no doubt that, were the description of the case inserted in the same monograph with those of other cases of undeniable yellow fever, without reference to the time at which it occurred, Mr. Kiddell would not be supposed to have died of any but the genuine disease.

"The conclusion will appear the more natural when we take into consideration the period of the year at which the case occurred; the high range of the thermometer at the time and for many weeks previous; the localities the patient had visited, and the circumstance that, but a few days after, many other cases, as regards the yellow fever nature of which there can be no difference of opinion, showed themselves.

"During the progress of the case, I was made acquainted with the phenomena, and I saw the body an hour or two after death. Dr. Keating recollects full well, that I expressed several times the opinion that the disease was one of a very suspicious character; that I stated that, if there were cases of yellow fever in town, I should feel no hesitation in reporting the one in question as of that kind, and that he had better watch it closely lest it might slip out of his hands before he was aware of danger. He recollects, too, that, when I heard of the sudden and remarkable remission on the third or fourth day, and afterwards saw the body, I was still more disposed to believe he had had to deal with a case of yellow fever, and that I strongly urged the propriety of a *post-mortem* examination, when, I felt convinced, the liver, stomach, duodenum, and small intestines would be found to present the anatomical characters appertaining to that disease.

"Entertaining these views respecting the nature of the foregoing case, I can have no hesitation in regarding it as an *avant-coureur* of what was about to happen—as a foreshadowing of the approaching epidemic. Whether or not many cases of a similar kind, or of a more mitigated character, occurred about the same time, and prior to the 19th of July, is more than I shall undertake to decide positively. But, considering that all the cases of yellow fever that ended fatally during the season, especially at the commencement, were not reported as such to the Board of Health; that the disease, being new to most of the practitioners of the city, may easily have been mistaken for something else; that such may more particularly have been the result in mild cases, and that very few of those that recovered from an attack of the real disease were reported at all, we can have no certainty that the case of Mr. Kiddell forms a solitary exception to the rule, and was the only one that presented itself prior to the arrival of the Mandarin and the outbreak of the disease at South Street wharf.

"Let it be observed, in addition, that doubts may be raised as to the correctness of the dates at which the attacks of some of the early cases recorded are said to have occurred. In regard to one of them, at least, some uncertainty exists; for while he is stated, on information obtained at his boarding-house, to

have been seized on the evening of the 20th, he himself—and we should suppose he knew something about the matter—affirmed at the hospital, where he was removed, that he sickened on the 17th.

“But be this as it may; whether the case of Mr. Kiddell was or was not the only one of the kind that occurred before the arrival of the Mandarin, at South Street wharf, matters little as regards the question before us; provided it be admitted, and I cannot conceive how any doubts can be entertained on the subject, that the disease of which he died was similar to that which subsequently spread epidemically, my object will be attained. In that event, the case in question must have been due to the action of the same poison which produced all the cases that appeared some days afterwards; and its occurrence early in July, shows indubitably that that poison floated, to some extent, at least, in the atmosphere of the wharves prior to the arrival of the Mandarin; and that, consequently, we are to look, not to the bilge-water of that vessel, but to other sources of infection, to account for the existence of the fever among us last summer. Such foreshadowings are not uncommon. They have been noticed in many of our epidemics, and in those of other places, where it was observed that the disease commenced with a single, or a very few cases; and that, after the recovery or death of these, it made a remarkable though partial pause. This pause was noticed as regards the early epidemics of this city, by Dr. Currie and Dr. Rush. It was dwelt upon in an especial manner by the late Dr. Hosack, who stated that it occurs in every visitation of the fever, and fixed its duration at from eight to twelve or fourteen days.

“Another and very strong reason for doubting, if not denying, the agency of the Mandarin in the production of the fever of last year, is derived from the circumstance that we are called upon to believe that an epidemic which lasted—as epidemics of yellow fever usually do here, and in other parts of temperate regions—nearly three months, was thrust-upon us by a vessel which did not remain in port more than a week after the discharge of her cargo; which is yet to be proved to have communicated the disease to a single individual who visited or worked on board of her, and which is admitted by all parties to have been perfectly innocuous after she left Almond Street. In a word, we are given to understand that a fever, which continued to prevail a very long while after a vessel left the wharf where her cargo was discharged, was nevertheless introduced among us by that vessel. That this vessel deposited, during the few hours she remained at the wharf after she was emptied, a poisonous agent of some sort, which continued to thrive and spread its baneful effects after the departure of the instrument of its introduction; and that the latter, after making the fatal deposit, and being removed from the spot it had helped to contaminate, ceased to be itself a focus of infection. The occurrence, in so far as regards the yellow fever, does not appear to be probable, and would, if true, be in direct contradiction to the result of former experience here, and in many places where the disease has often been observed.”

In conclusion, Dr. La Roche observes: “If the facts and arguments that have been presented in the foregoing pages are entitled to the value I attach to them, we may conclude that the origin of the yellow fever which, after an interval of more than thirty years, visited this city last summer, can in no way be ascribed to a poison issuing from the barque Mandarin, and contaminating the surrounding atmosphere; but must be viewed as the product of exhalations from sources of infection existing in the localities where the disease appeared.”

We have thus given as full an abstract of the important paper of Dr. La Roche as our limits will permit. Those who are specially interested in the subject, should refer to the paper itself, in which they will find many additional arguments in support of the opinion he maintains. We have quoted, however, sufficient to prove that Dr. La Roche brings to the consideration of this question a sober judgment, a well-disciplined mind, and a thorough acquaintance with the whole literature of the subject, which entitle his views to a respectful consideration, and that they should not be rejected until equally conclusive and *well-observed facts* can be adduced in opposition.

- ART. XXIII.—*The Mastodon Giganteus of North America.* By JOHN C. WARREN, M. D., late Professor of Anatomy in the University of Cambridge; President of the Boston Society of Natural History; Member of the American Philosophical Society; of the American Academy of Arts and Sciences; of the Philadelphia Academy of Natural Sciences; of the Academy of Naples and Medical Society of Florence; Honorary Member of the Medico-Chirurgical Society of London; Corresponding Member of the Academy of Medicine of Paris, &c. &c. Boston, 1852. 4to. pp. 219. With twenty-seven Plates.
- Address to the Boston Society of Natural History.* By John C. Warren, M. D., President of the Society. Boston, 1853. 8vo. pp. 48.
- Remarks on some Fossil Impressions in the Sandstone Rocks of Connecticut River.* By John C. Warren, M. D., &c. Boston, 1854. 8vo. pp. 54. With a Plate.
- The Preservation of Health. With Remarks on Constipation, Old Age, and Use of Alcohol in the Preparation of Medicines.* By John C. Warren, M. D., &c. &c. Boston, 1854. 8vo. pp. 140.

THE professional annals of our country do not, we believe, furnish a brighter example of long-continued industry and devotion to the improvement of our science than is afforded by the author of the above works. For half a century engaged in extensive practice, both hospital and private, he has, nevertheless, found time to record for the improvement of his brethren and for the benefit of mankind, the results of his large experience; and even now, though he has accomplished enough to satisfy the ambition of most men, and has arrived at an age when repose might be supposed desirable; and further, though suffering, we regret to learn, from impaired health, he still devotes his energies with a zeal which would be worthy of youth to the advancement of the profession he has so long adorned, and even pushes his researches into other fields of science, and contributes by his labour to their cultivation and improvement.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *Structure of the Spleen.*—In the sixth number of the *Quarterly Journal of Microscopic Sciences*, Mr. HUXLEY gives an account of his recent investigation into the structure of the splenic Malpighian bodies. From these it results, that one or more minute arterial twigs enter and frequently subdivide in the substance of the Malpighian body, making their exit on its opposite side, to terminate finally by breaking up into minute branches in the pulp. The artery is not only surrounded by, and in immediate contact with, the indifferent tissue of the pulp, but the latter is really the representative of a part of its tunica adventitia. The vessels within the Malpighian bodies are, however, not arterial ramifications only, as there is, in addition, a tolerably rich network of capillaries connecting the arterial ranuncules.

Mr. Huxley has not been able to convince himself of the existence of any fluid matter at all in the interior of perfectly fresh Malpighian bodies of any animal he has examined. These bodies appear to him solid, though soft; and to have an essential structure the same as that of every "indifferent tissue" he has examined, and not composed of cells, although here and there true cells are to be discerned.

In the human spleen, the Malpighian bodies, cannot be said to possess walls; the tissue at the line of junction with the pulp not forming a distinct membrane; and Mr. Huxley cannot see the membrane and other limitary structures which Mr. Sanders has so circumstantially described. On this discrepancy we may remark, in the words of a learned judge, that "those who see it, cannot see it unless it be there; and those who do not see it, cannot see it all."

It may be said, then, that the Malpighian bodies of the mammalian spleen are not closed follicles, and have no analogy whatever to the acini of ordinary glands, but that they are portions of the spleen, everywhere continuous with the rest, but distinguished from it by immediately surrounding the arteries, and replacing their tunica adventitia, by containing no wide venous sinuses, and by being composed of absolutely indifferent tissue.

Mr. Huxley considers also, with Leydig, that no line of demarcation can be drawn between the spleen, the lymphatic glands, Peyer's patches, the glandulæ solitariae of the intestine, the suprarenal capsules, and the thymus gland, of which "vascular glands" the primary form is represented by the intestinal solitary gland, which is nothing but a local hypertrophy of the indifferent element of the connective tissue of the part, and possesses no other capsule than that which necessarily results from its being surrounded by the latter. To these the follicles at the base of the tongue and in the tonsils must be added; and Mr. Huxley thinks that from these there is but one step to the liver.

2. *Vascular Tissue of the Schneiderian Mucous Membrane.*—Dr. KOHLRAUSCH published in MÜLLER'S *Archiv*. (1853, II. 2) a description, with figure, of what he designated as an erectile tissue, lying between the mucous membrane and the periosteum of the turbinated bones, especially at their posterior aspect. It is a venous plexus, which can be exhibited by insufflation or by injection, consisting of loops of venous twigs, running vertically to the bone, and closely connected by firm areolar tissue. The mucous follicles, which, in other parts of the nasal mucous membrane, are quite superficial, and have short funnel-shaped openings, lie here more deeply between the meshes of the venous network, and discharge their secretion through longer ducts.

This peculiar arrangement of the vessels of the Schneiderian membrane accounts for the profuse hemorrhage which so often occurs from the nose, as well as for the sense of fullness and tension within that organ, and the copious sero-mucous discharge from it, which are well known as ordinary symptoms of coryza.—*Assoc. Med. Journ.* March 24, 1854.

3. *Mucous Membrane of the Stomach.*—The *Zeitschrift für Rationelle Medizin* contains a paper by ECKER on the glandular apparatus of the gastric mucous membrane, which he examined shortly after death in the bodies of several young men who had committed suicide. The account he gives is as follows: In the middle part of the stomach, there are only simple cylindrical glands, each from half to three-quarters of a line in length, and about one-fiftieth of a line in diameter. They lie vertically, and are somewhat club-shaped at the closed extremity, just, in fact, as Sharpey and others have described them. The mucous membrane of the cardiac end of the stomach is composed of similar follicles; only many of them are bifurcated at the end. In the pyloric portion, we find, besides these, granular-looking bodies, which are glands somewhat resembling those of Brünner in the duodenum, and the ducts of which branch and end in grape-like vesicles. The function of these glands is, no doubt, to furnish mucus for the lubrication of the pylorus, where the bolus of food is necessarily most consistent. Besides all these, we meet with "lenticular glands," resembling the solitary follicles of the intestine, which have a diameter of from a quarter to three-quarters of a line, and are least abundant along the great curvature of the stomach.—*Assoc. Med. Journ.* March 24, 1854.

4. *The Anatomy and Surgical Relations of the Fascia of Scarpa.*—Mr. J. STRUTHERS states (*Monthly Journ. Med. Sci.* May, 1854), that the fascia of Scarpa is not commonly understood fully, in the simplicity of its anatomy and the importance of its surgical bearings. In the common mode of conducting the dissection of the groin, he maintains that the true origin of the fascia is divided, and its nature and connections cannot be seen or understood. He gives the following directions for demonstrating this fascia: "Having reflected the skin of the groin for some inches both above and below Poupart's ligament, divide the superficial fascia of the thigh two or three inches below Poupart's ligament, and, using the point and handle of the scalpel, turn it up, off the surface of the glands and superficial vessels, to about an inch above Poupart's ligament. Next, by an incision curving down from the anterior-superior spinous process of the ilium to near the symphysis pubis, divide the fascia through its whole depth, down to the tendon of the external oblique; and now dissect the whole fascia down, close off the external oblique. The lax cellular tissue here, yields almost to the handle of the scalpel, but at, or immediately below Poupart's ligament, the handle of the scalpel is suddenly and firmly arrested. The dissection is now done. Now take the femoral end of the dissected superficial fascia in one hand, and the abdominal end in the other hand, lifting them up a little, and stretching them up and down, and, on looking in below, a thin semi-transparent fibrous membrane is seen, passing between the superficial fascia and the fascia lata. This is the fascia of Scarpa, fully displayed; but if the glands and surrounding cellular tissue be now carefully picked away from its lower or femoral aspect, the fascia will be more clearly seen."

Mr. Struthers gives the following description of this fascia:—

"The fascia of Scarpa arises from the fascia lata close below Poupart's liga-

ment, passes upwards for an inch, and blends with the common superficial fascia. It is separated from the lower part of the tendon of the external oblique, behind, by very loose cellular tissue, and, before, from the common superficial fascia by the superficial glands and vessels; and is continued inwards around the cord, becoming continuous with the fascia of Colles, or true superficial fascia of the perineum. It is a thin aponeurotic or fibrous membrane, forming a barrier or septum across the groin, by passing between the fascia lata and the common superficial fascia. When the urine, infiltrated in the perineum, has been directed upwards along the cord by the fascia of Colles, the fascia of Scarpa prevents it from passing down the front of the thigh. In relation to hernia, it assists in directing an inguinal hernia into the scrotum, and forms one of its coverings; it is covered by a femoral hernia, and tends to prevent the femoral hernia from passing upwards on the abdomen, by means of its union with the common superficial fascia."

5. *The Process of Repair in Teeth.*—The recent number of *Guy's Hospital Reports*, contains a very interesting communication by Dr. S. J. A. SALTER, on the laws which regulate the formation of the "Dentine of Repair," one of the forms of what has been called secondary dentition, or that after formation by which the pulp cavity of the tooth is diminished or obliterated, after the tooth has attained a mature and adult condition.

There are three forms of secondary dentine: *Osteodentine*, in which the new tissue is arranged in systems resembling the Haversian systems of bones around isolated bloodvessels; the dentinal tubes radiating from each centre. It always occurs in states of irritation or inflammation of the pulp. *Dentine excrescences* are little nodules of secondary dentine, occasionally found attached to the interior of the pulp cavity of otherwise healthy teeth. *Dentine of repair* is the special subject of the paper. This deposit is thrown out within the pulp cavity, *opposite* to that part of the external surface of the tooth where a fracture or wearing of the original dentine has taken place, thus thickening the body of the tooth opposite the injured part, so that teeth which are worn down even level with the gum still present no cavity.

This process corresponds with the most beautiful exactness to the external lesion; as long as the enamel only is injured, no dentine of repair is deposited; but as soon as any of the dentine tubes are broken off or worn away on the surface of the tooth, so soon is there thrown out at their opposite extremities towards the pulp a deposit, limited with the utmost exactness to the injured tubes; not mathematically opposite, therefore, to the injured part, but *physiologically* opposite, according to the wavy course of the tubes. The dentine of repair is clear and translucent, and the part of the original dentine involved in the process becomes also more transparent than usual, in consequence of its tubules being filled up with solid matter.

6. *Elasticity of Arteries considered as a Cause of Animal Heat.*—Dr. WINN read before the Physiological Section of the Medical Society of London (May 8, 1854), a paper on this subject. The author stated that fourteen years since he had published, in the *Philosophical Magazine*, some observations which tended to prove that the elasticity of the arteries formed an important element in the generation of animal heat. About seventeen years since, while making some experiments with caoutchouc, he was forcibly struck with the property it possesses of evolving heat when suddenly elongated, and was led at the time to infer the probability of other bodies being similarly endowed. The elastic coat of arteries, especially, appeared to be one of the substances likely to exhibit this calefactory principle; and, in the event of this being the case, he thought it would not be unreasonable to conclude, that the incessant contractions and dilatations of the arteries during life must form an efficient source of animal heat. Three years subsequently he was induced to resume the subject, and, upon making an experiment with part of the aorta of a bullock, he was much gratified in being able to verify his previous conjecture. The experiment was performed as follows: Having cut off a circular portion of the descending arch of the aorta, about an inch in length, he laid it open and care-

fully removed its external and internal coverings. He then pulled it to and fro with a continuous jerking motion, in imitation of the systole and diastole of the artery, for the space of about a minute. Immediately on discontinuing this movement, he placed it upon the bulb of a thermometer, and had the satisfaction of noticing, after the lapse of two minutes, that the mercury had risen as many degrees. On removing the thermometer, the heat diminished rapidly. To be certain that the increment of heat was not derived from any source other than that in question, he took the precaution of covering his fingers with a double layer of flannel, to prevent the communication of heat from the body. He also covered his mouth with a handkerchief, to guard against the warm breath affecting the thermometer while watching the progress of the experiment. It may also be right to state that the experiment was performed in a room without a fire, the temperature of the air being 55°. There were several difficulties to contend with during the investigation. The chief impediment appeared to be the moisture of the artery, which, by its evaporation, had a tendency to carry off a portion of the heat. However, by carefully drying the artery with a cloth, he succeeded in obviating this difficulty to a considerable extent, and was enabled to perform the experiment twice consecutively in a satisfactory manner. He had also, within the last fortnight, repeated the experiment in the presence of a medical friend, with an equally satisfactory result. His attention was often arrested, while conducting the experiments, by other mechanical analogies between caoutchouc and the elastic coat of arteries. Like the former, the latter could be elongated to twice its ordinary length, and, on suddenly stopping the tension, would return to its usual dimensions with considerable force and a snapping noise. Physiologists, after having clearly proved that a great portion of animal heat is the result of chemical changes in the blood, yet confessed that a residuum of heat is not to be referred to this source. This residuum, he considered, arises from the mechanical action of the arteries. It would be exceedingly difficult to determine the precise quantity of heat given off during each beat of the artery; but if we admit the development of only a very small quantity, it necessarily follows, from the circumstance of the action of the arteries being in incessant operation during life, that the heat must quickly accumulate to a great extent; and it is even probable that the body, unless cooled by the functions of the skin and lungs, would, in a short space of time, become preternaturally hot. The following physiological and pathological facts appear to corroborate the views he had taken as to the mechanical source of heat:

1. The minute distribution of the arteries to every part of the system insures a general and equal distribution of heat.
2. The rigidity of the arteries in old age is a probable cause of the diminution of animal heat at the close of life.
3. The increased warmth of the body after exercise seems to be readily explicable upon the principle of increased force of the arteries.
4. In many diseases of the lungs, where their functions are at fault, and at a time when the arteries are beating with great strength and velocity, the heat of the body is found to be above the usual standard.
5. Medicines which diminish the action of the heart and arteries almost invariably reduce the temperature of the body.
6. The heat of local inflammation, in cases where the constitution does not sympathize to any extent, cannot be satisfactorily referred to any other source, as the arteries immediately in the neighbourhood of the affected part are throbbing violently, when the capillaries (which are supposed to play so large a share in the chemical theory) are generally considered to have their action impeded. Dr. Crisp has hinted that many cold-blooded animals are remarkable for the great elasticity of their arteries. This fact could not affect his theory. The languor of the circulation in this class of animals more than counterbalances any calefactory effect which might otherwise be produced by the resiliency of their arterial structure.—*Med. Times and Gaz.* May 27, 1854.

7. *Starch in the Brain.*—In our previous number, p. 466, we gave an account of the discovery by Purkinje and Virchow of starch globules in the human nervous centres. Mr. Busk states (*Microscopic Journal*, number 6) that he has satisfied himself of the structural and chemical identity of these bodies with

starch. He found these "corpora amylacea" in vast numbers in and on many parts of the brain (as on the septum lucidum, for example), in a patient who had died of cholera; and the cerebral substance in immediate contiguity with them appeared quite natural.

In the corpora striata he could find few or no starch grains, but an appearance presented itself which seemed to him to be connected with their formation. Many particles of sabulous matter were met with, which were lodged in irregular masses of what appeared a fibrinous or immature connective tissue substance; and, upon the addition of iodine, each mass of crystals was found to be immediately surrounded by an irregular thickness of a transparent matter, which was turned, not *blue*, but a light *purplish pink* by that reagent—a substance, in fact, closely resembling in that respect the very early condition of the cellulose wall.

8. *On the Relation that Fat bears to the Presence of Sugar in the Livers of the Mammalia and Birds.*—Dr. GIBB, in a paper read before the Physiological Section of the Medical Society of London (April 10, 1854), drew the attention of the Society to the bearing which the amount of fat in the livers of man and animals, and birds, possesses in relation to the presence of sugar in that organ, which may hereafter lead to some important deductions with reference to its pathology in connection with saccharine assimilation. From a series of experiments which he had performed upon the livers of birds and some of the mammalia, from 1849 to 1852, with the original object of estimating the quantity of sugar present, he found that those which possessed much fat invariably contained a larger quantity of sugar than those, again, which appeared to possess very little, if any, of that substance. Thus, among the mammalia, in dogs and sheep, whose livers possessed nothing unusual in their ordinary characters, the presence of sugar was demonstrated, but in quantity exceedingly small, as compared with that found in the seals, whose livers again were absolutely gorged with fat, and contained a very large quantity of sugar. Among birds, the livers of the palmipedes, or web-footed tribes, and the grallæ or waders, which, in most of the species, contained quantities of fat, were found to possess a much larger quantity of sugar than the livers of the gallinæ, or poultry, which were remarkable again for the absence of fat, as compared with the former. To apply this discovery to man, he instituted a comparison in regard to the quantity of sugar between healthy livers and the state termed "fatty liver," common in phthisis pulmonalis, and experiments clearly proved that the amount of sugar found in the fatty liver very much exceeded that of the normal healthy liver. These experiments were repeated with the same results on numerous occasions, with the examination also of other organs besides the liver, but which would not now be noticed. The importance which the knowledge of this fact is likely to bear, in connection with the secretion of fat and sugar by the liver, their relations to one another, and their connection with the function of respiration, cannot be over estimated. Bernard has clearly demonstrated the presence of sugar in the hepatic veins going from the liver, the inferior vena cava, and right side of the heart; in other words, in the blood going to the lungs, but none in that returning from those organs, unmistakably showing that the saccharine element must undergo some chemical change in the lungs, as has been inferred by Magendie. He was not prepared to say what influence the presence or absence of fat may possess in relation to the secretion of sugar in the liver, but it is a fact of sufficient importance to engage the attention of physiologists in connection with saccharine assimilation. The fact, too, of more sugar being found in the "fatty liver" of phthisis than in the healthy liver, may possibly be the result of the interference of the ordinary combustion of the lungs, owing to the arrest of function in portions of those organs, arising from the tubercular deposition. He would only just hint, at present, at a possible relationship between secondary mal-assimilation of saccharine matters and tubercle, but some further experiments are necessary before pronouncing an opinion.

Dr. Gibb has not examined the liver of diabetic patients in relation to the quantity of sugar contained in that organ.

9. *On the Deposit of Fat in certain Conditions of the Bodies of the Lower Animals.*—Dr. CRISP directed the attention of the members of the Physiological Society (March 13, 1854) to the above fact, which, as far as he knew, had hitherto escaped observation. A short time since, a Chilian eagle, which had been seventeen years in the Regent's Park Zoological Gardens, died suddenly from rupture of a bloodvessel in the lung. The bird was in beautiful plumage, in good condition, and weighed $6\frac{1}{2}$ lbs. Dr. Crisp found the lungs so tuberculated that scarcely a sound portion could be seen; notwithstanding this extensive pulmonary lesion, the bird was excessively fat; the flakes of fat in the pelvis and abdomen being large and solid. Dr. Crisp had met with many instances of a similar kind, more especially in birds; but they were not confined to this class of animals, for he had seen many examples of fat pigs with tuberculated lungs, and he had learned from some feeders of cattle that an ox with a damaged lung would often make fat sooner than a sound animal. Perfect rest, however, was necessary; and it should be borne in mind, that if the thoracic lungs of a bird were diseased, other parts of the body would take on a compensatory action. Dr. Crisp thought that the explanation of the phenomenon, in these cases, was, that the carbon not being eliminated by the lungs was converted into fat by its union with hydrogen and oxygen. In making a comparison between pulmonary tubercle in man and the lower animals, it is important to remember that the latter are not affected with the exhausting purulent discharges which generally occur in the human species.

Dr. Routh could understand the theory of the beneficial action of cod-liver oil in cases of phthisis, but was of opinion that Liebig's theory, as to the generation of fat in that disease, was not established. The respiratory efforts are increased to forty per minute in phthisis; and the amount of carbon and hydrogen eliminated is so great that a deposit of fat seemed to be impossible. The like remarks would also apply to birds, since their respirations are exceedingly frequent.—*Med. Times and Gaz.* April 1, 1854.

10. BERNARD, *on the Conditions under which Certain Substances, usually retained in the Blood, pass into the Urine.*—During active digestion, the blood of an animal contains a certain amount of sugar, which does not pass into the urine; but if the mass of blood be diminished by bleeding, the urine soon becomes saccharine. If we inject a limited quantity of saccharine matter into the blood of an animal ($\frac{1}{2}$ gramme for a rabbit fasting, of the weight of 2 kilogrammes) the urine will contain no trace of sugar; but if the animal be bled before being subjected to this experiment, a certain quantity of sugar will pass into the urine.

M. Bernard explains these phenomena by considering that the loss of blood renders the animal of less volume, reducing it to the condition of one of smaller size. It is known that small animals may be poisoned by doses easily borne by larger animals of the same species; and animals, after being bled, cannot withstand the same dose of a poison which they supported previously. These facts were formerly attributed to the increased absorption caused by the withdrawal of blood; but in the experiments above related, absorption had no part, as the substances were introduced directly into the circulation.—*Month. Journ. Med. Sci.* from *Soc. de Biologie*, 1853.

11. *Case of Diarrhoea Adiposa, confirmative of Bernard's Views with Regard to the Functions of the Pancreas.* By Mr. MARSTON.—This case was that of a man, a labourer, aged 35, who had frequent purging of a very peculiar fatty-looking substance. No tenderness, swelling, or tumour could be detected, except a slight enlargement of the liver.

"His appetite was good, and he had a great desire for saccharine matters, fat meat, and hydro-carbons generally. Urine was passed in abnormal quantity, pale in colour, with very slight reaction on litmus; sp. gr. 1.030. Under the microscope it presented a few epithelial scales, and a number of oil globules; on evaporating a portion, and treating it with ether, these globules were dissolved; there was less than a normal amount of urea and lithic acid, and no

albumen; but Trommer's test, yeast, and oxide of silver, indicated the presence of sugar, and the skin was harsh and dry, though he did not complain at all of these diabetic symptoms, *all* of which disappeared some time before death. Tongue was slightly furred; gums, and inside of lips, were pale and flabby. Pulse 100, but it varied during treatment from 90 to 120. No cancer, phthisis, or cardiac affection could be traced in any of his family, which had been generally healthy. The matters passed from the bowels presented a very fatty, tenacious, and peculiarly slimy appearance, deficient in bile, and altogether different from feces. On raising a portion on the point of a knife, it appeared in greasy masses. Under the microscope, numerous epithelial scales, with mucus, and a substance laden with oil-globules, in every respect similar to fat, were observed. Their fatty nature was rendered more evident by their being soluble in ether, and with liq. potassæ forming a semi-opaque gelatinous mass exactly resembling soft soap."

The treatment consisted in abstinence from "farinaceous and saccharine articles of food. Diaphoretics succeeded, though with difficulty, in producing diaphoresis; the sweat had no acid reaction on litmus. Olive oil, instead of being beneficial, only increased the discharge of fatty matter and deranged the stomach. Purgatives, mercurials, and counter-irritants, with small doses of iodide of potassium and ungu. iodin. to region of liver, and a variety of other remedies failing to produce relief, at last, all medical treatment was discontinued. His appetite continued good, but the adipose diarrhoea (four or five stools per diem upon the average) continuing, he gradually sank, and died apparently from asthenia, after having been under medical treatment altogether thirteen to fourteen months."

On dissection, the duodenum was "quite healthy, but the head of pancreas appeared to be converted into a hard, schirrhoid tumour, which did not press upon the ductus communis choledochus, as in most of the similar recorded cases, whilst the body and other parts of gland were atrophied, and its duct was found perfectly obliterated, and degenerated into an impervious cord."

Mr. Marston concludes some excellent observations on this case with the following remarks: "Mialhe, adopting Bernard's views, explains the action of the pancreatic juice upon the fats by the principle of fermentation. More recently, another set of physiologists, of whom Frierichs and Lenz appear to be the most prominent, have published a series of experiments to prove that its absence in the intestine does not prevent the digestion and assimilation of fat. They experimented upon cats, and found the usual amount of fatty matter in the chyle, after ligature of the pancreatic duct. Here, then, exists a division between two leading sects of physiologists; for Bernard still adheres to his views. The question comes, does this case in any way fill up the gap? It has been advanced by Bouchardat and Sandras, that the free alkali of the juice, or serum of the blood, would be sufficient to dissolve the fat; and, adopting Plattner's theory, the soda of the bile may be almost vicarious of the action of the pancreatic juice, in separating the fatty ingesta by saponification with its acid. But whether this could continue long is not evident, and what appears to be the real root of the question is, whether, under long continuance, this would not be incompatible with existence. In the case before us, it is possible that the choleate of soda of the bile, and the alkalies of the serum, did supply an alkali to the fat, thus saponifying it, and that the choleic acid, if Plattner's views be correct, taking albumen as its base, the resulting compound, which is the product of so highly an organized gland as the liver, on the one hand, and the chemical and antiseptic action of the gastric secretion, which is doubtless itself not a mere chemical combination, but a definite semi-organized material, on the other, might give rise to the fatty albuminous molecular base of the chyle; but that, this continuing, these alkalies were not only insufficient in quantity to the digestion of the fatty ingesta, but an imperfectly-organized material was formed, from which an imperfect chyle resulted.

"The presence of sugar in the urine in this case is difficult of explanation; but be it remarked that this was a transitory symptom, existing most when the patient partook largely of amylaceous matters, which are easily capable of being resolved into sugar, and ultimately ceasing altogether long prior to

death. It might be that a larger than normal amount of fatty matter was presented to and absorbed by the tributary branches of the vena porta, and that, through the intermediate action of the liver (if Bernard's views be correct), this was resolved into sugar and carried by the hepatic vein into the general circulation; and, being more than normal in quantity, did not altogether undergo decomposition in the lungs (as it is conceived to do) but was excreted by the kidneys. Again, the deposit of fatty matter in the viscera generally, may admit of a similar explanation; for, I apprehend, we must regard the fatty degeneration of the liver not as a primary affection, but as a secondary disease, resulting from the actual deposition of fat from the blood, it being found in a similar condition in almost all diseases attended by much emaciation. Perhaps the fatty ingesta in this case, not having been subjected to the vital and chemical action of the pancreatic secretion, had never reached a sufficiently organized state to undergo the necessary changes, prior to assimilation or secondary combustion.

"Looking at the case in all its bearings, it certainly appears to support Bernard's views, which I cannot consider entirely overthrown by the experiments before quoted."—*Glasgow Medical Journal*.

[This is a valuable addition to the six cases of Bright, Elliotson, Lloyd, and Gould, and supports, from the result of pathological observation, the experimental researches of Bernard with regard to the functions of the pancreas. In a memoir, published in the last number of the Prague *Vierteljahrschrift*, by Dr. Eisenmann, a seventh case is quoted by Lussanna, in the *Giornale Veneto di Sc. Med.* t. ii. 766, and an eighth case of his own is given. In the last two instances, however, the individuals recovered, the principal symptom having been discharge of fatty matters by stool.]—*Monthly Journ. Med. Sci.* Jan. 1854.

12. *The Effects of the Recumbent Position during Syncope, physiologically considered*.—Mr. RICHARDSON read before the Physiological Section of the Medical Society of London (April 20, 1854) a paper on this subject. The author commenced by stating that though the fact, that the recumbent or horizontal posture often affords marked and immediate relief in syncope, is generally admitted, no very distinct attempt had hitherto been made to explain the principles on which it acted. One view, however, had fixed itself in the professional mind, and required to be carefully refuted. This view is, that the horizontal posture relieves syncope, by allowing the blood to gravitate to the brain and medulla, so that these centres, gaining energy by this process, react on the heart and supply it with new vigour. This theory had been supported by many writers, among whom the author quoted Dr. Alison, of Edinburgh, Dr. Ash, and Sir George Lefevre. The latter author relates a case in which syncope occurred on the patient assuming the erect position. It was found to be connected with the presence of varicose veins in the leg, and was prevented by the application of bandages. In this case, Mr. Richardson observed, that the brain being deprived of blood was secondary to the fact that the propelling power of the heart was to a great extent lost through the mechanical impediment in the course of the circulation—an impediment which the bandages relieved. It was also obvious that the blood detained in the lower parts of the body could not reach the brain without first passing through the heart. Moreover, any renewed force which the heart might receive from the nervous centres would be quite useless until it contained blood on which to act. When we perform transfusion, we do so for the purpose of filling the heart with its natural stimulus, not for the immediate purpose of exciting the nervous centres. The recovery of consciousness on laying a person in the supine position is no proof of the correctness of the hypothesis above mentioned; for, when consciousness ceases during syncope, it ceases as a consequence of failure of the circulation, and returns in proportion as the circulation becomes re-established. Mr. Richardson had observed that the first symptom of recovery from syncope invariably was the return of the heart's beat, and that then muscular motion, consciousness, and animal heat followed. Again, in some instances, the action of the heart fails, while the functions of the nervous system remain perfect; and, on the other hand, the manifestations of the nervous system may be suspended by narcotic

poisons, while the heart continues to beat with power. There may also be extensive disease of the cerebro-spinal axis, and yet the heart's action remains unaffected. Again, in the animal kingdom, the size of the heart and activity of the circulation bear no relation to the development of the nervous system; and, in the formation of the vertebrate embryo, the heart begins to pulsate before it is connected with any nervous centres. Mr. Richardson next proceeded to offer his own theory of the manner in which the recumbent position produces recovery from syncope. The explanation appealed to mechanical laws, and was very simple. It must be remembered, that the arterial blood sent from the heart first ascends, and that the venous blood descends from the upper and ascends from the lower parts. When blood is withdrawn from the upper part of the erect body, the heart loses its power of sending the blood along the aorta; hence the blood, losing the *vis à tergo*, gravitates in the veins in the lower half of the body. At the same time, the heart not having sufficient power to propel the blood to the brain and other parts, consciousness is lost, and voluntary motion and the production of animal heat fail. Death would now soon occur, from the heart ceasing to pulsate, and from the blood coagulating in the veins; but the body falls, or is laid down, and then the blood contained in the veins of the lower part of the body is poured into the heart, and again it excites to contraction. Thus the whole circulation is restored, and the brain and every part of the body receiving a fresh supply of blood, resume their proper functions; but to no one of these parts is due the least credit for having restored the movements of the heart. When blood is withdrawn from the lower part of the body, the chances of recovery are much lessened; for what was in the former case a reservoir, now becomes a running cistern. The recumbent position is here equally valuable, since it leads to a distribution of blood through the vessels above the heart. It might be even an advantage to put the head, in these cases, slightly lower than the trunk, until the cause of the hemorrhage was removed. But, in general, the recumbent position is all that is required. The manner in which the killing of calves is performed in slaughter-houses, was adduced by Mr. Richardson as an instance of the effects produced by position on the loss of blood. He next proceeded to speak of syncope dependent on an over-burdened condition of the heart, or on debility of the cardiac walls. In these cases, the recumbent position enables the blood to pass more readily into the pulmonary artery and aorta, while the venous circulation generally is rendered more equable. Mr. Richardson then referred to several experiments which proved to demonstration the truth of his theory. Having slowly narcotized a kitten, he laid bare the heart by a careful dissection, without opening the right pleural cavity; he then punctured the arteria innominata, while the animal was suspended by the head. The heart continued contracting for some minutes, but at last the right auricle collapsed, and pulsation ceased. At this moment the body of the animal was reversed, and suspended by the heels. The auricle instantly refilled from the inferior cava, and the heart resumed its contractions. This was repeated with the same results. On another occasion, the vena cava inferior was tied previous to the reversion of the body, when no reaction took place until the ligature was removed. In a third experiment, the animal was suspended, in the first place, by the heels, and, the abdominal aorta being punctured in the middle, the auricle was allowed to collapse as before; the animal was then turned head upwards, when the auricle filled from the superior venous trunks. There could be no doubt as to the results of these experiments.—*Med. Times and Gaz.* April 22, 1854.

MATERIA MEDICA AND PHARMACY.

13. *Local Anæsthesia.*—The production of local anæsthesia in disease by the direct application of the vapour of chloroform, was first brought into notice by Dr. Hardy, of Dublin, and has lately excited much attention. (*Dublin Quart.*

Journ. Nov. 1853.) The instrument, which he invented for the purpose, consists of a small metallic cylinder holding a sponge; a pipe is fixed at one end, and to the other end a gum elastic bottle is attached, provided with a valve for the admission of air. When the sponge is moistened with chloroform, pressure on the elastic bottle forces the vapour through the pipe, and the jet of vapour can be applied directly to the affected part. The first effect produced by the application of the vapour is a sensation of heat, which may even cause uneasiness; but this is quickly succeeded by the subsidence and complete removal of pain. The relief is said to be as speedy and perfect as it could be by inhalation, and is so permanent that the pain does not return again for several hours, and then only in a mitigated degree; and there are no unpleasant subsequent effects, such as occur after inhalation, or after the use of narcotics. Dr. Hardy relates several cases, mostly of uterine disease, and generally of a very painful and distressing kind, in all of which immediate and complete relief was afforded by the anæsthetic douche. His first case was one of cancer of the uterus, where the excessive suffering was completely removed in a few minutes by the chloroform vapour; and, when the pain afterwards returned, the same means were always resorted to, with a like effect. In his second, third, fourth, and fifth cases, which were examples of uterine disorders, attended by great pain of the back and womb, the douche applied to the uterus caused the cessation of the pain, first in the back, and afterwards in the womb. In a case of great irritability of the nipples, in various other forms of local irritation, and particularly in a case of pruritus pudendi, the anæsthetic application was equally successful. From this experience of its action, Dr. Hardy considered the local application of chloroform to be preferable to its inhalation.

Thus strongly recommended, the practice was repeated lately in Paris pretty extensively, but, unfortunately, the same success has not attended it. At first, indeed, M. Nélaton, after applying the douche, incised an abscess on the foot without the patient showing any symptoms of pain; and M. Dubois produced insensibility in a painful abscess of the axilla and wound of the back of the hand. But, with these and a few other exceptions, the douche has generally failed to produce anæsthesia in surgical operations. Velpeau used it, without success, in a case of abscess and of cancer; Giraldes and Nélaton, in subsequent trials; Gosselin and Ricord found no effect produced by it; and Roger found it completely ineffectual in producing anæsthesia on the sound skin. Laugier, however, states that he has found this method of service; and, in a case of painful stump, after amputation of the thigh under M. Larrey, the patient felt great relief from his sufferings after the chloroform application. With the view of increasing the effect, several modifications of the original apparatus have been devised. M. Richard used a much larger reservoir of chloroform, and expelled the vapour by means of bellows. In a case of fistula in ano, in which he employed this apparatus, however, no other effect was produced except a sensation of cold; the incisions were as painful as they generally are. M. Maisonneuve having to perform amputation of the toe, enveloped the foot in an India-rubber bag, in which chloroform was poured, so as to keep the part in an anæsthetic vapour bath; but, instead of insensibility, a painful feeling of burning, and so much local hyperæsthesia were produced, that the operation had to be put off till next day. In short, the method has so completely disappointed expectation, that the surgeons have latterly entirely abandoned it. M. Velpeau, indeed, thinks that ether and chloroform cannot produce anæsthesia unless they are absorbed by the lungs; and he looks upon Dr. Hardy's cases as not at all conclusive, in consequence of the irregular duration of uterine pains, and their frequent spontaneous disappearance. Yet, even in uterine cases, M. Dubois has renounced its use as quite ineffectual. In consequence of the failure of Dr. Hardy's plan, attention is now being directed to other local anæsthetics. Velpeau still uses in many cases the production of cold by a freezing mixture of ice and common salt; and the cold produced by the volatilization of ether and chloroform has lately been tried, as likely to be more efficacious than the douche. M. Ricket has employed this local anæsthesia by refrigeration in two cases, with partial success. The chloroform or ether was dropped upon the part, and its volatilization hastened by a current of air from

a ventilating apparatus which M. Mathieu has invented. The incisions which were made in the part caused so little pain, that there could be no doubt that the sensibility was deadened by this means.¹

[However deficient in success hitherto, local anæsthesia is of such great practical importance, that it well deserves to be investigated and carried out, with all the appliances and resources of modern ingenuity. We understand that Professors Syme and Simpson have made trial of the anæsthetic douche in cases of abscess, but although a slight and superficial impairment of the sensibility was noticed, no diminution of the feeling of pain took place. Dr. James Arnott's plan, of freezing the skin and subjacent parts, seems to us well adapted for minor operations in surgery, and we feel surprise that it has not been more extensively tried in this country. M. Velpeau has pronounced strongly in its favour. M. Richat has published some cases of operations performed without pain, by means of an ingenious apparatus, constructed by M. Mathieu (figured *Gaz. des Hop.* April 1). By this instrument, a fair amount of ether and of air is simultaneously directed to the part. Dr. Liégard (de Caen) has pointed out that the peasants in Lower Normandy have long been in the habit of preventing pain by squeezing strongly the forearm or leg, when operations are performed on the hands or feet.]—*Monthly Journ. Med. Sci.* May, 1854.

14. *Production of Local Anæsthesia.*—Dr. SNOW in a paper read before the Physiological Section of the Medical Society of London (April 10, 1854), said that when a piece of folded lint moistened with chloroform was applied to the skin, and covered with some impermeable substance, it caused a sensation like that occasioned by a mustard poultice, and the skin became red. After the lapse of a few minutes to half an hour, there was a feeling of numbness in the part, and its sensibility was diminished, so that pricking with a needle did not cause so much pain as usual. He had never succeeded, however, in causing complete anæsthesia by chloroform applied to the sound skin, nor had he been more successful with several other agents he had tried. Hydrocyanic acid of 5 per cent., and a strong solution of cyanide of potassium in water, caused a diminution of sensibility, with less irritation, than any of the other medicines. The difficulty of causing local anæsthesia depended on the slow and sparing manner in which fluids permeated the cuticle, and the readiness with which the small quantity which did permeate was carried away in the blood. When the skin was denuded of cuticle, it was readily made insensible, even by the vapour of chloroform confined over it, and the raw surface could be rubbed without causing any sensation. The only means they as yet possessed of producing complete local anæsthesia was that of refrigeration, proposed by Dr. James Arnott. When a part was cooled by the application of a mixture of pounded ice and salt, it became of the colour of parchement, as hard as suet, and perfectly insensible. The insensibility, however, extended to only a very slight depth. He had congealed in this manner part of the palmar surface of the hand and fingers, but, on separating the latter, and examining the dorsal surface of the web which connects them, he found it quite sensible to the pricks of a needle, even when removed from the hard and insensible palmar surface by a thickness of only the tenth of an inch. The burning kind of pain caused by the application of ice and salt to a sensitive part, such as the hand or fingers, was very considerable, and it was still greater about five minutes afterwards, when sensibility returned. In fact, if he (Dr. Snow) required the application for any other purpose than to watch its effects, he would inhale chloroform while it was done. He therefore did not think this process very available, even for superficial operations, except when the surgeon or the patient had an objection to the inhalation of chloroform. A new plan had lately been tried in the hospitals of Paris, with some amount of success, in preventing the pain of minor operations by refrigeration. It consisted in dropping ether on the part, and increasing the evaporation with the bellows. He (Dr. Snow) had tried this on a patient of Mr. Ure, in St. Mary's Hospital, preparatory to his dividing the callous edges of an ulcer of the leg. It was quite successful at that edge

¹ *Gaz. de Hopitaux.*

of the ulcer where the ether had chiefly fallen, but less so at the other side. This process caused less pain than the application of ice and salt; and M. Matthieu, an instrument maker of Paris, had contrived a means by which it could be more effectually applied than in the above case.—*Med. Times and Gaz.* April 22, 1854.

15. *Modes of Exhibiting Cod-liver Oil.*—Those who have had large experience of the use of cod-liver oil must have been astonished at the surprising way in which, in a great majority of cases requiring its exhibition, it agrees. It is not easy to mark out beforehand any class of symptoms which contra-indicate its employment, if the existence of strumous disease call for it. Often symptoms, apparently the most likely to be aggravated, are removed or mitigated by its use in a way which surprises both patient and prescriber. Thus, in phthisical cases, a red tongue, acid eructations, biliousness, heartburn, liability to sick headaches, aching pain between the scapulæ, an instinctive and intense dislike to fat or greasy aliment, are symptoms which, without a question, may often be remedied by the use of cod-liver oil. These statements are, of course, applicable only to a certain proportion of cases; there are others in which its use is clearly indicated, but in which the prescriber's ingenuity is taxed to the utmost to get the patient to bear the remedy. The following memoranda on this part of the subject, founded on our observations of the practice of the various London hospitals, but more especially of the City Hospital for Diseases of the Chest, may probably be acceptable to some of our readers.

Cases in which difficulty occurs may be divided into the following classes: 1. *Those in which the nauseous taste of the oil forms the obstacle.* In these, the use of the pale oil will generally obviate the difficulty; it is, however, four times the expense of the brown, and is more liable to be adulterated, which are great objections. The taste of the brown oil may often be concealed by taking it floating on some bitter menstruum. A wineglassful of strong coffee, of ginger wine, of infusion of quassia, or, perhaps, best of all, a quinia draught, containing a drachm of the tincture of orange-peel, may serve this purpose. The oil may be stirred up in a little hot milk, and swallowed so warm that the sensation of heat overpowers the taste. Should these expedients fail, the patient may be instructed to put into the mouth a teaspoonful of marmalade or of black currant preserve; and, having well lubricated all parts with the sweetmeat, so as to fully absorb the attention of the gustatory nerve, then swallow the oil. Advantage frequently results from closing the nostrils when taking the dose. 2. *Those in which the oil excites sickness, and is quickly rejected by vomiting.* Many of the expedients mentioned above will obviate also this source of difficulty, more especially the use of bitters. Very few cases, indeed, will resist the influence of hydrocyanic acid and bismuth exhibited three times daily for a few days preparatory to the trial of the oil, and continued during its employment. Patients should be directed to eat a little dry biscuit or bread-crust before the oil, and then to take it floating on a cup of the coldest spring water. If these fail, as a last resource, the dose should be given in the recumbent posture, that is, in the morning, an hour or two before getting up, and in the evening, after going to bed. This last suggestion is one emanating, we believe, from Dr. Birkett, of the City Chest Hospital, and often answers very satisfactorily. 3. *Cases in which the oil cannot be digested.* This is a large and very important class. Patients complain that they have a great repugnance to the oil, that it makes them feel sick for hours afterwards, though seldom causing actual vomiting; that everything they take after it seems to taste of it, and that thus all relish for food is destroyed; that the oil "rises," either oily or with a most nauseous acid flavour. They frequently have bilious attacks; and, for a day or two in each week, probably the repugnance to the remedy is so great that they are quite unable to overcome it. If pushed under these circumstances, the oil does more harm than good; there are, however, extremely few such cases in which careful attention to the digestive organs will not enable its administration to be successfully conducted.

We copy the following formula from the Pharmacopœia of the City Hospital

for Diseases of the Chest, where it is in general use for the relief of the condition alluded to:—

“R. Rad. rhei ʒij; rad. zingiberis ʒij; rad. gentian. ʒiiss; sūd. carbon. ʒij; aquæ pur. ℥viij.”

The roots having been cut into small pieces, the infusion is made with cold water, and, after standing in a cool place for about twelve hours, is strained. The mixture is clear and bright, and not in the least disagreeable to take. In cold weather it keeps well; but in summer the addition of some tincture is necessary to prevent it from becoming thick. If cough be present, it is usual to combine with each dose from half a drachm to a drachm of paregoric; if sickness, from two to three minims of the hydrocyanic acid; and if the signs of atony, a small quantity of the tincture of gentian, calumba, or hop.

By the use of this mild stomachic (a wineglassful thrice daily), for a week or two before commencing the oil, and then continuing it during the exhibition of the latter, many patients have been induced to bear it with advantage, who had failed in other prior attempts to do so. The mixture is so mildly aperient that it almost never purges, and it may be continued for months together with the effect of improving most markedly both the appetite and the digestion. 4. *Cases in which the oil, although taken easily, cannot be borne in full quantity, and does not appear to produce much benefit.* In many cases of phthisis coming under this head, the combination of tonic medication with the use of the oil often answers well. So generally, in fact, is advantage derived from such combination, that at the Hospital for Chest Diseases there are very few patients, indeed, who take the oil alone. In almost all it is exhibited together with either the stomachic mixture above noticed, or some form of tonic. The favourite tonics are the sulphates of quinia and iron (gr. j with gr. ij ter die), or the sesquichloride of iron. The latter is extensively prescribed with the mineral acids in infusion of quassia; or, if the stomach be delicate, in the following more elegant form, which is a prescription of Dr. Risdon Bennett's: R. Tinct. ferri sesquichl. ℥x; acid. nitrici dil. ℥x; syr. zingib. ʒss; aq. menthæ viridis ʒj. Ft. haust.

Notice has been made repeatedly in our hospital records during the last two years of the practice of combining the use of cod-liver oil with that of mercury, in small doses long continued, which prevails extensively at the Hospital for Skin Diseases, in cases of lupus and cutaneous struma, and at several other institutions, in the treatment of chronic disease of the joints. This kind of treatment is becoming increasingly prevalent, and is certainly very applicable to many forms of strumous inflammation. The plan of giving the oil to counteract the depressing effects of a mercurial course for the cure of syphilis in cachectic states of the constitution, has also been previously mentioned, and is well worthy of being borne in mind.—*Med. Times and Gaz.* May 13, 1854.

16. *Medicinal Constituents of the Lemon.*—Dr. COGSWELL read before the Physiological Society (March 13, 1854), a paper on this subject. The author's attention had been drawn to the subject by the publications of Dr. Owen Rees and others, on the treatment of rheumatism by lemon-juice. The remedy was no doubt often successful, but still there were many failures, and these did not seem to be sufficiently accounted for, as had been attempted, by reference to the different forms of the disease. But there was much uncertainty as to what constitutes lemon-juice. Has it always the same composition, or are there not various accidental circumstances attending its preparation calculated to render one specimen different from another. Sometimes the fruit is ordered to be squeezed at home; at other times, the prescription is sent to a druggist, or the juice is allowed to be purchased where it is cheapest. When obtained from the fresh fruit, by depression without removing the rind, it is a turbid, pale yellow fluid, exhaling a grateful odour of the essential oil, and of a specific gravity in different specimens between 1043 and 1047. By distilling a fresh specimen which had not been filtered, the author had obtained half a drachm of essential oil from 12 ounces; but from another, after filtering, only five minims. Some purchased at a fruit-preserver's, which had stood for several months in a cask, and undergone a kind of fermentation, was bright yellow and clear, of density

1037, and yielded scarcely a trace of oil. It had received an impregnation of common salt from the former contents of the cask. A sample of lime-juice procured from the stores of a merchant-vessel, snielt of rum, had a density of 1010, and contained a multitude of torulæ. The effect of the different processes for preserving the juice mentioned by Christison, was to separate one or more of the ingredients which might be of consequence to its remedial efficacy. Heat would expel the essential oil, filtration remove the solid débris of the pulp, and the addition of alcohol with filtration withdraw the mucilaginous matter. It appeared that the druggists in London do not make a practice of keeping the juice ready on demand, but that they generally prepare it extemporaneously when ordered. The dose was equally a matter of uncertainty; for although it seemed to be the prevalent idea, that the average produce of a lemon was about half an ounce, the author had not found any ordinary specimen yield less than seven drachms, while the average was rather more than an ounce. Hence, he concluded that it was necessary to fix a standard for the composition and dose of the juice before its remedial efficacy could be fairly tested. In the rind, besides the essential oil, the author observed granules of starch. A decoction of the white spongy portion, afforded pectine, hesperidine, and a trace of tannin. Hesperidine was discovered by M. Lebreton, who employed orangettes; but his process was complicated and unproductive. The author had found it was deposited in crystals by evaporating a decoction of the white portion of the rind, while in the same portion of the shaddock, it was separated by simple maceration in cold water, and could be collected in a filter. A specimen as thus obtained was exhibited to the Society. It had a decidedly bitter taste, but turned, not red as commonly stated, but yellow with sulphuric acid. Another principle, called *aurantiin*, was supposed to be the source of the bitterness of the lemon tribe; but its existence had not been demonstrated, and the author was disposed to regard the hesperidine as the true bitter principle. The essential oil belonged to a class corresponding to the formula C_5H_4 , and including the other oils of the fruits of the lemon tribe, the oils of turpentine, juniper, savine, elemi, copaiba, cubebs, and pepper. Some of these were approved remedies in rheumatism. From trials made with the oil of lemon in this disease, in flatulent dyspepsia and leucorrhœa, the author entertained a high opinion of its therapeutical value, and thought that the chemical fact stated suggested the possibility of bringing together the various scattered evidences on the medicinal effects of the oils of the same class, and referring them to a general law. The fresh juice, when evaporated in a water bath, yielded about 8.5 grains per cent. of solid extract, and the ash obtained by incineration amounted to 0.27 per cent. The latter contained sulphuric and phosphoric acids, potash, lime, magnesia, and iron. A controversy existed as to whether the citric acid or the potash in lemon-juice was the true medicinal constituent. In reviewing the evidence, the author conceived that it was an error to suppose that either of them would answer so well separately. The efficiency of the juice probably depended, not on one or other of the ingredients, but on the whole combined. In using the essential oil, he gave it in doses of about fifteen minims, with two drachms of vinegar in decoction of barley, always being careful to keep the bowels freely open by purgatives. If the efficacy of lemon-juice in acute rheumatism depended on anything more than the refrigerant action of the citric acid, he thought the true anti-arthritic agent was the essential oil when present, and repeated his views as to the necessity of establishing a standard for the composition of the juice. A Table was exhibited, showing the correspondence in composition of the non-oxygenated essential oils mentioned in the paper.

Dr. Routh differed from the author as to the chemical elements in the lemon, to which the medicinal virtues might be attributed; the author attributing them to the essential oil, while Dr. Routh and Dr. Garrod believed them to be due to the potash. Acting on this principle, he was accustomed to give half to two drachm doses of potash, and thus add to the effect which would be obtained from lemon-juice. Dr. Garrod had not observed disease of the heart to follow rheumatism when treated in this way; and, moreover, he cured his cases within one week. The theory of the action of the potash, is simply that of neutralizing

the lithic acid, but its action is far more effectual if lithic or tartaric acids be united to the base. Oil of juniper, acting as a diuretic, had also in his practice effected the cure of rheumatism and gout within a week.

Dr. Glover referred to two classes of diuretics. The one by which the quantity of water is alone increased, as by the action of the oil of juniper; and the other, by which the solid constituents of the urine are increased in quantity, as by the action of acetate of potass. He was not disposed to admit that great simplicity in the exhibition of remedies is necessarily the best mode of procedure, and instanced the meconite of morphia as a better medicine than morphia or opium simply.

Mr. Headland criticized the author's statements somewhat severely, and was of opinion that the paper had proved nothing, and that the analyses were rather empirical than scientific. He doubted if the medicinal virtues of the lemon-juice could be attributed either to the essential oil or to the potass, for the small quantity of oil would be dissipated by boiling; and sailors who eat salt-beef, containing much potass, are subject to scurvy, and are cured by the administration of lemon-juice. Moreover, Dr. Bryson had remarked, that while the administration of nitrate of potass alone had benefited but one case in five ships'-crews, all the others were cured by lemon-juice. Fresh vegetables in general are sufficient to cure scurvy without Dr. Garrod's remedy of nitrate of potass; and it is very questionable if the disease can be cured apart from the use of vegetables. Upon the whole, he could attribute the good effects accruing from the use of lemon-juice to the combined citric acid and salts of potass.—*Med. Times and Gaz.* April 1, 1854.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

17. *Pyrosis, its Causes, Pathology, and Treatment.*—Dr. GEORGE BUDD, in an interesting lecture (*Med. Times and Gaz.* March 18, 1854) on Pyrosis, observes it is most probable that the disorder in countries in which it is endemic, is mainly owing to the influence of climate and to the diet of the poor not being sufficiently varied, and consisting too much of coarse and innutritious farinaceous food.

But if such be the main causes of the disorder, there are, doubtless, various other conditions that assist in bringing it on. Most of these may be classed under two heads:—

1. Excessive labour, insufficient clothing, loss of blood, and all other conditions that tend to exhaust the body.

2. Pregnancy, constipation, anxiety, and other conditions that tend to disorder the functions of the stomach.

We have already seen that waterbrash occasionally occurs in a high degree in the wealthy classes, especially in women, where it cannot be ascribed to any peculiarity in diet, and seems to be owing solely to such conditions as these.

Pyrosis, then, considered with reference to its exciting causes, is of two kinds:—

1. That which has been termed by some writers *symptomatic* pyrosis, which is brought on (without any peculiarity in diet) by pregnancy or some other condition that disturbs the functions of the stomach.

2. That which has been termed, in contradistinction to the former, *idiopathic* pyrosis, which prevails chiefly among the agricultural poor in rural districts, and which seems, in most cases, to be mainly owing to defective diet.

Many conditions conspire to render the disorder much more frequent in women than in men. Women are much more frequently in states of debility from the nature of their constitutions, and from their having in suckling and in excessive or unnatural uterine discharges, causes of exhaustion from which men are exempt; they have also more excitable nervous systems, and, in con-

sequence, the functions of the stomach in them are more apt to be deranged by mental influences and by disease in other parts of the body; and among the lower classes, they have generally a less nutritious diet, since the men, in order to support their more laborious work, take or have accorded to them a larger quantity of animal food and of malt liquors than is consumed by the weaker sex.

In the treatment of waterbrash, our first endeavour should, of course, be to remove the conditions that may seem to have brought it on or to maintain it.

If the disorder should seem to be caused mainly by a diet not sufficiently nutritious or consisting too much of farinaceous substances, the most effectual remedy will be a wholesome nourishing diet, containing a proper quantity of animal food *in its most digestible form*. Little permanent benefit can, indeed, be expected from medicine unless the diet is improved.

If the disorder should seem to have been induced, or to be kept up, wholly or in part, by fatigue, it is very essential that the patient should rest; if by constipation, that this condition should be removed by purgatives, such as aloes or colocynth, that do not offend the stomach.

After these points have been attended to, much further good may be done by medicines.

The medicines that have been found most useful in pyrosis are—

1. Medicines which have an astringent action on the coats of the stomach. Among these may be classed bismuth, lime-water, and the vegetable astringents—kino, catechu, krameria, logwood.

2. Sedatives, especially opium and the salts of morphia, which probably also tend to restrain undue secretion by the mucous membrane, but which are chiefly of use in allaying the gastralgia that attends pyrosis.

Medicines from these two classes may often be combined with advantage. Five grains of bismuth with a twelfth of a grain of the muriate of morphia, or five grains of the compound kino powder, or an efficient dose of catechu, krameria, or logwood, with opium, may be given two or three times a day.

3. Some other medicines have obtained repute in pyrosis which cannot be classed with the preceding. They have most of them an astringent action on the coats of the stomach, but act, directly or indirectly, on the nervous system as well.

The chief of these are, nitrate of silver, which may be given in pills, in doses, of half a grain, three times a day; nux vomica, which may also be given in pill, in the dose of from three to five grains, three times a day; quinia; and the mineral acids.

Some of the medicines I have mentioned have been popular remedies for pyrosis in districts in which the malady has prevailed.

It is stated that nux vomica is a popular remedy among the Laplanders, to whom it was recommended by Linnæus, and that lime-water was some years ago a popular remedy among the rural population of North Wales.

4. The disorder is often connected with anæmia; and steel is of great service, both in removing it and in preventing its recurrence.

The medicines of which I have had most experience in disorders of this class, and which are probably as efficacious as any, are bismuth, with morphia; krameria, and logwood, with opium; and steel.

18. *Treatment of Diabetes*.—Several cases of diabetes have recently occurred in the hospitals, in which the treatment so much vaunted two years ago by the permanganate of potash, has been tried fairly, without any good result. The theory which refers the original lesion to the nervous system, and considers the defective assimilation as a mere symptom, appears to gain ground among London physicians. Dr. Goolden, of St. Thomas's Hospital, related, in some clinical remarks, a few weeks ago, the case of a guard of a railway train, who received a severe blow on the head; and, while suffering from the consequent cerebral symptoms, passed large quantities of sugar in his urine. Guided by this fact, as well as by the well-known experiments on animals, in which saccharine urine may be produced by irritation to the floor of the fourth ventricle, Dr. Goolden has directed his measures of treatment to the head.

Four cases have been or are under treatment in St. Thomas's, and in two, very great benefit appears to have been derived; the patients, meanwhile, not being restricted in any way as to the use of vegetable diet. Shaving the scalp, blisters to the nape or scalp, and small, repeated doses of mercury have been the chief measures adopted.

19. *Topical Uses of Iodine.*—The value of iodine as a counter-irritant is year by year becoming more generally appreciated, and is yet much less so than it deserves. The iodine solution will probably, before long, entirely supersede mustard plasters, being at once more efficient, and much less disagreeable in its employment. The following notes on its applications will, perhaps, not be useless; they are the results of very extended observations in the hospitals generally, but more especially in those devoted to the treatment of diseases of the chest—1st. In the pleuritic stitches, or aching pains in the chest, so commonly recurrent in the course of phthisis, the iodine paint,¹ applied over the affected spot, usually affords, without any expense to the vital powers, much more relief than either leeches, sinapisms, or blisters. It may be used in almost all conditions of the system with perfect safety. 2d. In cases of aphonia or hoarseness, depending on inflammatory thickening of the parts concerned in the production of voice, great benefit may be derived from painting the iodine over the front of the throat externally. 3d. If the mucous lining of the fauces, etc., be thickened and congested, the solution may, without risk, be freely applied to the part itself. 4th. In the treatment of chronic enlargement of the tonsil, the application of iodine to the gland itself will sometimes effect a cure, but is much less generally efficient than constitutional treatment. 5th. In cases of chronic pleuritic effusion, or of consolidation of the lung, the solution should be painted over a large extent of the diseased side, and is of great service when the period for blistering or leeching has passed. 6th. Applied extensively over the belly, iodine is a useful counter-irritant in the incipient stages of strumous peritonitis. 7th. In strumous ophthalmia, the application of the pharmacopœial tincture to the skin of the lids is often effectual in relieving intolerance of light; much benefit may also be derived from like practice in cases of granular lids. In both instances, frequent repetition is necessary. 8th. In all forms of periostitis, whether syphilitic, strumous, or the result of injury, iodine paint is invaluable. 9th. It is needless, perhaps, to mention the employment of iodine as a local application to bronchocele, to inflamed joints, and to enlargements of the absorbent glands; with regard to the latter, a point is worthy of being borne in mind, to which Dr. Budd was, we believe, the first to direct attention, viz., the propriety of applying it to the skin beyond, and not over the affected gland, so as to allow of its being absorbed and taken through the gland in the course of the lymphatic circulation. 10th. Injections of iodine into the cavities of abscesses, glandular or otherwise, appear most frequently to produce good results, and to be unattended, except in very exceptional instances, by any risk. The theory of their use is, that they provoke adhesive and not suppurative inflammations, as, for instance, in the radical cure of hydrocele. 11th. In cases of contracted cicatrices after burns, in which treatment by extension is adopted, the application of iodine is of advantage in causing the absorption or softening down of the indurated structure. Some cases illustrative of this have recently been under care in the Middlesex Hospital. Care must be exercised, or ulceration may be caused. 12th. In cases in which the patient cannot be got to swallow medicine, as now and then happens in phagedæna of the throat, the specific influence of iodine may be induced by its endermic application, the best method being to paint over large surfaces of skin the pharmacopœial tincture, choosing a different part each time.

¹ The following is the formula for the solution alluded to:—R Iodinii $\mathfrak{z}\text{i}$; sp. vini rectificati $\mathfrak{z}\text{i}$; Ft. solutio. This should be allowed to stand by in a glass-stoppered bottle for some months before required for use. It then becomes thick and syrupy, rapidly dries when applied to the skin, and does not run. It should be applied with a camel-hair brush, one, two, three, or more coatings, according to the wished-for effect.

The reason why, as a counter-irritant in all forms of chronic inflammation, iodine appears so superior to other applications, is doubtless to be found in the fact that it is capable of absorption, and may thus act beneficially in two distinct methods.

We have enumerated above some of the chief uses to which the iodine solution is daily put in the practice of the London Hospitals, but do not profess to have mentioned all. These are, however, enough, we think, to prove its right to a place on the dispensing table of every Medical practitioner.—*Med. Times and Gaz.* April 8, 1854.

20. *Abortive Treatment of Smallpox by Zinc Plaster.*—Dr. JOHN HUGHES BENNETT remarks that the efficacy of the mercurial plaster in preventing pitting in cases of smallpox is well established; but excessive and dangerous salivation has been occasioned by it; and he asks whether the mercury is necessary for the success of the treatment. "Its original propounders in Paris," he remarks, "may, indeed, have supposed that the absorbent powers of the drug was the true cause of its success, but it seems to me that another explanation may be offered. There is, for instance, a close analogy between the mode of healing of wounds and ulcers, so well described by Dr. Macartney of Dublin—that is, the so called 'modelling process'—and what takes place in the ectrotic treatment of smallpox. In the former, cicatrices are far less liable to be produced than after healing by the first or second intention; and in the latter, the pitting or cicatrization is prevented. The artificial plaster, therefore, takes the place of the natural scab or clot of blood, protects the parts below, and enables them to heal slowly but more perfectly than if exposed to the air uncovered and uncompressed by superjacent crusts. If this be the correct theory of the ectrotic treatment, the mercurial might be discarded, and any kind of plaster which would concrete on the face, might be expected to produce the same beneficial result. This session, I determined to try the effects of such a plaster, and after two or three failures have succeeded in procuring one that answers perfectly. The first case I treated with simple lard, thickened with starch and powdered charcoal, but it was so little coherent, that the patient, during the night, rubbed it off on her pillow or with her hands, and on her recovery she was pitted all over. In another case I tried carbonate of magnesia saturated with oil. But this also failed. In a third case, however, common calamine (*zinci carbonas*), saturated with olive oil (proposed by Mr. Bird, one of the clinical clerks), formed a coherent, tough crust, which remained on the face, and was found to answer well. Three cases of natural smallpox have been since treated in this manner with the result, not only of preventing the pitting, but of diminishing the local and general symptoms, exactly in the same manner as I have formerly detailed, as being the effect of the mercurial plaster. The following is one of these cases:—

"Alexander Ross, æt. 13, never been vaccinated, was seized with shivering on the 7th January, followed by the usual symptoms of fever. Entered the Infirmary on the 9th, when a few papules were observed on the face and arms. On the 12th, the face was thickly covered with vesicles, which from their closeness would certainly have become confluent. The mask of calamine and oil was now applied. The disease ran its usual course, the eruption being confluent on the arms and trunk. Throughout the progress of the case, the application of calamine saturated with oil preserved a firm and coherent crust, and was renewed from time to time. The patient experienced no smarting of the face, there was no swelling of the eyelids, no purulent discharge, or local unpleasant symptoms of any kind. The secondary fever was tolerably smart, delirium being present two days. On the 22d the mask came off, leaving a clean smooth surface, free from all trace of pitting. Dismissed quite well on the 26th.

"The following formula, after numerous trials, has been found to constitute the most efficient plaster: Carbonate of zinc, 3 parts; oxide of zinc 1 part, rubbed in a mortar with olive oil to a proper consistence."—*Monthly Journ. Med. Sci.* April, 1854.

21. *Formula for Gouty, Rheumatic, and Neuralgic Affections.*—In the *Revue de Thérapeutique Médico-Chirurgicale* for Oct. 1, 1853, we are told that Dr. GAFFARD, of Aurillac, has endeavoured to ascertain the composition of a secret remedy named pills of Lartigue, which have proved very successful in the treatment of rheumatism, gout, and neuralgia. Dr. Gaffard has arrived at the following formula, which he has communicated, in order that it may be tested, to the Society of Medicine of Toulouse.

R. Extract of *sabadilla*, prepared with boiling alcohol, gr. xvss; Barbadoes aloes, and pure Aleppo scammony, of each gr. lxxviiss.—Mix. To be divided into ninety-six pills.

Two of these pills are given every six hours, until the bowels have been purged from four to ten times in twenty-four hours.

To obtain a complete and lasting effect, the use of the pills must be continued for some time. But as their purgative effect increases in direct ratio to the repetition of the dose, and as it is important to regulate this effect and render it uniform, the intervals between the doses should increase in an arithmetical ratio of three hours. Thus, after having obtained the wished for effect, whatever number of pills may have been taken, the dose is delayed for three hours: that is to say, if the interval has been six hours, it becomes nine; if nine, it becomes twelve; and so on, until twenty pills have been taken which is generally sufficient.

After each dose of the pills, a cup of tea should be drank; and this should also form the ordinary beverage of the patient while the purgative effect continues. There must also be an interval of at least an hour between the dose of pills and the hour of rest.—*Assoc. Med. Journ.* Feb. 10, 1854.

22. *Cotyledon Umbilicus in Epilepsy.*—W. H. RANKING, M. D., of Norwich, in a letter to the *Med. Times and Gaz.* (April 1, 1854), states that, as far as his own experience goes, he should pronounce the cotyledon utterly worthless as an anti-epileptic remedy. "I have given," he says, "Hooper's preparation, carefully and perseveringly, in more than thirty cases, public and private, in patients of all ages, and in not a single instance could I trace the smallest benefit to its operation. Many cases certainly were of an unmanageable character, and derived no benefit from any other medicine; but I have notes of three or four in which a most marked improvement has followed the use of the oxide of zinc in increasing doses, after several weeks had been, as I consider, wasted in the employment of cotyledon."

23. *Oxide of Zinc and Hyoscyamus for arresting Colliquative Sweatings in Phthisis.*—Dr. THEOPHILUS THOMPSON recommends (*Lectures on Phthisis*) oxide of zinc and extract of hyoscyamus, of each four grains, made into two pills, to be taken at bedtime, for arresting the colliquative sweating in phthisis.

24. *On the Influence of Belladonna in Counteracting the Poisonous Effects of Opium.*—Dr. Graves had first suggested that in continued fever, with protracted pupils and coma, if an agent administered internally would occasion dilatation of the pupils, it might also relieve the other symptoms of cerebral derangement. Dr. THOS. ANDERSON, acting on this theory, administered large doses of belladonna in two cases of poisoning by opium, which he related as follows:—

A patient, of whom I had charge, and labouring under delirium tremens, having received an overdose of a solution of the muriate of morphia, became comatose. He had taken, in thirty-six hours, two ounces of the solution of the muriate of morphia, and it had been continued by the attendant after sleep was procured. When I saw him he was in profound coma, his breathing was stertorous, amounting to no more than four or five per minute, and his pupils were contracted to mere points. His pulse was excessively weak, and rather slow; it was quite impossible to rouse him. I ordered him immediately the following mixture: Tincture of belladonna six drachms, in five and a half ounces of water, of which an ounce was to be given every half hour. Three ounces of the mixture were administered with great caution, after which his

pupils began to dilate. The six drachms of the tincture of belladonna were taken, and in four and a half hours after the first dose of it was given, the patient was in the following condition: The coma was entirely gone, respirations were between twenty-two and twenty-five per minute, the pupils were much dilated, the pulse had risen to nearly one hundred and twenty in the minute, and was also increased in strength. His countenance, also, from being cold and pallid, had become much flushed, and the whole body was much warmer. He replied readily and coherently to all my questions. He continued to improve for three days after, when, rising suddenly to stool, he fainted, and before the assistance of the nurse could be procured, he was dead.

A fortnight afterwards, a woman, about fifty years of age, took, at 4 o'clock P.M., two drachms of laudanum, and at half-past 5 P.M. three drachms more. She was brought to the Infirmary at 8 o'clock P.M. After making vain attempts to rouse her from the coma, by walking her about, etc., the stomach-pump was used at a quarter past 8 o'clock. By this means her stomach was thoroughly evacuated, but no trace of opium was detected by smell or sight. It had probably been all absorbed. A current of electricity was then applied to her hands for nearly ten minutes, but without rousing her. I saw her at a quarter to 9, P.M. for the first time, and on being told that she had been poisoned by laudanum, I determined to try the effects of belladonna.

At that time her pupils were contracted to mere points, her respiration was stertorous, ten per minute, the pulse was feeble, and the extremities rather cold. Between 9 and half-past 9, I gave her one ounce of tincture of belladonna in three ounces of water, which was all swallowed, but with difficulty. In the course of the next half hour two drachms more were administered. At 11 P.M. the first alteration in the size of the pupil was observed; the respirations had also then increased to twelve or thirteen in the minute, and the pulse was much stronger. The symptoms continued to improve till 2 A.M., when all indications of opium poisoning had disappeared. The woman was then sitting up in bed talking to the nurses, with pupils dilated to a little more than their natural size, and still slightly sensible to light. The extremities were quite warm, the pulse was about 100, and of good strength.

She gave me a coherent account of her motives for taking the poison, of the amount of money she had spent in purchasing the laudanum, and the names of the druggists where it had been procured. She also replied sensibly to questions about her family, and the age and occupations of her children. She continued awake till nearly 4 o'clock A.M., after which she slept till 9 A.M. In the morning I found her pretty well, her pupils being no more dilated than they were four hours after the first administration of the belladonna. She complained, however, of nausea, but unaccompanied with vomiting. This symptom, along with the dilated pupils, had entirely disappeared in the course of two days. She was kept in the hospital, under observation, for ten days after the accident, at the end of which time she was dismissed, perfectly well. The tincture of belladonna, used in both these cases, was of the strength of four ounces of the leaves to two pints of rectified spirit, and prepared by percolation. Half a drachm is considered a full dose. I have seen dilatation of the pupil produced by a drachm given at once.

The committee on Dr. T. Anderson's communication reported that they had designed to test Dr. A.'s views by experiments on animals, but had found, on inquiry, that the animals commonly used for experiment were almost entirely insusceptible of the poisonous action of opium or belladonna. Where the effects were so different from those observed in man, it was obviously impossible to pursue the investigation which they had intended. The committee, therefore, while recognizing that Dr. A.'s views require more extended observations in order to confirm them, deemed them worthy of the attentive consideration of the Society.—*Proceedings of Physiological Society of Edinburgh, in Monthly Journ. Med. Sci.* April, 1854.

25. *Local Use of Veratria, especially in Scrofulous Affections of Joints.*—In the *Glasgow Medical Journal* for Jan. 1854, Dr. J. B. KLINGNER has published a paper on the local use of veratria. He considers that it is a powerful specific

stimulant; and that it is also an anodyne, different, however, from opium, seeing that it does not blunt the nervous sensibility, but rather excites it, which is known by a tingling, pricking feeling, often accompanied by slight shocks resembling those that are produced by electricity, and by an increase of temperature, yet unaccompanied by redness, which, however, does take place when veratria has been used for a long time, and when there is a tendency to active inflammation. It is inadmissible in active inflammation; but when this stage has subsided, of extraordinary efficacy in chronic non-malignant swellings, especially around joints; it is inadmissible where there is heat and redness. Where, however, the active stage has passed, it is eminently useful in removing residual indurations and swellings: thus, in tumefactions and stiffness of joints, often remaining after sprains, luxations, and other injuries, it is valuable; as well as when synovial membranes are thickened, ulcerated, and a partial ankylosis is established. Indolent and scrofulous ulcers assume a healthy action, and cicatrize, under its use.

The mode of application must be varied according to circumstances; and it requires a great deal of perseverance, both on the part of the patient and the medical attendant, as also much caution and care. Dr. Klinger commonly uses the veratria in the form of ointment, in the proportion of five to ten grains to the ounce of axunge, the veratria being at first dissolved in a little spirits of wine or tincture of capsicum. Of this, about the size of a small bean is rubbed into the affected part with the bare hand, gently and steadily shampooing it for a quarter of an hour. Under this treatment, the swelling will decrease sensibly every day, the joint will assume a more healthy aspect, and become gradually more movable, particularly if passive motion be daily practised. No bandage of any kind is to be applied. If the patient be anxious to have the part covered, a slip of flannel may be loosely applied. Mercurial ointments are worse than useless; shampooing, even with simple ointment or oils, is preferable; and when veratria cannot be longer tolerated, on account of the itching it produces, sometimes over the whole body, friction with glycerin is of immense value. Glycerin seems to be readily absorbed, and soon gives a more natural look to the limb, which remains moist and comfortable long after its use.

Dr. Klinger finds veratria useful in cases of deposit resulting from the non-removal of effete matter. These deposits occur most frequently in such situations as the knee and elbow joints. There is a feeling of coldness in those localities when affected by the disease, and anything that gives heat for a time is grateful; yet warm applications, although of temporary relief, always tend to increase the disease. Cupping, leeching, blisters, etc., are often used with the view of causing diminution of the swelling, and, as it is said, to subdue inflammatory action. Dr. Klinger's opinion is, that diseases which are benefited by veratria are principally those caused by want of vigour in the whole system.

"When a joint, such as the knee or elbow, is affected by scrofulous deposit around it, and consequent ulceration, and partial or complete ankylosis is established, and the whole has become a bluish-white unshapely mass of certainly destructive disease, doughy to the feel, and of a lower temperature than natural, some time will elapse before the veratria seems to have any effect; yet, by persevering daily with its use, and shampooing, it will gradually diminish in size, and the distended, cadaverous-looking skin will present a more natural appearance, and acquire a higher temperature. If the frictions with veratria ointment be persevered in at night, and a tincture composed of concentrated tincture of capsicum, with twenty grains of veratria to the fluid ounce, be rubbed in the following morning, after a time redness and heat will take the place of the former whiteness and cold, the joint itself will become more movable and at the same time somewhat more painful. If the disease do not give way by absorption of the effused matter, we must endeavour to induce healthy inflammation by stimulating the diseased mass, by which means the scrofulous deposit is transformed in great part into laudable pus. Such, however, can only be the case when the constitution has undergone a favourable change. The doughy mass becomes softer, and fluctuation is per-

ceptible at different points; but the process of absorption is very slow. The best plan is, to introduce an exploratory needle, or a sharp-pointed bistoury, into the tumour once or twice, and continue the veratria frictions. In about two days after this operation, the heat and redness in the part will increase, and a painful throbbing will be complained of by the patient, with concomitant sympathetic fever. The wound made by the needle or bistoury discharges for a few days merely a little hot serum, after which the swelling, as well as the heat and redness, rapidly increase. These symptoms ought not, however, to alarm the medical attendant. He ought not to apply too soon fomentations and poultices: the inflammation must be allowed to come to a point which insures success, after which poultices may be applied to moderate its violence, and some cooling medicine, and, if the case require it, even colchicum may be given. When the inflammation is subdued as far as is desirable, the poultices may be continued for about two days longer, and the use of the veratria cautiously resumed. In most cases, it will now be found necessary to give a free outlet to the pus; and, when it is completely evacuated, the bottom of the wound is to be touched every day with lunar caustic, and dressed simply with dry lint; or, if very indolent, with equal parts of Venice turpentine and resinous ointment. Care ought to be taken, at the same time, not to touch the wound during the frictions with veratria ointment. After the contents of the abscess are discharged, the joint becomes more distinct, but, at the same time, more painful to the touch. The integuments around it, from the partial destruction of the periosteum, adhere gradually to the bones themselves, which, however, in course of time, are again somewhat loosened; and a loss of substance of the bone and adjacent tissues becomes apparent, by depressions in the site of the evacuated abscesses. Some little deformity is generally left, and the motion of the joint is often somewhat impaired; yet, where the disease has not been allowed to proceed too far, the cure is complete. Where, however, the articulating surfaces are enlarged, or complete ankylosis is established, a complete restoration of its mobility can never take place. After the deposit surrounding the joint has been either absorbed or evacuated, the remaining stiffness is at least to a great degree removed by steady and cautious attempts at flexion and extension of the limb, repeated daily, and shampooing with veratria ointment at first, and towards the end of the cure with glycerin. Besides the enlargement of the articulating surfaces, the contraction of the flexor muscles impedes very much the extension of the limb. As the joint increases in size, the limb above becomes atrophied, and the whole at last becomes a source of bodily and mental distress and irritation.

"The hip-joint is also not unfrequently the seat of the disease, which here implicates not only the joint itself, but is apt to carry its destructive tendency to the whole os innominatum. In this joint, owing to its depth, veratria has not the same power, and cures of hip-joint disease must be more rare and difficult than those of the more superficial joints.

"In hydrops articuli, produced by external injuries or cold, in or appearing as a sequela of typhus fever, scarlatina, measles, etc., effusions from sprains, dislocations, and chronic inflammation of the bursæ and tendons, veratria is of the greatest efficacy; but it can only be used with safety and benefit, when no active inflammation is present, or when the constitution is not much broken down by long standing local or constitutional disease. When the patient is affected with tubercles of the lungs and mesenteric glands, we cannot expect a cure; but mere irritative fever from local disease does not warrant us in giving an unfavourable opinion."

The vertebræ sometimes become affected by scrofulous disorganization; we must, therefore, be careful before we give too favourable a prognosis, in cases where even the lungs and abdominal viscera appear to be healthy. We must not in all cases rely alone upon the efficacy of veratria locally applied, for the condition of the viscera may have had much to do in the beginning with the after development of the local symptoms. Dr. Klingner illustrates his observations by the records of five cases of diseased joints.—*Assoc. Med. Journ.* Feb. 10, 1854.

26. *Exercise of the Vocal Organs in Ulceration of the Larynx.*—In the *Journal de Médecine et de Chirurgie Pratiques* for October, 1853, we learn that in the laryngeal ulceration which frequently complicates pulmonary consumption, M. Trousseau applies, as local remedies, solution of nitrate of silver, calomel with finely powdered sugar, nitrate of bismuth with sugar, arsenicated cigarettes, fumigations with chlorine, iodine, etc., and instead of prescribing silence, as all writers have done, he orders his patients to speak. He thus employs a mode of treatment analogous to that adopted in cases of ulceration of the leg by M. Boyer, and which is advocated in this country by Mr. Chapman and others. M. Boyer allowed his patients to walk about and follow their ordinary occupations when their legs were bandaged; and the result was a more rapid and solid cure, the cicatrix acquiring a strength which rendered subsequent laceration of comparatively infrequent occurrence. M. Trousseau waits until the acute or painful stage of the disease has passed; he then submits his patients to a course of gymnastic exercises of the voice and speech. He is careful not to make them speak in a low voice; having learned from priests, many of whom lose their voice, that the confessional fatigues the larynx more than preaching in a large church.

During four or five months at least, the patient reads slowly and aloud five or six times daily, taking care, at the end of each period, or member of a period, to inspire as much air as the lungs can contain. He must make deep inspirations, and then emit several sounds in succession without exhausting the provision of air. He then inspires again, and emits fresh sounds, always avoiding the higher notes, which are fatiguing. If, after several months, the voice remains false or obscure, we may employ a process which was the secret of a professor of singing named Larochette, but which M. Trousseau accidentally discovered. Larochette used to desire singers who had lost their voice, to take as deep an inspiration as possible, and then, suddenly and in as little time as possible, to emit a short shrill note, expelling all the air which they had inspired. M. Trousseau has seen many persons who had lost their voice for years, recover it in a fortnight under this treatment. It must be remembered, however, that vocal gymnastics have succeeded only when the ulceration of the larynx has been cured, and when the aphonia has been, properly speaking, only a want of power arising from the prolonged rest of that organ.—*Assoc. Med. Journ.* Feb. 10, 1854.

27. *Neuralgic Headache.* By J. MURPHY, M. P.—Neuralgic headache is synonymous with those headaches described by some old authors as hemicrania, by others as clavus hystericus, and by Dr. Graves as hysterical congestion. It is peculiar to females, and to females during a certain period of their existence only, from puberty until the final cessation of the menstrual secretion. Dr. Graves gives a graphic description of the symptoms and of the injurious effects of the usual routine treatment. He calls it hysterical congestion; but he seems not to have understood its true pathology. There is no doubt of its being hysterical; but there is no congestion, for the seat of the pain is in one of the nerves of the scalp, which can be easily proved by a slight examination, and it is therefore an external headache. The error may have arisen from his having met with cases where this headache was in combination with the anæmic headache. The proper name which should be bestowed on this headache, in order to facilitate the diagnosis, is spinal irritation of the sub-occipital nerve. Spinal irritation is beginning to be well understood in this country; we are indebted to a French physician, M. Valleix, for the discovery. Since then many other disorders, such as irritable mammae, pleurodyne, and neuralgic headache, are discovered to originate in functional derangement of the spinal cord; and I believe whoever will carefully compare these disorders with cases related by Dr. Tilt, must come to the conclusion that they are nothing more than symptoms of subacute ovaritis. They are hysterical disorders, and *hysteria is subacute ovaritis*, which displays its phenomena on the sensitive and motive nerves of the spinal column.

On comparing the neuralgic headache with the phenomena of spinal irritation in other parts, we find how exactly they coincide. Like spinal irritation,

it is a form of hysteria, and therefore peculiar to females. It is not only peculiar to females, but attacks them only during the menstruating period of their existence; that is, from about the thirteenth to the fiftieth year. It is exacerbated just previous to menstruation, makes its first attack on the left side, and rarely passes over to the right side.

Cause.—As this form of headache is peculiar to the female sex, it must therefore have its origin in some organ peculiar to them; and as it is felt during a certain period of existence only, the organ must have the performance of its functions limited to that period. As there is no organ by which these two facts are explicable, unless the ovary, it is not unphilosophical to conclude that the disorder proceeds from the ovary. There is certainly also the uterus, but the functions of this viscus cease on the removal of the ovaria. We daily meet with the uterus inflamed, ulcerated from cancer or cauliflower excrescences, distended by hydatids or pregnancy, producing moles and polypi, but none of the phenomena of spinal irritation are present. In the married female, who bears children regularly, it is scarcely ever known. Before the commencement of menstruation, or after its termination, it is equally rare.¹ What is the state of the ovary, I do not pretend to affirm. If inflammation, yet it has often yielded to tonics; it may depend on moral causes, but such an explanation has never satisfied me. An accumulation of feces in the rectum has appeared to me as occasionally the source of irritation; in a few cases, I think it was traceable to ascarides in the rectum. We witness the action of cold in paralyzing the trunk of a motor nerve, the portio dura, as it escapes from its cranial foramen; but cold cannot be a cause of this headache, otherwise why should not the male sex equally suffer.

Occasionally, spinal irritation, in other parts, has been observed earlier in life, but I have not met with the headache; and, as the headache has occurred some years before the appearance of the menses, so I believe it possible it may arise a few years after their total cessation. The headache resembles spinal irritation, also, in a curious and hitherto unexplained phenomena; *commencing* on the left half of the body, we occasionally meet with it also on the other side; but I have never discovered that it began there, nor is it ever restricted solely to that side. When both sides are attacked they are equally so, the left being by far the more painful. As another proof of its being spinal irritation, if further proof be necessary, we find it under two distinct forms, and these forms are easily distinguishable by the nature and extent of the pain. In the one, it is confined to the exact tract of the sub-occipital nerve, it is lancinating or shooting, intermitting, and chiefly felt at its termination in the integuments of the temporal region; when severe in this spot it is the *clavus hystericus*. When the whole course of the nerve and its branches are implicated, the entire left side of the scalp is very tender, sometimes exquisitely so; this is the hemi-crania. It is singular how much this disease is confined to the left side of the head; we find such to be the fact in ninety-nine cases out of a hundred. It seldom reaches the aggravated form of *clavus hystericus* without being accompanied with other well-known hysterical symptoms, which, of course, facilitate the diagnosis.

Diagnosis.—This headache attacks females exclusively. I have never heard or read of men suffering from this kind of headache. It is only during the menstruating period of life that even females are liable. The pain is referred to the left side of the head; it is worse on the approach of the menstrual flow; it is found in the track of the sub-occipital nerve. The course of this nerve is well known; it accompanies the sub-occipital artery, emerging from the spinal canal; it passes along the back of the head, midway between the mastoid process and the mesial line, sending branches to the integuments which cover the parietal protuberance, and terminating in the temporal region. Its course, from its exit to its termination, can oftentimes be accurately ascertained, from the

¹ While writing the above, I referred to Dr. Tilt's work on Diseases of Females, first edition, and in page 58, he gives the valuable fact that he found the right ovary affected in only five out of seventeen cases. Now, it might be worth inquiry to ascertain whether the left had not been previously affected, but that the irritation was transferred to the right, as we see in ophthalmia occasionally.

pain induced by pressing upon it. Although the head suffers, pressure may not always produce the pain, for it is intermitting. In general, however, pain may be thus detected in one of three places; on the left side of the neck, where the head and vertebræ join, at the parietal protuberance, or in the temporal region; when concentrated in the last spot, it is the well-known clavus. It is sometimes painful in all three, and sometimes in its whole track. It is, however, rare that the tenderness is absent in the occipital region. The part suffers more when pinched than when pressed. When the branches as well as trunk suffer, we then have hemicrania, a most painful form, less intermitting than the other, and preventing the unfortunate girl from lying on the affected side. It is more commonly met in the unmarried female, from the twenty-second to the thirty-fifth year; but the married females who are childless do not escape.

This headache is chronic, intermitting, may continue for days, weeks, or months, then subside, and return after the lapse of months or even years. A first attack is seldom felt before the twentieth year, nor after the thirty-fifth. The pain is generally of a shooting kind, darting from the neck towards the temple, and never towards the neck, by which it is easily distinguished from odontalgic pain. Neuralgia of the left mamma (irritable breast), or of the seventh or eighth intercostal (pleurodyne), frequently coexists. It is sometimes found in combination with the anæmic, but more rarely with the congestive headache. From caries of the body of a vertebra it is easily distinguished by the pain being superficial, being confined to the left side of the spine, by its not becoming worse when the head is flexed on the chest, nor by jumping, nor by pressing the head against the spinal column.

This neuralgic pain sometimes accompanies the rotated spine. It is singular how often toothache is mistaken for headache, especially for this form. In both, the pain is described as shooting in the course of the nerves, but in toothache the pain shoots towards the neck and ear, leaves no tenderness of scalp, never goes so high as the parietal protuberance, and is more correctly discovered by learning that a paroxysm is brought on by food, sometimes when warm, at other times when cold.

Treatment.—If the disease be not complicated, we can promise relief. The bowels should be kept open by regulated diet, and by aperients, such as castor oil, olive oil, lenitive electuary, powdered rhubarb, soluble tartar, or the compound rhubarb pill. If the bowels are obstinate, an enema of a pint of cold water daily answers the double purpose of removing the contents which may irritate the ovary, and as a local application to the organ chiefly in fault. The cold hip-bath is a valuable remedy when the constitution is vigorous, but all these things are inferior to sea-bathing. Stimulants should be abstained from, employment should be found for mind or body, but physical efforts are preferable. The sedentary position required by the needle, especially in solitude, is very injurious. A sinapism over the exit of the nerve gives great temporary relief; a vesicating plaster of cantharides is better, but it oftentimes leaves a mark, and therefore, on account of sex, age, and position in life, may be objectionable. A croton-oil liniment, made with one drachm of oil and one ounce of camphorated tincture of opium, and rubbed until pustules appear, is preferable, as it leaves no permanent blemish. The belladonna plaster, mixed with powdered opium, or a liniment of extract of belladonna, rubbed with mucilage, are useful and unobjectionable remedies. Speedy relief is occasionally afforded by veratrine or aconitine ointment, made with from four to six grains to half an ounce of spermaceti ointment. The finger used in rubbing should have a piece of bladder interposed.

One ounce of tincture of aconite, with seven ounces of rose-water, is a safer remedy to trust to inexperienced hands than the veratria. The internal medicines are not so easily chosen. Tonics are frequently required, and they may be combined with anti-hysterical remedies. The disulphate of quinia may be exhibited in a strong infusion of valerian; compound iron pill, with assafoetida in large doses, is very beneficial. If there be irritability of the stomach co-existing with profuse menstruation and leucorrhœa, pills of valerianate of zinc, half a grain three times a day, with one drop of creasote, answer many intentions. If there be much debility, the sulphate of iron may be combined with infusion of valerian and ammonia, or the ammoniated tincture of valerian may

be prescribed. The pain is sometimes so acute that some relief is quickly demanded, and half a grain of morphia will lessen the pain for a while until other remedies have time to act. For the leucorrhœa, one drachm of acetate of zinc to one pound of distilled water is useful as a lotion. But we are sometimes perplexed, for the tonic treatment is not the best for a full plethoric female; leeching or even general bleeding is required, but the cases are rare which require general bleeding.

If the patient be not very weak, and there is much leucorrhœa and menorrhagia, the treatment laid down by Dr. Tilt for subacute ovaritis should be adopted. He leeches in the menstrual interval, and then blisters the iliac regions; but as his work is universally read, the treatment is well known. Sea-bathing, when practicable, should never be omitted.—*Lancet*, May 20, 1854.

28. *Hysterical and Spasmodic Cough*.—Dr. ANDREW WOOD said, at a meeting of the Medico-Chirurgical Society, March 1, 1854, that about a fortnight ago, in a public institution for the education of girls, one of the inmates was seized with a peculiar spasmodic cough, coming on in paroxysms every half hour or twenty minutes, and for some time he was at a loss; but others of the girls becoming affected, Dr. Wood recognized an old friend, which had come under his notice in a kindred establishment about two years ago. About a dozen of the inmates were then affected, and on coughing made a sound resembling the cry of a turkey-cock. No remedy seemed to check the complaint, except removing those affected to their homes. In one case, where from the violence of the symptom the larynx was feared for, chloroform, then but newly introduced, had an admirable effect. In the present instance only three or four cases have occurred, those affected having been removed to separate houses—an opiate usually completing the cure.—*Monthly Journ. Med. Sci.* May, 1854.

[A similar case occurred two years since at a boarding-school in Philadelphia, and created considerable excitement in the neighbourhood.]

29. *Report regarding the Cases of Hydrophobia which occurred in France during the year 1852*. By M. AMBROISE TARDIEU. (In the year 1850, the minister of agriculture and commerce, on the recommendation of the committee of public health, sent a circular to every prefect in France, requesting him to give information regarding any cases of hydrophobia which might occur in his department. A number of reports were in consequence sent in, but as these were in some respects incomplete, a fresh circular was issued detailing more particularly the manner in which the cases should be recorded. From the information so obtained, M. Tardieu drew up a report regarding the cases which occurred in the years 1850–51, as well as in 1852. As the report for the year 1852 is much more complete than the others, we subjoin an abstract of it):—

1. The number of cases of hydrophobia which occurred in France during the year 1852 was 48. These were observed in 14 departments: the department in which the greatest number occurred was that of the Hautes Alpes (in the southeast of France, latitude between 44° and 45°); while the department of Lozère (also in the south, and having the same latitude as the other) came next.

2. With regard to the sex; 36 of the 48 cases were males, 12 females; the proportion in the two preceding years was almost the same.

3. The following table exhibits the ages of the subjects affected with hydrophobia:—

Below 5 years, in 1852,	3	in two former years,	4 = 7
From 5 to 15	16	" "	14 = 30
" 15 to 20	4	" "	11 = 15
" 20 to 30	3	" "	9 = 12
" 30 to 60	17	" "	37 = 54
" 60 to 70	1	" "	7 = 8
Above 70	0	" "	6 = 6
Not mentioned	4	" "	0 = 4
	<hr/> 48		<hr/> 88 = 136

This table shows the incorrectness of the opinion which ascribes the disease to the effects of terror, for it shows that 7 children under five years of age have been attacked.

4. All the cases which occurred in 1852 originated in the bites of dogs, except one, where the bite of a cat was the cause of the disease.

5. The situation of the wounds inflicted by the rabid animals was as follows in 48 cases: On the face 13 times; on the upper extremities 15; on the lower extremities 12; not mentioned 8. In two of the cases the disease was communicated by pet-dogs which were accustomed to lick their masters' faces, and where excoriated lips were the seat of inoculation.

6. In 40 out of the 48 cases, the date of the inoculation has been observed. It occurred in March, April, and May in 10 cases; in June, July, and August 16; in September, October, and November 4; in December, January, and February 10.

7. It seems a considerable number of individuals who are bitten by rabid animals escape the disease. During 1852, some observations were made on this point, and it appeared that out of 44 persons bitten about the same time 23 only were attacked.

8. The period of incubation of the disease was exactly noted in 20 cases. It was as follows: Less than a month in 8 cases; from 1 to 3 months 10; from 3 to 6 months 1; 11 months 1.

9. The duration of the disease in 20 cases was 2 days in 6 cases; 3 days in 8; 4 days in 5; 6 days in 1.

10. The termination of *confirmed* cases of hydrophobia was constantly fatal. Of the 48 cases, it appears that only 27 came under this category, in the others the effect was merely local. In 12 of these 27 cases no precaution was taken, in 4 no mention is made of this circumstance. In 8 of the remaining 11 cases cauterization was resorted to immediately; in 3 at a late period. Of the 21 individuals who escaped (see ¶ 7) cauterization was energetically performed in 12 cases; the details of the other 9 have been omitted.

11. As to the mode of cauterization employed, the actual cautery was used in all the cases but 5, and these were treated by protonitrate of mercury, nitric acid, ammonia, or butter of antimony. In Germany, it has been proposed to excise the bitten parts, and then to wash the wounds with a solution of caustic potash.—*Monthly Journ. Med. Sci.* May, 1854, from *Annales d'Hygiène, &c.* January, 1854.

30. *Hydrophobia in a Cretin—Remarkable Development of the Intelligence during the Paroxysms of the Disease.*—The following curious case is related by M. NIEPCE. A. Chauvet, a cretin from birth, at the age of 17 years presented, in a marked degree, all the physical and mental characters of cretinism. He could only articulate a few words imperfectly. He had not sufficient intelligence to learn reading or writing, nor to understand the catechism. His affections were little developed: he had some liking for his mother, but showed none for his brother. On the 10th May last, he was bitten by a mad dog; the wound was slightly cauterized with some drops of ammonia by a druggist, about an hour after the accident. Nothing was observed till the 27th July following, about eleven o'clock, when Chauvet refused to eat or drink; and two hours afterwards all the symptoms of hydrophobia made their appearance. From the commencement of this disease, to the great astonishment of every one, Chauvet spoke with much greater facility than he had ever done before, addressing those around him, and relating the sufferings which he felt. In the intervals of the paroxysms, he called his mother and brother, showing his affection for them by the most tender caresses, and entreating them not to leave him alone. He caused the priest to be sent for, and on his arrival expressed with tears his bitter regret that he had never been able to learn the catechism. During the remainder of his illness, his intelligence became always lucid during the paroxysms of suffering, when he would put questions to those around him and give directions to them; but as soon as calm or depression ensued, the natural state of his intellect returned. On the 1st August, acute delirium came on, during which he spoke frequently and with volubility, citing

facts which had happened several years before, and to which he had never seemed to pay attention. The delirium lasted till night, when it was succeeded by deep coma. He died at five o'clock on the following morning.—*Monthly Journ. Med. Sci. from Gaz. des Hôp.* Aug. 27.

31. *Hydrophobia Five Years after the Bite—Death—Autopsy.*—[Though we confess our incredulity of the possibility of the incubation of hydrophobia being prolonged for five years, we copy the following case, as it is a curious one, and it presents a fair view of the opinions of various authorities in regard to that point.]

The feature of greatest interest in the following case is the unusually long interval which elapsed between the infliction of the dog's bite and the outbreak of the symptoms. Before entering upon it, therefore, it may be well to glance at the facts and opinions previously recorded in respect to the possible length of incubation in the disease in question. Although, probably, the views entertained by the bulk of the Profession tend strongly in an opposite direction, yet there is on record a very large body of evidence in favour of the opinion that the incubation stage of hydrophobia may be prolonged over a series not only of months, but of many years—in fact, indefinitely. Romberg, in his work on "Diseases of the Nervous System," says: "An analysis of sixty authentic observations has shown that the shortest interval between the introduction of the poison and the appearance of the disease is fifteen days, the longest from seven to nine months, and that the average period is from four to seven weeks." Out of thirteen persons who were bitten in one day, by a mad wolf, and whose cases are described by Trollet, the disease appeared in 1 between the 13th and 30th day, in 4 between the 30th and 40th, in two between the 40th and 53d, and in 1 three months and a half after the bite. Lenhossek mentions two patients who were attacked respectively 6 and 9 months after the infection. Dr. Meade mentions an interval of 11 months, Bossiere of a year, and Nourse of 19 months. Dr. Hamilton considers 19 months, and Dr. Hunter 17 months, to be the longest intervals worthy of credit. Dr. Levin, in his "History of Diseases Transmissible from Domestic Animals to the Human Race," states, that the incubation period of hydrophobia may be only a few hours, or may be extended to many years. Dr. J. Hassinger has recorded a fatal case in the *Transactions of the Vienna Medical Association*, in which the disease appeared two years after the bite. A very carefully sifted case has been published by Dr. Bardsley, of Manchester, in which it seemed certain that no less a space than twelve years had passed since the accident. In many of these almost incredible cases, it appears certain that the affection possessed all the symptoms of hydrophobia; and as it is admitted by almost universal consent, that the disease never occurs spontaneously in the human subject, there is but one other source of fallacy to which we can turn. That source is the possibility of a more recent infection having occurred than the one mentioned by the patient. A severe dog-bite, it is allowed, is an occurrence not very soon forgotten; but then it must be borne in mind that a bite is not necessary to the production of the disease, and that a mere lick on an abraded surface will amply suffice. Mr. Lawrence mentions the following: "The Hon. Mrs. Duff had a French poodle, of which she was very fond, and which she was in the habit of allowing to lick her face. She had a small pimple on her chin, of which she had rubbed off the top, and, allowing the dog to indulge in his usual caresses, he licked this pimple, of which the surface was exposed, and thus she acquired hydrophobia, of which she died." Here, then, is a source of contagion to which most individuals are more or less unconsciously exposed, and it is very possible that some of the supposed cases of extraordinarily delayed outbreak may be thus explained. There is one, however, which cannot be so dismissed, and which occurred under the exact circumstances requisite to afford an almost incontestable proof of the phenomenon alluded to. It is that published by Mr. Hale Thompson, in Vol. I. of the *Lancet*. The subject of it, a lad, aged 18, had been twenty-five months in close confinement in prison, and during that time had never been exposed to the bite of any animal. He had been bitten severely by a dog seven years before in the right hip, and the scar still remained. During the whole

period he was under observation, he was sullen, gloomy, and reserved, and was never known to look the person in the face to whom he spoke. Death occurred after a three days' illness, during which "the most decided symptoms of hydrophobia were manifested." In this case, as in the one we are about to record, great pain was complained of at the seat of injury. Observations such as the above must, we think, be held to make it extremely probable that the period of incubation in hydrophobia is quite an indefinite one, and that, in the following case, the history given might be fairly accepted as explanatory of the occurrence of the disease.

Thomas Spink, a very muscular young man, aged 19, was admitted into Guy's Hospital, under the care of Dr. Hughes, on the evening of Monday, May 15. A message had been sent previously, by the Surgeon in attendance, requesting that some one would come to his house, in order to administer chloroform, previous to his removal to the Hospital, as the spasms were so violent that he thought great difficulty would otherwise attend the attempt. A dresser accordingly went to the patient's house for that purpose; but, finding that the chloroform seemed only to excite him, its exhibition was desisted from. During the journey, which was only a short one, every breath of air appeared to excite the most violent spasm about the throat. On arriving at the Hospital, at half-past nine in the evening, the man was seen by Mr. Stocker, the Resident Medical Officer, and subsequently by Dr. Hughes and Dr. Gull. The latter gentleman took a great interest in the case, and stayed with the patient during most of the time he was under care; and in compiling the following, we had the benefit of some remarks upon it made by him on the following day. At first sight there was a wildness about the patient's expression, and an appearance of terror and alarm, which those who had ever before seen a case of hydrophobia could not mistake. The least breath of air threw him into a violent paroxysm of spasm, which appeared mainly to affect the pharynx, but in which the head was also thrown back, and there seemed some tendency to opisthotonos. His conversation was generally wild and incoherent; but he could, when more quiet, answer questions, and stated his age and name, but, as it was afterwards found, not quite correctly. He complained of much pain in the left leg, and immediately below the knee, on that side, was the scar of an old bite. There was no lockjaw whatever; but, although he appeared very thirsty, and anxiously attempted to drink, yet he could not swallow a drop of fluid, violent spasm being immediately produced by the attempt. There was much adhesive and frothy saliva about his mouth, which he was constantly engaged in the attempt to spit out. Gasping eructations frequently occurred, but there was no actual vomiting. He was in the greatest alarm, and appeared from his expressions to be in fear that it was intended to murder him. Respiration was constantly attended by sighing efforts, and almost by shuddering; his condition in this respect much reminding the bystanders of that induced by a sudden plunge up to the neck in cold water. At first, it was not thought necessary to confine him, but afterwards it became needful to do so, as on one occasion, watching his opportunity, he had thrown himself out of bed, and sprung violently against a window which was near. After this, his arms were tied down to the sides of the bed. His pulse was rapid, and subject to sudden alterations in frequency, varying in the course of a few minutes from 100 to 130; the skin was warm and moist, and the pupils widely dilated.

The remedy first tried was the Indian hemp, the extract of which (obtained fresh from Squires's), in the enormous dose of ten grains, was exhibited by injection into the rectum. The first quantity was almost immediately expelled, but the second was retained. It appeared, however, to exert not the slightest effect in controlling the tendency to spasm. The paroxysms continued to recur almost constantly, and were induced by the slightest causes. At about two o'clock A. M. it was decided to exhibit chloroform. The man resisted the attempts to make him inhale it, tossed his head about (he was strapped down), and even made an effort to bite the hand of the administrator. The first effect was to excite and render the paroxysms much more violent and constant; as insensibility was induced, however, the tendency to spasm subsided, and ultimately the patient lay quite quiet. It was noticed that his pupils, which pre-

viously had been widely dilated, contracted when under the influence of the chloroform. The inhaler being removed, it was found that the effect of the anæsthetic was never prolonged more than a few minutes at a time, after which the spasms again began to occur. With short intermissions, the patient was kept under the influence of the remedy for more than an hour, when, on account of the extreme collapse, it was deemed necessary to suspend it. The pulse was now not perceptible at the wrist, and the surface was cool and clammy. After the chloroform was laid aside, some returns of spasm took place, but not violently; the pulse never became perceptible, and the patient, after gradually sinking, died about a quarter to four A. M. A few hours previous to death, there had been noticed some emphysema of the cellular tissue in the root of the neck, caused, no doubt, by rupture of the trachea or some part of the larger bronchial tubes during spasmodic closure of the glottis.

Of the numerous observers who witnessed the symptoms just described, there were none, we believe, who felt any doubt as to the name by which the disease should be designated. As distinguishing it from mania, there were the attacks of spasm, the great susceptibility of the external surface to impressions, and the incapability of swallowing fluids. The delirium was one of terror and extreme fear, very distinct from the fierce and violent raving usually present in phrenitis. From tetanus, its difference was not less well marked. There was neither trismus, rigidity of the abdomen, or tonic spasm of muscles, nor did the paroxysmal attacks involve the muscles of the extremities. In tetanus, the intellect is almost invariably clear to the end; here, it was clouded by delusions of a peculiar and pathognomonic character. The *post-mortem* examination was performed on the following morning by Dr. Habershon, and was productive of little more than negative results. Having briefly specified them, we will next give the history of the case previous to admission into the hospital. As much of the interest of the narrative depends upon the degree of confidence which may be placed in the history, we may state that its facts were ascertained by Dr. Gull from the father of the patient by a very careful cross-examination, and that they obtained the full credence of that physician.

Autopsy, twelve hours after death.—There was great *post-mortem* rigidity, all the muscles being hard. With the exception of the hands, which were rigidly flexed, there was no distortion of any of the extremities. On opening the head, the veins or the meninges were found full of blood, and the brain substance itself presented a little more of vascularity than is seen in the average of examinations. The whole of the pharynx was deeply congested, and of a slight bluish tinge; the boundary of the congestion was definite, and terminated at the commencement of the œsophagus. The tonsils were of usual size. The left side of the heart was firmly contracted, and the blood was universally fluid. The posterior parts of the lungs were loaded with blood, and some parts presented the characters of incipient lobular pneumonia. There was interlobular emphysema about the root of the left lung. The cord was taken out, but, with the exception of some very questionable softening of a part in the middle dorsal region, it presented nothing morbid. Beyond general congestion of the abdominal and thoracic viscera, distension of the intestines with gas, and a dry condition of the peritoneal surface, no other departures from the healthy condition were observed. The surface of the corpse was carefully examined for any traces of recent wounds or abrasions, but none were discovered.

Previous history of the case.—The father of the young man was by trade a bricklayer, and his son had worked partly with him and partly as a "tumbler" at fairs. Five or six years ago, the boy, then aged about 13, had been severely bitten in the left leg by a strange pointer bitch. The dog had puppies at the time, and the occurrence took place in the road.¹ The father of the boy witnessed the occurrence, and remembered it well, because he had afterwards to dress the injured part. Nothing whatever was known as to the history of the

¹ As no history of the dog could be got at in this case, it may be worth noticing, that during the few weeks immediately subsequent to parturition, dogs are, according to the observations of Youatt, peculiarly liable to madness.

dog. After the wound had healed, the boy never again complained of it, and he manifested no alteration in temper or manner. He was usually of quiet habits, and not much addicted to drink. He had always lived at home, and the father felt certain he should have known if any bite, wound, or other injury had occurred to him since the one mentioned. He had enjoyed excellent health, in every respect, until Friday, the third day previous to his admission. On the afternoon of that day (he had been staying at home, being out of work,) he went to sleep, and woke up in the evening, appearing disturbed, and stating that he had a very bad dream. He afterwards took some tea, and then went out for awhile. At night, he complained of a severe headache. On Saturday, he still complained of not being well, but, in the afternoon, walked with his father a distance of some miles, to "tumble" at a neighbouring village. Late in the evening, he commenced the practice of his evolutions, but, after having stood once on his head, complained that it had hurt him so that he must give it up. During the walk home, his father noticed that his limbs seemed weak, and the journey took them many hours. Throughout Sunday he stayed in the house, still complaining of "splitting headache," and appearing very ill. He could, however, swallow fairly, and had no noticeable difficulty in doing so. Late in the evening he got up, and went alone to a neighbouring public-house to get some beer, which he drank, but, as the landlady who served him afterwards stated, with the greatest difficulty, as "it seemed as if it would have choked him." On Monday morning he said he could not drink, and took only a little sopped biscuit, which he appeared scarcely able to get down. In the afternoon of that day the attacks of spasm began to occur. He exhibited the greatest terror of some impending mischief, although perfectly conscious. A medical man who had been sent for entering the room, he begged him not to approach, as he could not, he said, help striking him if he did. These symptoms becoming rapidly aggravated, he was, as we have already seen, conveyed to the hospital late in the evening.

Having already entered so fully on the incubation question, we have not much to add in the shape of comment on the above. The premonitory symptoms of the outbreak, the duration of the attack, and the nature of the illness, presented nothing different from the ordinary course of hydrophobia. It is worthy of note, that the lad had manifested no form of nervous disease whatever during the long interval subsequent to the bite. The fatal attack could not in any way be attributed to fear or apprehension, since he did not appear to have in the least anticipated any ill consequences from his accident. The effect of the chloroform inhalation would seem to have been almost precisely similar to that which we have noted in many of the more acute cases of tetanus, which have been mentioned in the series we are at present engaged on. The drug was efficient in allaying the morbid irritability, and in completely controlling the tendency to spasm, but exercised no preventive influence. Its effects rapidly passed off when inhalation was relinquished, and the vital powers of the patient failed as fast as they could have been expected to do had it not been used.—*Med. Times and Gaz.* May 27, 1854.

32. *Morbid Changes in the Mucous Membrane of the Stomach.*—Dr. HANFIELD JONES read a paper on this subject before the Royal Medical and Chirurgical Society, May 23, 1854. The first part of this communication comprised a description more particularly of the minute glandular structure of the mucous membrane of the stomach, in which the author corroborated the account given by Kölliker. On first commencing his researches into this subject, he was not aware that lenticular or solitary glands had been seen in the mucous membrane of this viscus. The author, not imagining they could be normal structures, had at first viewed them as simply nuclear deposits, supposing they were of new formation. Kölliker had observed that these lenticular glands did not constantly occur in the stomach of adults, even though they might be possibly always present in those of children. In very many cases he had met with no traces of them; in others they were seen to be extremely numerous, covering the whole surface of the stomach; yet the thought could hardly be excluded, that the morbid conditions of the part, which were always present, had not had

something to do with their formation. The author thought it difficult to fix any exact limit to the healthy development of these glands. He considered the gastric tissue in its most normal and efficient state when there were but few of these glands or nuclear masses, and when those that existed did not encroach materially upon the tubular or gastric glands of the stomach. He thought great individual varieties might exist; that they were naturally larger and more numerous in some individuals than in others. He ventured to think that these solitary glands, and their groups in the intestines (Peyer's patches), had really no use, and fulfilled no function in the human body, but existed in a rudimentary state, in obedience to the law of unity of type. They might be regarded as portions of undeveloped embryo substance, existing in inverse ratio to the surrounding specially organized tissues, and with this view their simple nuclear structure, so common in embryonic parts, was very accordant. The author thought that the epithelial contents of the tubular glands were thrown off during digestion, and formed an important constituent of the gastric juice, probably the so-called pepsin. The evidence of this rested on examination of the stomachs of animals killed while digestion was proceeding, and of a man who died suddenly soon after a meal. The following deviations from the typically healthy condition of the stomach were mentioned as examples of morbid changes:—

1. *Nuclear masses.* It was doubtful what degree of development of these was to be considered as surpassing the physiological limit; but observation proved that they became both hypertrophied and atrophied, and the latter seemed to take place by a kind of liquefying, so that a cavity was formed containing a clear fluid and some nuclear corpuscles.

2. *Diffused nuclear formation.* The effect of this was, that the tubes became more or less atrophied and obscured by interstitial deposit.

3. *Intertubular fibroid formation.* The tubes became atrophied by the presence of a fibroid or granular deposit, in which some altered vestiges of the tubes might be brought into view by acetic acid.

4. *The tubes appeared to decay spontaneously,* but not from the presence of new fibroid tissue.

5. *Black pigmentary deposit,* occasionally within the tubes, more often between them; sometimes yellow pigment was found; both might be regarded as altered hæmatine.

6. *Cystic formation,* produced in one of three ways: First, a nuclear mass liquefied and left a cavity; secondly, while atrophy of the tubular glands was going on, a portion of one became distended; thirdly, a cyst was produced as a large vesicle, a true new formation.

7. *Mammillation,* usually affecting the pyloric region.

8. *Gathering up of the lower parts of the tubes,* so as to form a group of convolutions like the acini of a conglomerate gland.

9. *Unhealthy condition of the epithelium of the tubes,* occasionally exhibiting the characters of a fatty degeneration.

10. *Self-digestion* was of frequent occurrence, and mostly confined to the splenic region; the mucous membrane was more or less deeply coloured, thinned, smooth, and semi-translucent. In extreme cases the nerves and vessels were seen altered, as when treated by strong acetic acid.

11. *Small dark-red, circumscribed spots,* manifestly the result of hemorrhage; ulceration often took place in these.

12. *The tenacious adhesive mucus of gastric catarrh.* Its microscopical characters were very clearly described.

The author had observed torulæ in the mucus of the stomach of a diabetic patient. The paper was accompanied by a table of one hundred cases of *post-mortem* examinations, in which the morbid changes in the mucous membrane of the stomach were fully and minutely described, together with an analysis of these cases, in which the influence of age and sex, habit of life, &c., were considered, as well as the frequency of the several morbid changes already enumerated. Eight drawings, executed by the author, illustrated very intelligibly the diseased conditions which the microscope had revealed.—*Med. Times and Gaz.* June 3, 1854.

33. *Waxy Degeneration*.—This peculiar change in organs has been recently made the subject of discussion in the Physiological Society of Edinburgh. It is observed in the spleen, the kidneys, and liver, and indicates an important constitutional cachexy. The organ affected with it is firm, its section is smooth, and its feel hard like wax.

In the *liver*, studied microscopically, according to Dr. W. T. GAIRDNER, there is a change in the entire structure, but chiefly in the glandular epithelia, which are compressed, irregular in form, and have atrophied nuclei. They are also altered in a manner which can hardly be described but easily recognized after being once seen, presenting a peculiar *horny* refraction, and giving the idea of a substance much denser and more resistant than the normal elements of which these cells are composed. Sometimes the cells cohere in masses, sometimes they are very small, at others as large as the ordinary cells.

In certain forms of Bright's disease of the *kidney*, a precisely similar alteration takes place in the histological elements of that organ; but here, the vessels, and particularly the arteries and the Malpighian tufts, are the first portions distinctly affected.

In the *spleen*, on the other hand, the most marked change is in the contents of the sacculi. This change in the albuminous tissues is not always confined to the original structures of the body: old deposits in serous membranes and tubercle often exhibit it, and even cancer sometimes undergoes this change, when the cancer cells cannot be distinguished from those of the liver, except by their lighter colour.

In the spleen, according to Dr. Sanders, under a power of 250 diameters, the nature of the waxy degeneration can be distinctly seen in their sections. The Malpighian bodies, forming rounded transparent granulations, appear as clear spaces surrounded by the red pulp. The normal corpuscles of these bodies are found converted into, and replaced by masses of a colourless, dense, highly translucent, homogeneous material. At first, these masses appear to exhibit no structure of any kind; but, on careful examination, the outlines of irregular cells may be discerned.

This "waxy degeneration" is a pathological change of great importance. It is not now observed or noticed for the first time, but is well worthy of a more careful study than has hitherto been bestowed on it.—*Assoc. Med. Journ.* March 24, 1854.

34. *Metastasis, in its Practical Bearings*.—Mr. HUNT read a paper on this subject before the Medical Society of London (April 22, 1854), the object of which was to inquire in what manner, and to what extent, the knowledge of such a principle or contingency should modify or restrain the practice of the physician and the surgeon. He (Mr. Hunt) stated that metastasis did not appear to be well understood, and that a doctrine had been deduced from it which appeared to have no rational foundation. Authors of repute had described certain local diseases which were known to be subject to metastatic changes as being salutary or protective to the system, so that they could not be cured without danger to more important organs. The diseases thus described as "salutary" had no pathological character peculiar to themselves, but it happened that they were all, to a certain extent, difficult of cure. They were—gout, asthma, hæmorrhoids, fistula, ulcers of the legs, otorrhœa, and certain chronic eruptions. It had been argued that, as we were able to relieve internal congestion by external counter-irritation or artificial discharges, so the suppression of spontaneous morbid irritation or discharge was liable to induce disease of the viscera. But this opinion was contrary to physiological principles, and it was not supported by facts. Still, in certain patients of weak constitution and peculiar idiosyncrasy, it did occasionally happen that disease of the surface suddenly receded, and the head, heart, lungs, or some other organ, became affected; and the question for discussion was, whether, in cases where there was reason to apprehend metastatic action, it was best to leave the disease alone, or whether there was any safe mode of treating it. Where metastasis had resulted as an apparent consequence of medical or surgical treatment, it might be suspected that the treatment was improper. Thus, the immersion of a gouty foot in cold

water had been followed by hemiplegia. Such metastasis probably would not have occurred had the attack of gout been treated constitutionally; and this principle would apply in all cases. Local diseases, and especially the eruptive, should be treated by careful attention to the state of the general health. Even in local diseases of local origin, the health might become secondarily affected, and it might then be unsafe to rely on local remedies. It had rarely occurred to him (Mr. Hunt) to meet with a case of skin disease, or ulcer, or fistula, in which the general health had improved under the progress of the local disease. The reverse very commonly occurred: patients suffered in their general health in consequence of local irritation, and recovered when the source of that irritation was removed. Still, in all cases, it was better to treat local disease by internal remedies, of which evacuants should form a chief part. Under this practice, he (Mr. Hunt) has never once seen the recession of eruptions or the healing of ulcers followed by any consequences of a serious nature. On the contrary, if the health had been previously in any degree deranged, it had generally been found much improved by the treatment. The author observed, however, that in those rare cases in which a strong disposition to metastasis existed, there was some peculiarity of constitution in the patient which resisted the ordinary influence of medicine. These cases, being rare, often found their way into print, and therefore became apparently more numerous than they really were. But their practice must be guided, not by the exception, but by the million of cases which follow the general rule; and we might greatly question the existence of any "salutary" disease, which, as a safeguard to health, demanded a moment's consideration at their hands.—*Lancet*, April 29, 1854.

35. *Fatal Effects of imperfect Union of the Umbilical Vein after Childbirth; Jaundice as the Result of the imperfect closure of the Ductus Venosus.*—Mr. HENRY LEE read to the Physiological Society March 18, 1854, the particulars of two cases, one under the care of Mr. Willing, of Hampstead: the other under the care of Dr. Herapath, of Bristol. In the first, jaundice was the most prominent symptom, and commenced immediately after birth. Six days after the separation of the funis (in which process there was nothing abnormal), slight hemorrhage made its appearance. The blood passed was of a thin serous character, scarcely discolouring the linen upon which it was received, and, when dry, not stiffening it. The bleeding continued for two days in spite of every attempt to avert it. It then stopped after the umbilicus had been plugged by cobweb. The little patient, however, shortly afterwards sank. On a *post-mortem* examination, the umbilical vein, one umbilical artery, the ductus venosus, the ductus arteriosus, and the foramen ovale, were all found open. No coagula were found in any part of the body, and the blood discharged from the vessels, both before and after death, had evidently lost its power of coagulation. It appeared entirely deficient in fibrin. There was no appearance of inflammation of the lining membrane of the umbilical vein, although the structures around, near the umbilicus, were somewhat condensed and thickened. The jaundice in this case was attributed by Mr. Lee to the blood from the vena porta having passed through the ductus venosus instead of circulating through the liver. When the natural circulation through the ductus venosus at childbirth ceases, if that vessel remains open, it affords a ready passage to the blood from the portal system, and thus the blood from which the bile should be secreted may never get to the liver at all. Under such circumstances, the bile would necessarily accumulate in the system. Instances have been related in which the secretion of bile went on perfectly well where there was no vena porta; but there were cases in which the hepatic artery was very much larger than natural, and supplied the amount of blood required for secretion as well as for nutrition. In Dr. Herapath's case, some blood escaped from the umbilicus at the time the penis separated, and shortly after birth the child became jaundiced. It was subsequently attacked with erysipelas, and secondary deposits formed in various parts, but not in the lungs. This was accounted for by Dr. Herapath, from the circumstance, discovered on a *post-mortem* examination, that the foramen ovale had remained open. It was therefore thought that the vitiated stream of blood from the lower cava had

been directed, as in the foetal circulation, by the Eustachian valve, through the foramen ovale to the left side of the heart, and thence to the system generally; whereas the blood from the superior cava, which had not been directly contaminated, alone passed into the right ventricle, and was thence sent to the lungs. In this case the umbilical vein was found filled with curdy, puriform fluid, and afforded a strong contrast to the first case, in which the vein contained only a very delicate filamentous coagulum, and presented within no other sign of disease. The differences observable in the two instances in this respect supported, as Mr. Lee believed, the doctrine which he had now for some years advocated—namely, that the puriform fluid found in the vein in such cases was formed by the softening of the fibrin and of the coagula which the vessels contain; and that where, from whatever cause, no such deposit of fibrin or coagulation of the blood occurs, there no puriform fluid would be found in the vein, and none of the ordinary signs of inflammation of the lining membrane of the vein would be present.

36. *Portion of Intestine discharged after Intussusception.*—Dr. PATERSON, of Leith, exhibited to the Medico-Chirurgical Society of Edinburgh a portion of intestine, still invaginated, about ten inches long, which had been passed by a patient of his twelve days after the occurrence of the symptoms of ilius.

The patient, a girl, nine years old, made a perfect recovery. The disease began after an attack of dysentery. Much vomiting and great prostration of strength occurred at first; on the fifth day, a large quantity of bloody serum was passed by stool, and without any feculent matter; on the seventh day, a small quantity of feculent matter was passed, and occasionally till the twelfth, when a perfect relief occurred to all the symptoms on the passage of the large portion of intestine shown to the Society. The treatment which was pursued, was the application of morphia to the blistered surface of the abdomen, and opium by enemata, as recommended by Dr. Evans, of Carlisle. (See *Monthly Journal* for January, 1854.)

Dr. Simpson said, that he had seen the case along with Dr. Paterson, in which there was immense distension of the belly; indeed, from the great enlargement of the cutaneous veins, the vena cava appeared to be obliterated by the pressure. Dr. W. Thomson, in his collection of cases (see *Edinburgh Medical and Surgical Journal*, vol. xlv. p. 102), had only given two instances of the disease in patients so young, and none in which a double invagination had taken place, as, on examination of the preparation, appeared to have been the case in the present instance.—*Monthly Journal of Medical Science*, March, 1854.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

37. *Phlebitis, especially in connection with the deligation of Veins.*—Mr. WEEDEN COOKE, in a paper read before the Harveian Society, began by referring to the influence of fashion in both medicine and surgery, and stated that “many of the novelties suggested as means of relief, fade into deserved oblivion; others gain a temporary notoriety, and swim upon the stream for a time; whilst a few remain as lasting and glorious additions to our established means of thwarting the influences of accident and decay in the human frame.” The revived and improved practice of vein-tying seemed to require consideration as to the notoriety of receiving it into the legitimate modes of treatment, and in order to bring the former and the present methods of performing this operation into juxtaposition, the author recited the experiences of Sir Everard Home, Sir Astley Cooper, and Sir Benjamin Brodie, describing the incision plan of the former surgeon, and the subcutaneous division of the vein, performed by the last of these eminent men; and the ill success attending these operations, so much so that they were repudiated and abandoned by

all surgeons at that time, from the frequency of the alarming symptoms of phlebitis and depositions of pus in various parts of the body. Sir A. Cooper was so impressed by the fatality of his own cases that he declared to his class "he would rather have his femoral artery tied than his great saphena." It was also the opinion of this great surgeon that when the operation does not prove fatal, its ultimate effects are perfectly nugatory, and this opinion is maintained to the present day by Sir B. Brodie, who, in a recent conversation with the author, stated that if one vein be obliterated, another will take on the varicose action, and so after a short time the evils for which this dangerous proceeding is adopted, are renewed. The author then passed in review the various writers upon the pathological causes of phlebitis, as occurring traumatically or idiopathically, and gave especial praise to the elaborate paper on this subject, by Mr. Arnott, in the fifteenth volume of *Medico-Chirurgical Transactions*, and to the investigations of a more recent date of Mr. Henry Lee. "Although the deligation and division of veins was abandoned, phlebitis was still witnessed, arising from various causes. Venesection, which at that time was called the sheet anchor in all inflammatory affections, was the most prolific means of providing subjects for inquiry. The secondary effects of amputation, and the excision of tumours; the purulent deposits in the viscera, after injuries to the head, as well as the phlegmasia dolens of parturient women, all yielded evidence of the dangerous character of this disease, and the tendency to diffuse itself over the whole system. The author then referred to ten fatal cases of phlebitis after venesection, reported by Mr. Arnott, and mentioned one at present under his own care in the Royal Free Hospital, where obliteration above and below the incision took place, and a deposition of pus over the wrist, attended by adynamic fever, was the result: this patient recovered. Having referred to the recorded cases of phlebitis, from tying patulous veins during operation, the author mentioned one that had occurred to himself. The patient was an old man of sixty-eight, suffering from an extensive epithelial cancer of the hand and wrist, for which amputation was performed: a vein bled very freely, and loss of blood being most undesirable, from the low condition of the patient, Mr. Cooke put a ligature upon it. Four days after, the vein became highly inflamed, and a certain amount of sloughing of the stump ensued. On the eleventh day the calf of the right leg became inflamed, and the inflammation extended up the saphena, into the thigh. Constant fomentation kept the inflammation in abeyance, and no points of suppuration were formed, although the leg remained œdematous for some time. He was, however, discharged, with the stump quite healed, leg free from inflammation, and able to walk tolerably well, in five weeks, and has since remained well. This case occurred a year and a half ago. The author suggested that the actual cautery should be used for staying venous hemorrhage in such cases, rather than the ligature. Passing on to the consideration of the more modern practice of the use of potassa fusa and the Vienna paste, in obliterating varicose veins, Mayo's cases were referred to, and Mr. Arnott had stated to the author his experience of this proceeding, which went to show that if it be used only to produce a coagulum in the vein, it acts beneficially; but that if the slough extends to the vein itself, the products of this action will most probably get into the circulation, and alarming secondary deposits will be the result. The difficulty of controlling the action of these caustics within exact bounds must limit their use very much, for who can be sure of effecting this? The still more recent and most approved mode of obliterating varicose veins is by the needle and twisted suture of M. Davat; but as yet no convincing set of cases has been published showing its immunity from the dangerous consequences seen to proceed from other operations on the veins, but, although not published, a great number of cases have been so treated, and Mr. Ferguson, in a letter to the author, states that he has employed the needle and twisted suture for the last fifteen or eighteen years, for the purpose of obliterating varicose veins, without any bad results. "Our excellent president, Mr. Coulson, is in the habit of using this means of obliterating varicose veins frequently, and even allows his patients to walk from his house to their houses without ill effects." Mr. Arnott also informed the author that he occasionally

employed this mode of treatment, but always with the precaution that the thread did not pierce the coats of the vein, but only remained sufficiently long to obtain a firm coagulum. With this wise proviso the operation may become an innocuous one, but, as it is customary to allow suppuration of the coats of veins, cases were not few in which phlebitis of a most alarming character has been set up. One of these occurred whilst the author was house-surgeon at the Royal Free Hospital, amongst Mr. Gay's patients. The needles and twisted suture were applied to the saphena at several points, phlebitis came on in the leg and thigh, subsequently there was deposition of pus in the shoulder-joint, and after that the lungs were attacked, and he spat fetid pus; but by good living and good treatment he pulled through, though certainly with much difficulty, and the effects remained for a long time. The responsibility of the surgeon who runs these probable risks for effecting a purpose which may be obtained by rest and constitutional treatment is tremendous; and, even though forty-nine cases out of fifty escape evil results, we are not justified in placing the fiftieth in so perilous a position. The author then referred to Mr. Henry Lee's mode of tying and dividing the vein, and thought this more complicated procedure as much, if not more, objectionable than the needle and suture, and quoted from *The Lancet* a case in which much secondary mischief had resulted from this operation. Having shown the occasional unhappy consequences of obliterating veins by operations, it remained to indicate a few of the means of relief, both local and general, which would obviate the necessity for these measures. Continued rest in the recumbent position, with an occasional batch of leeches over the inner condyle of the femur, are the prime requisites. Blue pill and dandelion, with a brisk purge now and then, are the medicines of most service; and Bourjeaud's beautiful elastic contrivances, with or without a strip of soft leather, spread with soap and adhesive plaster, to support any particular bunches of veins, afford all the requisite local treatment to obviate the possibility of sudden rupture, and remove pain. The constitutional symptoms and treatment of phlebitis were further illustrated by two cases occurring in the author's practice. In the first, a young gentleman from Ceylon, it arose from getting wet, and required highly stimulating treatment with bark, quinia, chlorate of potash, and opium, from the commencement of the attack. In the other case, that of a young robust surgeon, it was developed by sitting on damp grass after a hard day's shooting, and required in the first instance antiphlogistic treatment; but subsequently, when the pus had formed, it was necessary to stimulate largely, and give the same medicines as in the first case. Much benefit was attributed to the chlorate of potash, given in fifteen-grain doses, three or four times a day. Seeing the occasional danger attendant on any operation for obliterating veins, and the certain return of the diseased condition, the author considered that, with our local and constitutional remedies at hand, we were not justified in exposing our patients to this risk, and expected to see vein-tying a second time fall into disrepute.—*Lancet*, May 27, 1854.

38. *Popliteal Aneurism; long-continued compression—eventual solidification of the Sac.*—Thomas A—, aged twenty-eight years, was admitted into Guy's Hospital, 31st Aug. 1853, under the care of Mr. Poland, who was officiating for Mr. Hilton.

Description and History.—The patient is a strong muscular man, with dark hair and eyes. From the age of twelve years till twenty-two, he was engaged as a smith, since then he has been employed as a carman. The man has had a good deal of wrestling, and was reckoned very dexterous; his health has been good, the only illness he remembers being an attack of scarlatina followed by dropsy, when he was about nine or ten years old. About two months before admission, on getting into bed one night, the patient felt a sudden pain of a pricking and darting description in the right ankle, which continued for some time, and was very severe. He looked upon this attack as one of gout, and fell asleep; but the next morning was again annoyed by returning pain, which, however, soon afterwards passed away. From this time the uneasiness frequently recurred, and gradually crept upwards, involving the calf of the

leg, and ultimately settling in the popliteal space. The pain would generally come on when the patient was at rest, and moving about, affording him relief. It went on increasing, and prevented him from sleeping, and he found no alleviation but in walking about.

A practitioner mistook the swelling in the popliteal space for a chronic abscess, and advised the application of tincture of iodine. This caused much distress, and the patient eventually applied for admission at this hospital.

State on first Examination.—On applying the fingers to the popliteal space, a swelling may be detected; it is about the size of an orange, elastic, and pulsates forcibly; it communicates to the finger a peculiar thrilling sensation, and the pulsations are synchronous with those of the heart. A very slight amount of pressure on the femoral artery suffices to arrest them, and when the compression is continued, the sac can easily be emptied; it appears rather thin, and when the pressure is suddenly removed, the blood rushes through it again, giving a very unpleasant and even painful sensation to the patient. He, indeed, *feels* the blood as it suddenly flows down the artery, whilst a very distinct bruit de soufflet can be heard on applying the ear to the tumour. The patient complains of pain when the swelling is handled, most of the uneasiness being around the knee. The pulsations can easily be seen by raising the leg. Another case of the same kind being treated at the time by pressure, under the care of Mr. Poland (to which we shall shortly allude), the latter decided upon using the same remedial means in this case.

September 1.—The usual apparatus was employed, consisting of the common clamp, so constructed that the pressure might be graduated, and the weight for the groin. Some difficulty was experienced in bringing the pad of the clamp to bear exactly on the artery, and as the weight of the cylinder pressing upon the artery in the groin was quite sufficient to control the flow of blood, the clamp was only used occasionally during the day.

The pressure caused the veins of the whole leg and thigh to become very prominent, and the parts about the knee swelled up considerably. A tingling, pricking pain was experienced down the thigh, and there was also partial numbness; but these sensations were less disagreeable than the pain in the popliteal space when the pressure was off. The bowels were ordered to be cleared with calomel and colocynth.

Second day.—The patient did not get much sleep last night, but, upon the whole, has rested better than on previous occasions. The weight in the groin was kept on most of the night; the knee is very painful; the tumour much the same.

Third day.—Bad night again. During the whole of the preceding day pressure was kept up by means of the weight and the clamp, which latter was arranged so as to act properly. The patient was seen frequently by Mr. Poland and by the dresser, and managed very nicely to alternate the pressure himself. Oxide of zinc to be powdered over the parts pressed upon. Pulsations of the tumour somewhat lessened in force; other matters much the same.

Fourth day.—The patient did not sleep well last night, and will take a grain of opium this evening. The pressure was kept up all the preceding day as before; but the man complains of a good deal of pain at the inner side of the knee-joint, when the articulation is handled. The tumour is decidedly firmer to-day; the pulsations not quite so forcible, but the thrilling sensation is still communicated to the hand.

Seventh day.—The pressure has been continued, but even two grains of opium gave the man but little rest at night; he feels very unwell, and is rather feverish. Two grains of morphia to be taken in the evening.

Eighth day.—Much better; has rested last night, but still complains of pain when the inner part of the knee and to some short distance above it is manipulated; the pressure has been admirably kept up, and the cure appears to be rapidly taking place.

Tenth day.—Going on very nicely. The patient screwed up the clamp rather too tightly last night, and having fallen asleep, the pain from the pressure caused him to dream about it. He felt as if a number of women were tearing the flesh from the bones about the knee, and so extreme was the pain

that he screamed out lustily; but after loosening the apparatus he was soon composed. The sac is certainly becoming denser; the pulsations are a little more reduced in force, but the thrilling sensations to the fingers and the tenderness about the knee still continue. General health good.

Twentieth day.—On the night of the fourteenth day, the patient neglected to adjust the pressure properly; the blood passed uncontrolled through the vessel, and the next day the pulsations were as forcible as on the first day of admission; the sac, however, was still tolerably firm. Since that time he has paid more attention to the apparatus. On the twentieth day the pulsations were much more subdued, and the sac firmer, so that on the whole the patient is going on well again. The enlargement of the articular branches of the artery, both on the inner and outer side of the joint, has been very obvious since the tenth day; even then the branch on the right side could be clearly and distinctly felt.

Twenty-seventh day.—During the past week things have gone on in the same way, the aneurismal tumour being now in the same condition as on the twentieth day. On the twenty-sixth day the patient was attacked with a little bilious derangement, which was controlled by calomel, opium, and a saline purgative; this produced rather excessive purging, for which chalk mixture and laudanum were taken.

Thirty-fourth day.—During the past week, matters have been going on in the same manner; little advance has been made; the tumour still pulsates but feebly, and it is harder; the leg is free from that continual pain which the patient originally experienced, although there is now an occasional temporary numbness and sometimes a darting pain in the thigh. This pain is probably the effect of the pressure; the patient has regained his ordinary state of health.

Forty-first day.—Much the same; the tumour is still pulsating; the integuments are slightly denuded, and the parts subjected to pressure are very tender and painful; the groin is sore. The oxide of zinc was ordered to be assiduously applied, and the points of pressure to be varied. General health good.

Forty-fifth day.—Much the same.

Fifty-seventh day.—Pulsation has not yet ceased; the tumour has become much firmer, and is a great deal smaller; it has not progressed so well as it would have done if the patient during the last week had been as attentive to it as he should have been.

Eighty-first day.—The tumour is much the same as stated in the last report. It cannot be diminished by pressure; it is firm and smooth, but pulsates strongly; the parts subjected to the compression of the instruments are still very tender.

Ninetieth day.—On the night of the eighty-eighth day, and part of the eighty-ninth, some gentlemen sat up, that the pressure might be continuously kept up night and day; this has produced some good effect, but the pulsations still continue.

Hundred-and-second day.—The swelling is certainly less, but the pulsations are strong, though they vary much as to their force; two or three days ago they had nearly ceased. The same apparatus is employed; groin still tender and sore from pressure; a plaster was applied to it. The general health continues good.

Jan. 14, 1854. (Eighteen weeks after admission.)—The tumour is still pulsating to a small extent, though not constantly, for at times it will cease for a few hours. The patient's health continues very good, the limb is free from pain, though somewhat swollen about the knee, and a few veins may be seen meandering about the joint. For the last week the pressure (by the same apparatus) has been made upon the *lower third* of the thigh instead of the upper, but no very obvious improvement has been yet observed from this proceeding.

April 4, 1854. (Twenty-ninth week after the beginning of the treatment by compression.)—Pressure is still kept up in the lower third of the thigh. Since last report, the patient has continued to make slow but sure progress. Mr. Hilton, yesterday, applied around the knee, and over the tumour, a bandage, and a tourniquet was kept on the artery for five days after the application of

the roller. The pressure of the tourniquet was gradually diminished until taken away altogether, whilst, at the same time, the bandage was daily more tightened. Pulsation in the tumour has much decreased before the tourniquet was taken off; but this result was considerably assisted by the bandage.

On April 22 (thirty-first week), the patient was ordered to move about; and, on May 2, compression having been continued for *eight months*, he was considered well-enough to be discharged with caution; the bandage still applied, though with a moderate degree of pressure. The artery may be said to be almost in its normal condition, with the exception of slight induration.—*Lancet*, May 27, 1854.

39. *New Mode of treating Ulcers from Irritation of the Nails*.—Mr. URE made some observations on this subject before the Harveian Society (March 2, 1854). He referred to the case of a young woman, aged twenty-three, who had been under his care in St. Mary's Hospital. Four months before her entrance, the great toe of the right foot became uneasy and swollen, the patient having pared the nail the day preceding. Ere long, a painful and irritable sore made its appearance by the side of the nail, which discharged from time to time a quantity of thick, bloody, and sometimes black-looking matter. She was thus disabled from going about and earning her livelihood. For four years antecedent, she had been in indifferent health, and had suffered from excessive flow of the catamenia. She had procured the advice of some respectable practitioners, and been under treatment during several weeks, without, however, obtaining any benefit. As the sore was rather in an inflamed state on her admission, poultices were applied. On the third day, when all surrounding inflammation seemed to have subsided, Mr. Ure prescribed the use of a salve composed of one grain of finely levigated arsenious acid, incorporated with an ounce of spermaceti ointment. He was led to try this remedy by the suggestion of Mr. Copeland, who deemed it almost a specific in ulcers of this nature. This was steadily employed for about ten days, without producing any marked change on the sore. Mr. Ure then ordered, instead, the continuous application of a hot saturated solution of alum. This induced rapid absorption of the thickened parts, and prompt cicatrization of the ulcerated surface, so that the patient was enabled to leave the hospital, cured, in the course of three days. Mr. Ure observed that, while alum is soluble in five parts of water at 60° Fahr., it is soluble in little more than its own weight of water at the boiling temperature. A hot saturated solution is, consequently, more energetic in its action than a cold one. He had been led to resort to its use in the above instance, from having witnessed its efficacy in an analogous case. A gentleman consulted him (Mr. Ure) in the autumn of 1852, concerning a sore seated by the side of the nail of the great toe, which had annoyed him for nearly a year. A variety of treatment had been exhausted in vain attempts at cure, including the plan proposed by Sir A. Cooper, of paring the nail as thin as possible, raising the edge, and inserting a small piece of lint between it and the sore; the free application of lunar caustic, as recommended by Mr. Wardrop; and also blistering the part. By the constant application of a saturated solution of alum, as hot as could be borne, this refractory sore was eventually healed in a few days.

40. *Gunshot Wound inflicted by the Minie Rifle*.—Mr. TUFNELL communicated to the Surgical Society of Ireland (March 11, 1854), the particulars of a case, which, he said, was at present of rare occurrence, but one he feared that was likely before long to become very common indeed. Its rarity just now, however, must make it interesting to the surgical profession, and for that reason he was induced to bring it under the notice of the Surgical Society.

The cast, which lay on the table, was taken from the foot of a boy, *ætat* 16, who was accidentally wounded by a shot from a Minie rifle, while the troops were practising at the Pigeon House, on the 25th of July last, and admitted into the City of Dublin Hospital the same day. The case was useful in showing the great difference between a wound inflicted by a conical piece of lead, as propelled from a Minie rifle, and the round bullet fired from a musket. The

nature of the injury was represented by the cast before the Society. There was a small semilunar split in the integument on the dorsum of the foot, situated over the centre of the metatarsal bone of the little toe, about a quarter of an inch in length, giving an appearance as if the ball had glanced across it, and superficially cut the skin. There was none of the appearance of a gunshot wound, there was no contusion, nor were the edges inverted. The ball passed downwards, fracturing the fourth metatarsal bone, and lodging itself deeply in the sole of the foot, yet leaving no trace of its passage beyond the most trifling slit in the skin, and that so oblique that a probe would not follow the track. It was impossible to discover the position of the ball on admission, and for seven or eight days from the receipt of the injury, no inflammatory action followed. The boy then got out of bed, and walked upon the wounded foot, the consequence of which was that inflammation commenced in the part, and tumefaction of the sole followed. He (Mr. Tufnell) then cut down through the centre of the foot and removed the ball, which was lying on its long axis close up under the metatarsal bone of the great toe. This ball weighed twelve drachms, the weight of the ordinary musket-ball being only eight drachms, making a difference between them of half an ounce. As to the fracturing of the fourth metatarsal bone in this case, there was no doubt about it, for he now exhibited a portion of the metatarsal bone which he extracted along with the ball. If the members present would take the trouble to examine the two balls before them, one of which had been fired and the other not, they would perceive a vast difference in them. The hollow iron cup in the one unfired, lay barely on a level with the base of the cone, whilst in the one which had been fired it was sunk deeply into the lead. This cup acted as a kind of wedge, and drove it more forcibly on. He found that the charge of gunpowder employed to propel a ball of twelve drachms from a Minie rifle was seventy-five grains; whereas in the ordinary musket, seventy grains were used for a ball of only eight drachms. On examining samples of the gunpowder employed, however, he found there was a great difference between them. That used for the Minie rifle was much harder and more highly glazed than the other, and being, therefore, a much finer description of powder, a smaller charge would perhaps be required than of the ordinary powder. The weapon now supplying to the army was a very superior arm to the musket. A soldier with it can fire with the greatest precision at six hundred yards, whilst with the ordinary musket he could not shoot with precision at more than two hundred yards. A man supplied with a Minie rifle, had therefore an immense advantage over one armed only with the common musket, and there was every reason to anticipate that the injuries inflicted by the former would be more severe and dangerous than those caused by the latter. He did not imagine that a shot from a Minie rifle would make a revolution round the body, as happened so often with the old musket-ball. His impression was, that if a Minie shot struck with force against the cartilage of the ribs, it would go directly through the body and transfix a man like an arrow, instead of passing round and escaping behind at the back. No later than that day, he saw a case illustrative of the latter, which was produced in the following manner: A deserter, observing that the escort in charge of him were drunk, attempted to escape. Two or three of the soldiers pursued and caught him; one man very drunk run up, and although the others held him securely, fired his musket into him; the ball struck upon a rib, ran round to the spine, and got exit there. That man would probably recover; but if it had been a Minie ball that was fired at him, he would assuredly have been killed.—*Dublin Medical Press*, March 29, 1854.

41. *Dislocation of both Femora at the same time in the same Subject.*—H. L. PRICHARD, Esq., relates (*Assoc. Med. Journ.*, April 21, 1854) the following example of this accident:—

“Thomas Phillips, aged 15, a railway wagoner at the Cwmavon works, on the evening of the 25th of March, while in a stooping posture in the act of turning the switch, was overtaken and knocked down by a truck running with some velocity down an inclined plane. He escaped the wheels; but the body of the

carriage, which was ten inches only from the ground, passed over him, and, as he described it, 'doubled him up.'

"The case presented a remarkable appearance. The knees and toes were inverted; the former were closely approximating each other, while the latter rather overlapped one another. The head of each femur was distinctly felt under the glutæi, on the dorsum of the ilium.

"The reduction of the right hip was readily effected with the pulleys, in the manner recommended by Sir Astley Cooper, with no other preparation than that of chloroform, which the patient took *con amore*. In eight or ten minutes the bone passed into its socket with a satisfactory jerk, audible to the bystanders. The left was immediately afterwards subjected to the treatment in like manner, but was not so easily reduced. Extension having been kept up during thirty-five minutes, the knee was found to have descended parallel with that on the opposite side. The pulleys were relaxed, and the limb simultaneously rotated, while my colleague, Mr. F. M. Russell, kept up the transverse extension in the usual manner, and the reduction was effected, but without a perceptible crack—attributable, I apprehend, to the exhaustion of the muscles from continued extension.

"I presume an accident of this description is very rare. It is the first with which I have met; and I do not recollect to have read of such a case."

[An example of the same accident was recorded in our preceding No., p. 412, by Dr. T. C. Barker.]

42. *Dislocation upwards and backwards of the Metatarsal Bone of the Great Toe alone.*—The following case of this very rare accident was communicated to the Surgical Society of Ireland, Feb. 11, 1854, by Dr. J. H. POWER.

John Nichols, aged 49, was admitted into Jervis-street Hospital, on the 23d of January last; he had been a passenger in the Australian emigrant ship, the *Taylor*, which struck upon the rocks of Lambay on the morning of the 21st.

Whilst the vessel was rolling from side to side after she had struck, he watched his opportunity and leaped from the vessel upon a ledge of rock, about eighteen or twenty feet from the spot from which he sprang. He felt his situation now was one of danger, for he every moment feared, from the rolling of the ship, that the masts might perchance strike him whilst lying upon the rocks. He therefore crept upon his hands and feet, as well as he could, until he had escaped beyond the reach of accident, either from the ship or sea. From the place to which he had escaped he was conveyed to a farmhouse upon the island, and from thence, upon the 23d, he was conveyed by the Thames steam vessel to Dublin, and at once conveyed to hospital.

His escape, however, was not unaccompanied by injury, and that of a very severe and painful description. His right foot had sustained the principal violence of the leap which he had made, and for this injury he was admitted into hospital.

The foot was greatly swollen, both in the sole and on the dorsum; the general swelling arose chiefly from the effusion of blood and serum. On making examination as to the length of the foot, it was evident that *there was shortening, measuring from the heel to the point of the great toe*. This shortening evidently depended on some altered condition of the bones between the ankle-joint and the last phalanx of the great toe. For in this situation the fore shortening was quite apparent, although the end of the tibia was clearly situated in its normal position. The hinder extremity of the os calcis also occupied its natural relation to the bones of the leg. I was struck with the remarkable appearance presented by the sole of the foot. Nothing of the ordinary hollow or concavity was here visible; on the contrary, its place was supplied by a well-marked convexity, firm and resisting. When we examined the upper surface of the foot, we ascertained a firm and unyielding elevation; and notwithstanding the general tumefaction, it was quite clear that this elevation was formed by some bone in a state of luxation. When the sole of the foot was next carefully examined, it was found that another firm body subjected itself in this situation, and was nearly the antipodes to the tumour on

the dorsum of the foot. The inner side presented a twisted appearance; the great toe seemed as if rotated on its long axis; its inner margin was turned upwards and inwards, and its outer margin in the opposite direction. From the point of the toe to the elevation on the dorsum measured four inches and a half.

All the other articulations of the foot appeared to me to be uninjured. I came to the conclusion that this was an example of that exceedingly rare accident—namely, *a dislocation of the metatarsal bone of the great toe upwards on the dorsum of the foot.*

My colleague, Mr. Stapleton, with whom I had the pleasure of being on duty at that time, examined the case most carefully, and fully confirmed the conclusion at which I had arrived. We decided on immediate reduction, and having ascertained that our patient's great toe terminated in a full and bulbous extremity, I determined to avail myself of this advantage, and accordingly fastened a narrow and firm piece of calico, in the form of the clove-hitch knot, upon the great toe, immediately behind its bulbous termination. This was our means of extension. I next caused the leg to be flexed upon the thigh; the knee was then fixed by an assistant, whilst another, with both hands, laid hold on the leg near the ankle, and so kept it fixed also. Extension was now made in the direction of the abnormal axis of the toe—that is, it was drawn firmly and steadily downwards and forwards in the first instance. When the extension had been kept up for a short time, I placed my right hand, clenched firmly, against the resisting bodies, which caused the principal swelling in the sole of the foot, whilst I placed the fingers of my left hand firmly upon the elevation on the dorsum of the foot. I now directed the gentleman, who had charge of the extending medium, to alter suddenly the direction of his force, and to pull the toe in the direction upwards and forwards, and whilst this manœuvre was performing, I forcibly pushed upwards my right hand against the tarsal bones, whilst with my left I pushed downwards the tarsal end of the metatarsal bone of the great toe, and soon had the satisfaction of hearing a peculiar friction sound which conveyed the pleasing intelligence to Mr. Stapleton's sense of touch and hearing, as well as to my own, that the displaced bone had been reduced to its ordinary situation. The foot was then placed in a suitable position, and the patient is now progressing to our satisfaction.

Upon the subject of dislocations at the tarso-metatarsal articulations, we find the following records. Two cases of dislocation of the metatarsus from the tarsus came under the observation of Dupuytren; the first was *a dislocation of all the metatarsal bones on the corresponding bones of the tarsus*; the second, *a dislocation of the metatarsus on the tarsus, affecting both feet at once.* In Professor Smith's admirable treatise on *Fractures in the Vicinity of Joints*, two more cases are recorded; in the first, *the metatarsal bones, along with the internal cuneiform, were luxated upwards and backwards upon the tarsus*; in the second case, *the second, third, fourth, and fifth metatarsal bones, together with the internal cuneiform bone, were dislocated upwards and backwards upon the tarsus.* In Mr. South's case, *there was a luxation of the fourth and fifth metatarsal bones upwards and backwards upon the cuboid.*

In the 33d number of the *Dublin Quarterly Journal of Medical Science*, Mr. Tufnell has recorded a case of luxation downwards and backwards of the three internal metatarsal bones. In the cast which I now exhibit to the Society, we have the features of the accident described by my friend Mr. Tufnell.

From all I can collect as to the nature of these various luxations at the tarso-metatarsal line of articulation, I have not been able to discover the record of a case precisely similar to that which I have brought before the Society to-night—namely, *a dislocation upwards and backwards of the metatarsal bone of the great toe alone.*—*Dublin Medical Press*, Feb. 22, 1854.

43. *Radical Cure of Reducible Inguinal Hernia by a New Operation.*—Mr. SPENCER WELLS read before the Royal Medical and Chirurgical Society, May 9, 1854, a paper on this subject. The author commenced by alluding to two cases in which he had operated himself, to three others which had fallen under his own observation, to nineteen operated on by Professor Sigmond, of Vienna, to

thirty-two by Professor Rothmund, of Munich, and to an indefinite number by Professor Wützer, of Bonn, the inventor of the proceeding, in none of which had any fatal result followed, success having been secured in a very large proportion. He remarked upon the prevailing opinion against any operation for the radical cure of reducible hernia, and traced it to the fatal results of dangerous methods now deservedly exploded, arguing that, if any plan could be shown to be both safe and effectual, the judgment of the profession should be modified. After some observations on different means of effecting a radical cure, he described that of Wützer, and exhibited the instrument used by this surgeon. It consists of a wooden cylinder, a needle passing through it, an outer wooden cover, and a screw to bind the cover and cylinder together. The mode of employing it was then described. After pushing a part of the skin of the scrotum before the forefinger, the cylinder takes the place of the finger, the needle is passed through it, through the sac and integuments, and serves, with the screw, to fix and bind the cover and cylinder together, so that the invaginated skin, the walls of the sac, and the abdominal integuments are pressed together, with any degree of force that may appear safe or desirable. The instrument is left *in situ* for six or eight days, with the effect of producing adhesion of the whole of the sac, a firm organic plug filling the inguinal canal. Very little pain is said to be produced. The patient is confined to a sofa until the needle puncture is healed, and wears a truss for three months afterwards to avoid the danger of breaking up of the new adhesions. Mr. Wells then went on to show that there were grounds for admitting this operation within the province of legitimate surgery, and that the classes of cases in which it should be performed were—first, in strong persons up to 40 or 45 years of age, where the hernia is of a moderate size. In such persons, a perfect cure may be expected. Secondly, in cases where the hernial protrusion is so large that it cannot be kept up by any mechanical contrivance. Here a radical cure is not to be hoped for, but the evil is so much diminished that a truss can be made to keep up the hernia efficiently. He concluded by expressing his belief that, as by this method the whole of the inguinal canal can be permanently closed, it is far superior to all others, and that the evidence he had adduced of its safety and success should lead to its further adoption in this country.

Mr. Holmes Coote asked whether M. Jobert de Lamballe had not injected into the hernial sac some pure tincture of iodine, which had produced exudation of a mass of lymph occupying the whole of the sac, and extending up the inguinal canal. He believed it had been stated, in some of the foreign journals, that such a plan had been adopted, and had resulted in a permanent cure.

The president said, it would be interesting to know the comparative safety of this proceeding and that just recommended by Mr. Spencer Wells.

Mr. Arnott asked if he perfectly understood the principle on which the new instrument acted? It appeared that a small piece of wood was introduced into the inguinal canal, pushing a portion of the scrotum before it, and that, by means of a needle carried through the piece of wood, the whole was stitched together for a time. Observations could hardly at present be made upon the mode of proceeding recommended by Mr. Wells, as it was one of which, as yet, they had no experience. There was certainly some evidence in its favour, and the number of cases which had been adduced that had not been attended with any risk, showed that the operation might be performed in the way suggested without much chance of mischief. Some of the persons who had been mentioned as having performed the operation were well known to himself; one especially, in whose testimony he should have great faith, if a sufficient time had elapsed since his operations, was Dr. Sigmund, one of the surgeons at the large hospital at Vienna. It was stated that he had had nineteen cases of the kind, fifteen of which were successful. It was not, however, stated, how long the cases were observed after the operation. He should like to hear from Mr. Wells, what was the condition of the parts after a year or two; whether the portion of skin which was invaginated remained in the inguinal canal, or came down; and also what length of time had elapsed between the operation and the supposed permanent cure of the patient. The principle of introducing

a portion of skin into the canal, and stitching the parts together was not new, but the particular method described no doubt was so.

Mr. Wells said, that Mr. Arnott's description of the action of the instrument was perfectly correct. A cylinder was introduced in the place of the finger; a cover was placed over it, and it was kept in the proper position by the needle which was passed through the skin and sac; the two were then screwed together, and left in that position seven or eight days. Professor Sigmund had told him (Mr. Wells) that he had examined a case six years after the operation. The first cases of Professor Wutzer were in 1837; and last year some of these were under his observation, and had remained perfectly cured. He (Mr. Wells) had seen his own cases lately—one of them he operated on in 1848, and the other in 1850. Both patients remained perfectly sound, although they took very active exercise. With regard to the state of the parts, the plug, in the course of time, nearly disappeared; indeed, after five or six years one could scarcely tell that an operation had been performed. Five or six months after the operation, a round hard plug might be felt in the canal, but it gradually became absorbed. He (Mr. Wells) concluded that the adhesion was perfect, as no relapse took place. He believed that M. Jobert de Lamballe had injected iodine into the hernial sac with success, but only in a single case. Many cases, however, were on record in which the injection of various stimulating fluids had led to fatal peritonitis, and that, he believed, was one reason why all operations for the radical cure of hernia had fallen into discredit.—*Med. Times and Gaz.* May 20, 1854.

44. *Abnormal Obturator Artery in Femoral Hernia.*—Mr. MACKENZIE showed to the Medico-Chirurgical Society of Edinburgh (February 1, 1854), a dissection of a sac of femoral hernia, in which the abnormal obturator artery had encircled the neck of the hernia, and in which he had had the good fortune to operate without wounding the vessel.

The patient was a female; the hernia, of the size of a hen's egg, had been strangulated for two days before she was admitted into the hospital. The sac was opened in the operation, and the stricture, in Gimbernat's ligament, divided as usual. A slight flow of venous blood took place on the stricture being divided. The bowel was very dark, but not gangrenous, and was returned into the abdomen. The symptoms were quite relieved, and the wound healed rapidly. On the eleventh day after the operation, however, a little fecal matter began to ooze through a small part of the wound, which remained unclosed; a preternatural anus was established. After a time the feculent discharge began to diminish in quantity, and for some time there seemed to be every prospect of recovery, when pulmonary symptoms appeared, and she died from phthisis six months after the performance of the operation.

The parts were removed for dissection. The peritoneal sac of the hernia formed the walls of the preternatural anus, and the obturator artery, with an accompanying vein, was found closely united to the edge of Gimbernat's ligament on the *inner* side of the neck of the sac. The obturator artery arose in a trunk, about half an inch in length, common to it and the epigastric, from the external iliac artery, and had been pushed, by the descent of the hernia, to the pubic side of the neck of the tumour.

It had been a disputed point among anatomists how far the common variety of the origin of the obturator artery endangered the vessel in the operation for femoral hernia; but it was generally admitted that the descent of the hernia between the femoral vein and the abnormal artery (as in this instance) was very rare.

Cases had been recorded where fatal hemorrhage had occurred in the operation, apparently from this variety of the artery, but Mr. M. was not aware of any case having been recorded in which this variety had existed, and in which the vessel (as in the present case) had escaped being wounded in the operation.

The fortunate operation was of course mainly attributable to accident; but he had used the precaution, which he had always adopted in operating for femoral hernia, of introducing no more than the point of the knife within the stricture, and of using a slightly blunted cutting edge for the division of the

stricture—a precaution which had been long ago recommended with a view of avoiding the accident in question.—*Monthly Journal of Medical Science*, March, 1854.

45. *Amputation at the Shoulder Joint*.—MR. ALEXANDER URE read a paper on this subject before the Harveian Society, March 16, 1854. He observed that amputation at the shoulder-joint may be performed in a variety of ways. M. Lisfranc, in his work on Surgery, had described no less than thirty-two different modes of doing the operation. Those chiefly resorted to at the present day were either the flap operation, or what is termed the oval method. Where the patient was of a spare make, and the shoulder of moderate dimensions, the oval method answered well; where, on the other hand, the shoulder was large and brawny, the flap operation was more eligible. He remarked, that where the soft parts were extensively torn and bruised, or the shoulder was misshapen by disease, no specific could apply, and the surgeon must be guided by existing circumstances. The author, after pointing out the anatomical relations of the part, detailed the successive stages of each operation. In reference to the formation of the flap, he stated that if the arm be brought into a horizontal position, the deltoid muscle can be fully grasped by the hand of the surgeon, and lifted up in a relaxed condition, so as to permit of the knife being thrust between the muscle and the capsule of the joint. He inculcated the importance of leaving the division of the axillary vessels to the last stroke of the knife, and said the flow of blood might be completely controlled by simply directing an assistant to place his two thumbs on the bleeding face of the stump, while with the fingers of each hand he laid hold of the integuments. If the compression were skilfully made, one hand would usually suffice. He deprecated every method of amputating in this situation, which necessitated mediate ligation. The author related the particulars of a case in which he had recourse to the oval operation. The patient was fifty-five years of age, of rather short stature and spare make; his calling that of a cow-keeper. He was brought into St. Mary's Hospital on September 16, 1853, having shortly before been knocked down by a heavy-laden wagon, the wheel of which crushed his left arm, producing a compound comminuted fracture of the humerus just above its upper third. The adjunct soft parts were severely bruised and lacerated; the deltoid muscle was partially detached and retracted towards the acromion process of the scapula; there was a penetrating wound through the integuments at the site of the fracture by which a probe could be passed nearly up to the neck of the bone; the limb below the seat of injury was quite cold; no pulse could be felt at the wrist. When seen by the author about an hour after admission, the patient was in rather a prostrate condition; the countenance anxious; the pulse in the sound arm feeble, tremulous, 108 in the minute: the tongue dry, and its surface yellowish-brown. After consultation with two of his colleagues, the author proceeded at once to remove the arm at the shoulder-joint, the patient having been previously rendered insensible by chloroform. A vertical incision was made through the skin and subjacent tissues down to the bone, commencing at the acromion, and terminating at a point corresponding to the fold of the armpit. This was continued by a semicircular sweep through the integument of the back of the arm, and brought round so as to join the extremity of the former cut. The fleshy structures towards the acromial side were then carefully dissected, and after cutting across the tendons, and laying open the capsule of the joint, the head of the bone was dislocated. The edge of the knife was now directed so as to cut its way out, downwards, behind the bone, precaution being had to avoid the artery, which at this stage was firmly held and compressed by Mr. James Lane. Four vessels were immediately secured in the ordinary way. As a quantity of blood continued to well from the axillary vein, it was likewise tied. A portion of the bruised flesh having been removed, the edges of the wound were brought together, and retained in apposition by means of sutures, and a pledget of wet lint applied. During the operation the subclavian artery was most efficiently compressed upon the first rib over the clavicle by Mr. Lane; hence the amount of blood lost did not exceed two ounces, and that chiefly venous. In reference to ligation of the vein, the

author observed that it not only served to arrest the bleeding, but also to obviate the risk of jeopardy from the ingress of air into a gaping orifice in close proximity to the heart. A wound of the axillary vein had already proved fatal from this circumstance. Roux thus lost a patient while amputating at the shoulder-joint. There was an anatomical reason why such an accident was prone to occur. The axillary vein below the clavicle was so intimately connected with the coraco-brachial fascia, and the perichondrium of the cartilage of the first rib, that when incised or cut across it was unable to collapse, and thus offered every facility for the inlet of air. On examining the amputated limb, it was ascertained that the bone was broken right across a little below the neck, and again two inches lower down, the intermediate portion being shivered lengthwise into four irregular fragments. These sharp pieces were deeply buried in the adjunct flesh, which was in a mashy condition both above and below. The sheath of the brachial vessels was gorged with blood for several inches in length. The artery was apparently entire. The patient, after he was placed in bed, was directed to have a dose of laudanum, and every third hour half an ounce of brandy. The patient made a good recovery, and was removed on the 16th December to the convalescent institution at Carshalton; from thence he was discharged quite well in less than a month. In some remarks upon the case, the author alluded to the dangers attendant upon the attempt to save a limb in such a state from the consequent exhausting processes of sloughing and suppuration, or from the supervention of tetanus. He also made some remarks on the importance of the after treatment, chiefly with reference to the position of the patient.—*Lancet*, April 1, 1854.

46. *Amputation at the Knee-joint.*—By H. G. POTTER, Esq., Surgeon to the Newcastle Infirmary.—Harriet S—, aged forty, was admitted into Newcastle-upon-Tyne Infirmary on the 8th of December, 1853. She states that about twenty years ago she knelt upon a small stone, which gave her great pain in the knee. From that time until about six months ago she had severe pain, at intervals, in the joint, but was not laid up. During the last six months she has been confined to bed, and though everything seems to have been tried which was likely to do good, the disease increased, and the leg became more and more flexed until, as at present, it has reached the utmost degree possible. Any attempt at extension gives intense pain; some tortuous sinuses run down to the bone; and there is every symptom present which indicates ulceration of the cartilages. She is very thin and hectic, and is extremely anxious to have the limb removed.

Operation. Dec. 13.—An incision commencing a little above the middle of the external condyle was continued across the knee, round the upper half of the patella, to the middle of the inner condyle, and ended a little above it. This incision separated the patella from its superior attachments, and opened the joint. The ligaments were next divided, and the saw introduced behind the condyles, which were with the greatest facility sawn through. I used the saw I described in THE LANCET of 1845, vol. ii. p. 546, and which will be found to be of great use in such operations. The knife was now placed behind the joint, and a full-sized flap formed from the back of the leg. No difficulty was met with in any part of the operation, and the flaps came nicely together, in which position they were retained by sutures and plaster. Chloroform was successfully administered by Mr. Gibb, our talented house-surgeon.

On examining the joint after removal, the cartilages were found to be ulcerated, and the synovial membrane pulpy.

Dec. 14.—There is a remarkable change in the countenance this morning. From the time she entered the hospital, until to-day, she has had a very haggard look; now, however, the countenance has assumed a placidity which contrasts very favourably with its previous disturbed appearance. The pulse is quiet and regular, and she rested well during the night.

From this time the case went on well, the flaps united by the first intention, the patient acquired strength and flesh, and was discharged cured on the 17th of March, 1854.

Should I again perform this operation, I would remove the diseased synovial

membrane from the upper flap, because I feel convinced that this diseased structure was the cause of a discharge which continued much longer from an old sinus than would have been the case had the synovial membrane been removed.—*Lancet*, May 27, 1854.

47. *Excision of the Knee-joint*.—Mr. R. J. MACKENZIE stated to the Medico-Chirurgical Society of Edinburgh (March 1, 1854), that he had now performed this operation in three cases. The first would be immediately presented to the Society, and would speak for itself. Recovery in the second case had, from various causes, proved more tedious than it had at first promised to be. The patient, however, progressed satisfactorily, though slowly; and Mr. Mackenzie had no fear as to the ultimate result. The limb was becoming rigid at the knee, and was straight, shapely, and but moderately shortened. He hoped at a future meeting to present this patient, with a sound and serviceable limb. In the third (a peculiarly unfavourable case for the operation), the result had been fatal. The case was one of acute ulceration of the cartilages, accompanied by suppuration; the joint being distended with pus. The patient, a lad of 18, was in an advanced stage of hectic, emaciated, and much exhausted by continued suffering. He was most reluctant to submit to amputation; and, having seen one of the patients walking about, on whom the operation of excision of the knee-joint had been performed, he expressed an anxious wish that an attempt should be made to save the limb by the performance of this operation. It was a matter of regret that this request had been complied with; for, although amputation held out but a small chance of recovery, the case was little suited to test the merits of the operation of excision. Immediate and great relief followed the operation; and, as far as the limb was concerned, everything had progressed as favourably as could have been wished. Diarrhoea, however, which had been threatened before the performance of the operation, set in; the tongue and lips became covered with aphthous crusts; he suffered from cough and night sweats; and sank, in the course of a month after the operation. The wound was in great part healed, and no difficulty had been experienced in keeping the limb in excellent position. Permission could not be obtained to examine the body; but Mr. Mackenzie had little doubt that tubercular disease of internal organs would have been found.

Mr. Mackenzie stated that, since the subject had been last brought before the attention of the Society, the operation had been performed by several able surgeons in England, Scotland, and Ireland. He had written to four of these gentlemen, requesting them to furnish him with the result of their experience; and he had received from all very kind and full replies to his inquiries. He read some extracts from the letters which he had received from Mr. Page of Carlisle, Mr. Gore of Bath, Dr. Keith of Aberdeen, and Mr. Butcher of Dublin, all of which gave favourable accounts of the cases under their care, and expressed themselves as being advocates of the operation.

A patient (a man between forty and fifty years of age), was then exhibited, in whom Mr. Mackenzie had performed the operation of excision of the knee-joint a year previously. He appeared to be in robust health, and traversed the hall, in various directions, with ease and celerity. In walking, the toes were pointed downwards, but, his body yielding slightly, he planted the foot firmly on the ground, and rested his entire weight on the limb without uneasiness. In the horizontal posture, he elevated and depressed it with the greatest nicety. On examination, the limb was found slightly curved outwards, firmly ankylosed at the knee, three quarters of an inch shorter than its fellow, and equal in muscular development. The heel of the shoe was raised inside three-fourths of an inch, to allow for the shortening. He stated that he had been walking for six months.

Mr. Syme said, that the question was not as to the possibility of saving the limb, but how far excision of the knee-joint could be substituted for amputation of the thigh, and an useful limb preserved. Now, all subsequent experience went to show that the operation was not a good one, and that it could not with advantage be substituted for amputation, even admitting the possibility of keeping on the limb. He questioned the usefulness of the limb in the case

shown to the Society. The future must be looked to; and much depended on the avocations of the patient. Should they be rough and laborious, the limb would speedily be affected with swellings or ulcerations, and amputation would become necessary. Again, how many times have the limbs been lost, or unsatisfactory results obtained? The question was not Mr. Mackenzie's skill as an operator; but how far we could regard this operation as a boon to the human race.

Mr. Mackenzie was sorry to hear the statements of Mr. Syme, which he could not but regard as the results of mere theory, and not deduced from a fair consideration of the facts which had been brought forward. He would only refer to the opinions of some of the best surgeons in the country, whose recent experience of the operation bore ample testimony to its value, as shown in the letters which he had read to the Society. Mr. Syme had spoken of the great loss of life following the operation. In this Mr. Syme was mistaken; of the last fifteen cases in which the operation had been performed, three cases only had terminated fatally; a proportion which would surely bear comparison with the operation which Mr. Syme recommended, amputation of the thigh. Mr. Syme, too, spoke of the useless limbs which were preserved. He would refer to the patient whom they had just seen, and would leave the Society to judge whether his limb were useless; he would refer to such cases as that of Mr. Page, where the patient was standing or moving about at his employment the whole day, sometimes walked six or seven miles to the country for his amusement, and could walk with any of his companions. He would refer, indeed, to the gratifying results in this respect of nearly every case in which the operation had been performed of late years. With scarcely an exception, the operation had been the means of saving a truly serviceable limb.

Mr. Syme did not expect to change Mr. Mackenzie's opinions. The object of his remarks was to state his own; and a little time would show which were correct. He might be wrong in the conclusions to which he had come; but he begged to adhere to them.—*Monthly Journ. Med. Sci.* May, 1854.

48. *Dieffenbach's Operation for Ununited Fracture.*—Mr. MACKENZIE exhibited to the Medico-Chirurgical Society of Edinburgh (February 1, 1854), a thigh-bone on which he had practised Dieffenbach's operation for the cure of ununited fracture, the drilling of the ends of the bone and the insertion of an ivory peg.

The patient, a seaman, had fractured his thigh between two and three years before he was admitted into the hospital under Mr. M.'s care. He was treated in a public hospital in America, and he attributed the want of union to the limb not having been kept steady, the bandages confining the limb having been loosely applied.

The fracture was in the middle of the bone, and the lower fragment was retracted about an inch and a half on the inner side, and behind the upper. The limb was quite flexible at the seat of fracture, and was almost entirely useless as a means of support.

The ends of the bone were drilled with a small gouge worked by a carpenter's brace, and a peg, two and a half inches in length, was driven home with a hammer. The presence of the foreign body gave rise to such extensive inflammation and constitutional disturbance, that it was found necessary to remove it at the end of eight days. Profuse suppuration took place, undermining the whole deep textures of the thigh, and it seemed doubtful for some time whether the patient was not to sink from the effects of the operation. He rallied, however, and the suppuration gradually diminished. Firm osseous union took place, and at the end of some months the patient began to make use of the limb in walking. A sinus, however, continued to discharge matter at the seat of operation; and, after he had been walking for some weeks, he suffered from an attack of erythema of the limb; suppuration of the knee-joint followed, accompanied by hectic fever, and amputation was performed immediately above the seat of fracture.

The preparation showed the fractured ends firmly united by a large osseous deposit.

Mr. Mackenzie believed this was the only case in which Dieffenbach's method

had been practised in Edinburgh. The result was not encouraging as regarded its further trial, at least in the thigh. A considerable amount of success had followed the practice, in ununited fractures of the leg and forearm, in the hands of the London hospital surgeons; but the only other case in which, as far as he could find, it had been attempted in the thigh, had proved equally unfortunate; he alluded to a case by Mr. Square, of Plymouth, in which, as in the present case, it had been subsequently found necessary to perform amputation. —*Monthly Journal of Medical Science*, March, 1854.

49. *Puncture of the Bladder through the Symphysis Pubis.*—In our No. for April last (p. 403), will be found recorded a case in which this operation was performed by Dr. D. LEASURE of New Castle, Pennsylvania. At a recent meeting of the Medical Society of London, we find (see *Lancet*, April 15, 1854), that a paper entitled “A new method of puncturing the bladder by Dr. Brandes, of Jersey, was read. This new method is the one practised by Dr. Leasure. Dr. Brandes states that the practicability of this operation may be readily proved by any one who chooses to try the experiment upon the dead body, and appeals to the successful results of 100 instances in which he himself has operated on the dead subject. In these experiments, whether performed with the bladder empty or distended, he had never once failed in causing the instrument to enter that organ. The mode of operating is extremely simple. “The patient should be placed erect, resting against a wall; a longitudinal incision, about one inch in length, is then to be made in the integument immediately covering the symphysis pubis. A hydrocele trocar is then to be introduced at its upper third, and directed somewhat obliquely downwards and backwards towards the sacrum, varying the direction according to circumstances. A piece of flexible catheter is then to be introduced through the canula of the trocar, which, being retained *in situ* by a tape passed round the hips, completes the operation.” Mr. Hancock stated that the chief objections which he had heard urged against the operation were, the risk of infiltration of urine into the cellular tissue occupying the space between the pubes, bladder, and neighbouring parts; the difficulties likely to attend the healing of a wound in a cartilaginous substance, and the possibility of the inflammation at the symphysis being communicated to the peritoneum. The advantages of this over the other operations for puncture of the bladder were considered to be—the ease with which that viscus might be reached, in consequence of its fixed position in this situation, although it or the parts in its neighbourhood might be diseased; the absence of danger of wounding any important structures, especially the peritoneum; its better adaptation to cases of contracted bladder, whether from age or any other cause, in corpulent persons; in displacements of the bladder, whether congenital or the result of injuries sustained in the perinæum or pelvis, or as results of pregnancy, retroversion of the uterus, obliteration of the meatus urinarius by disease, preternatural descent of intestines, &c.; that it is less painful, and is more expeditiously performed than the other operations; that the wound being smaller, the source of irritation would be less, a point of much importance where irritation and fever already exist in the system; the catheter being more fixed is less likely to escape from the contraction of the coats of the bladder, or, on the contrary, to slip into the bladder, an accident which has been known to occur where puncture above the pubes has been had recourse to. It can, likewise, be more readily replaced when its withdrawal has been necessary. It is eligible, in certain cases, in which puncture of the rectum or urethra are inadmissible on account of altered states of the parts. The readiness with which the patient may be placed in a position suitable for the draining away of the urine, in which respect it is greatly superior to the supra-pubic operation. Its applicability in cases of retention of urine arising from inflammation in the bladder itself, where that viscus becomes too irritable to admit of the accumulation of the urine to the extent even of a few ounces without extremely urgent symptoms of retention supervening, remaining, of course, at the same time, in an undistended condition, a state which would render the supra-pubic operation ineligible. The author, moreover, announces that since his paper was

written in the year 1838, the operation has been successfully performed on several living subjects by himself and others, without any of the ill consequences apprehended by some theorists. Cases are related in the *Medical Transactions*, &c., Bengal, vol. viii. part 2.

50. *Ovariectomy, with reference to its Introduction into Legitimate Practice.*—[Mr. G. B. CHILDS lately read a paper on this subject before the Medical Society of London, an analysis of which, with the report of the discussion which took place, we give in order to show the opinions entertained in London in regard to this operation.]

Since the end of the last and the commencement of the present century—the era marked by the labours of Hunter and Baillie—the numerous diseases classed under the vague term of female complaints have received much attention, and have been better understood, principally through the more frequent employment of improved philosophical instruments, and also through more minute and extended inquiries into the phenomena of disease. In these investigations the diseases of the ovaries seem to have attracted the notice of British earlier than of Continental medical men. Three operations had been proposed for ovarian diseases—viz., puncture, excision, and extirpation. The operation of puncture was scarcely better than leaving the patient without any treatment, for the records of 170 cases collected by Dr. Robert Lee proved that the disease seldom remains quiescent under no treatment, while it is seldom cured and scarcely ever relieved by tapping. The author would not, however, follow the opinion of those who ascribed death to this operation, for the fatal result could no more be attributed to it in this case than in ascites. The operation of excision, as well as those by injection and the seton, were shown to have arisen from the discovery that so-called ovarian diseases were often accompanied with hydatids or cysts, by steatomatous or sarcomatous growths, and that sometimes even malignant growths were present. Le Dran endeavoured to destroy the morbid cyst by injection and wadding; Chopart and Desault opened the sac or sacs with caustic; Dzondi laid open the sac and attempted to remove it by forceps; and at last De la Porte and Morand proposed extirpation, which was performed by L'Aumurier in 1782. Mr. Childs then noticed the operation of Mr. I. B. Brown, and referred to the complications often discovered during the operation for excision. Proceeding then to speak of ovariectomy, he disapproved of the usual practice of delaying the operation, alleging that delay can produce no benefit to the patient, and that it will be attended with greater probability of the formation of adhesions. The errors in diagnosis which have been sometimes committed form no argument against the operation. It was a remarkable fact that sterility was a very common accompaniment of ovarian disease; but it was also true that ovarian tumours were sometimes found in women who had borne several children. The author adduced a short analysis of the cases of ovariectomy, collected by Dr. R. Lee, from which it appeared that—1. The abdomen was laid open in eight cases, in which no disease was found; of these two died. 2. The abdomen was opened in fifty-four cases, in which the diseased mass could not be removed; of these there died eighteen. 3. The diseased ovary was removed in one hundred patients, of whom thirty-eight died. The mortality was not to be considered large when it is remembered how common it is to delay the operation. In many of the fatal cases, and of those in which the operation could not be performed, it would be easy to show that delay had been the principal cause of the failure or of the fatal result. Hence the author concluded that to abandon ovariectomy in all or even in most cases would neither contribute to the interests of humanity nor to the honour of surgery. The author concluded his paper with the narrative of two cases in which he had performed the operation. The first was successful; in the second, the patient died of diarrhoea.

Dr. Snow remarked that much uncertainty existed with reference to ovariectomy, from the very unsatisfactory nature of the statistics of the operation. The successful cases were usually published, but many unsuccessful ones, he believed, never met the light. He did not insinuate that cases were withheld because they were unsuccessful; but many gentlemen who performed the

operation were not in the habit of laying their cases before the Societies, or publishing them in the journals. His (Dr. Snow's) own experience, which extended only to four cases, at which he had been present, was one recovery and three deaths. He did not think that the comparison between this operation and amputation could be sustained; there was no analogy between the cases requiring the proceeding. In cases which required amputation the patient might be unable to exist without its performance; but this was not essentially the case in ovarian dropsy, particularly in the early stage, at which Mr. Childs recommended interference; for then the general health of the patient might be good, and there might be no reason why he should not live for a number of years. It would in such a case be scarcely justifiable to operate with the imminent risk of death ensuing in three days, and in cases performed late the result was generally fatal.

Dr. WINN considered that the paper of Mr. Childs had placed the whole question in a clearer light than it had been before presented; but still thought we were yet unable to arrive at a satisfactory opinion regarding the value of the operation, from want of information on some important points connected with it. He believed that the danger to be apprehended from wounds of the peritoneum had been much overrated. He could not, however, recommend a very early interference in these cases, from observing how Nature occasionally interposed to arrest the disease. He had lately seen one case in which the patient had improved much after the disease had existed for many years. Dr. Ashwell had recently recorded in *The Lancet* some cases of fibrous tumours of the uterus which had disappeared spontaneously.

Mr. GAY remarked on the more frequent performance of ovariectomy since the introduction of chloroform. No doubt the proceeding was successful in many cases, but he agreed with Dr. Snow that our statistics on the subject were valueless. It had been stated that the average duration of life in persons affected with ovarian dropsy was three or four years: if you could prolong this period by operation, then a boon was conferred by its performance. But we had no evidence before us to show that this was the case. It was desirable, however, to determine which were the cases likely to be benefited by operation. The first case recorded by Mr. Childs he (Mr. Gay) considered favourable to the proceeding; but he had doubts as to the propriety of the operation in the second case, from the circumstance of some ascites being present, fluid being detectable in the cavity of the peritoneum.

Mr. I. B. BROWN said that hitherto the statistics of the operation had been so imperfect that no conclusion could be drawn from them. Individual experience, he thought, must decide the question as to whether or not this operation was a justifiable one. Having tried most of the plans which had been suggested for the cure of ovarian dropsy, with the exception of that recommended by Dr. Tilt, and that of injecting the emptied sac with iodine, he had come to the conclusion that ovariectomy was a justifiable operation in certain cases. He thought that recoveries from that procedure would become more numerous as our knowledge respecting the disease improved. The diagnosis was no doubt a difficult one in some instances, and much neglected by clinical teachers. He looked for a better state of things. One of the most important points in the diagnosis was to determine whether the disease was a merely local one, or connected with such a contamination of the blood as to render it malignant. In the second case recorded by Mr. Childs, he augured badly of the operation from the appearance of the patient, which was that of a person affected with malignant disease; but, under all the circumstances of the case, he had considered the operation quite justifiable. He agreed with Mr. Childs on the importance of not delaying the operation until the patient was in an exhausted state. In fatal cases after operation, he had usually found the kidneys to be diseased. It augured well for the operation that Mr. Fergusson, at one time so warmly opposed to it, should, in the last edition of his *Practical Surgery*, have observed, that in "well-selected cases it was a proceeding to be recommended."

Dr. MURPHY, after making some remarks on the fallacy occasionally following data drawn from statistics, considered that correct knowledge of the disease

and operation under consideration must be derived from the careful study of individual cases. In illustration of this, he related three cases. The first was that of an old lady, sixty-five years of age, who suffered from an ovarian tumour, which was partly solid and partly fluid. The risks of the operation were explained to her, and she consented to its performance. Mr. Erichsen accordingly removed the tumour, and the patient got quite well. In opposition again to this, a case had come under his (Dr. Murphy's) care, of a young lady, aged eighteen, who suffered from an ovarian cyst of a simple character. He put her under treatment by pressure, after drawing off the fluid, and she got quite well, no return of the fluid being perceptible at the end of six months. In a third case, the adhesions appeared to be so firm that operative procedure seemed to be hopeless. In this case, however, the tumour burst into the vagina, and had since gradually disappeared. He regarded the operation of ovariectomy as more analogous to that for aneurism than amputation, in the former of which the patient might at any time die suddenly from the disease, and a dangerous operation only could save the patient.

Mr. PILCHER could not regard the second case mentioned by Dr. Murphy as perfectly cured; sufficient time had not yet elapsed to enable us to form an opinion upon this point. The statistics of the operation were most unsatisfactory; for even those cases recorded as successful, he feared in too many instances were the reverse. In the practice of a gentleman eminent for this operation, he (Mr. Pilcher) had heard of three cases which had been regarded as successful, but which had proved fatal from the effects of the operation. In one case, a lady had enjoyed life for two years after the operation, but died at the end of that time from chronic abscess occurring in the base of the broad ligament. In a second case, a lady married after the operation, but died suddenly from causes traceable to the operation. In the third case, no reason had been assigned for the death, but it was fair to presume that it was not independent of the operation.—*Lancet*, April 15, 1854.

51. *Treatment of Ovarian Dropsy by Injections of Iodine into the Cysts.*—Professor SIMPSON read the following paper before the Edinburgh Obstetrical Society:—

It has been often proposed to treat dropsy of the ovary upon the same principles as hydrocele or dropsy of the tunica vaginalis. In accordance with this view, Drs. Hamilton, Scudamore, and others, have in former times injected ovarian cysts with irritating solutions of sulphate of zinc, &c.; but the results have in general proved so unfortunate and disastrous as to prevent a repetition of the practice.

In 1832, Mr. Martin first recommended the use of tincture of iodine as the surest and safest injection for the cure of hydrocele; and this drug seems now almost universally adopted by surgeons in the obliterative treatment of this variety of local dropsy in the male subject.

Latterly, various surgeons, particularly Velpeau, Bonnet, Belluerimi, &c., have extended the practice of iodine injections to the treatment of other local dropsies and cysts; to chronic abscesses; diseases of the joints, &c. And the past experience of surgeons on the subject would certainly seem to show that while the local and direct application of iodine to morbid secreting surfaces has a great power of modifying, altering, and arresting even the secretory action of these surfaces, and often changes suppurative into adhesive inflammation, it shows at the same time wonderfully little aptitude to excite any excess of local irritation and pain. Hence naturally arose the question whether it could be safely and successfully injected into such large cysts as those of the common form of dropsical ovary.

In 1846, Dr. Alison, of Indiana, recorded the history of a chronic case of ovarian dropsy that had been repeatedly tapped, and which he injected at last with a solution of iodine. Severe symptoms followed, but the ultimate result seems to have been favourable. In 1851, Dr. Simpson assisted Mr. Syme in injecting a cyst in the neighbourhood of the ovaries, but not a common cystic form ovary. The symptoms which ensued were those of considerable excite-

ment; but the original cyst apparently became obliterated. Another one in its vicinity has lately shown itself in this patient.

Within the last year, Dr. Simpson has, subsequently to tapping, injected into dropsical ovarian cysts the tincture of iodine in seven or eight cases. For this purpose he has employed the common tincture of iodine of the Edinburgh Pharmacopœia, undiluted. He has usually thrown into the cyst two or three ounces of the tincture. In some cases he has allowed a portion of the injected fluid to re-escape; in others has retained the whole of it in the sac of the cyst that was tapped. From these cases he drew the following conclusions:—

1. In none of the cases of ovarian dropsy treated with iodine injections after tapping has he yet seen any considerable amount of local pain follow the injection with one exception; in most instances no pain at all is felt; and in none has constitutional irritation or fever ensued. In the one exceptional case considerable local irritation followed, and the pulse rose to 110; but the same phenomena occurred in the same patient after previous tappings without iodine being used.

2. While the practice seems thus so far perfectly safe in itself, it has by no means proved always as successful as in hydrocele in preventing a reaccumulation of the dropsical fluid; for in several instances the effusion into the sac seems to have gone on as rapidly as after a simple tapping without iodine injection.

3. But, in two or three of the cases, the iodine injection appears to have quite arrested, for the time being, the progress of the disease, and to have produced obliteration of the tapped cyst, as there is no sign whatever of any reaccumulation, though several months have now elapsed since the date of the operation.

Lastly. Accumulated experience will be required to point out more precisely the special varieties of ovarian dropsy most likely to benefit from iodine injections, the proper times of operating, the quantities of the tincture to be injected, and other correlative points. Perhaps the want of success in some cases has arisen from an insufficient quantity of iodine being used, and from the whole interior of the cyst not being touched by it. The greatest advantage would of course be expected from it in the rare form of unilocular ovarian cysts. In the common compound cyst the largest or most preponderating cyst is usually alone opened in paracentesis; and though it were obliterated, it would not necessarily prevent some of the other smaller cysts from afterwards enlarging and developing into the usual aggravated form of the disease.—*Monthly Journ. Med. Sci.* May, 1854.

52. *Summary of the Statistics of some of the more Important Operations performed in the principal London Hospitals during the last six months of 1853.*—*Lithotrixy* would not appear to be in great favour with London surgeons, at least not in hospital practice; since, out of 32 patients presenting themselves with stone in the bladder, it has been performed in only 4 cases. Of these 4, two died and 2 recovered. The recoveries were in men of middle age, and in each case, after about two months' treatment and from five to six sittings, the entire calculus was considered to have been removed in both instances, and the patients, we believe, yet remain without relapse of symptoms. In one of the fatal cases, the man was aged 57, and death from cystitis and subsequent pyæmia followed the first operation. In the other, the man was aged 30; cystitis had followed the first operation; in the second, the bladder was injured, and death resulted a few days afterwards. In both, death was therefore fairly attributable to the operation.

Lithotomy has been performed on 28 patients, with the result of 22 recoveries and 6 deaths. The risks attendant on this operation vary so greatly with the age of the patient, that we may conveniently classify them with regard to that circumstance.

Boys under fourteen.—20 of the above 28 cases were boys, and of these 2 only died, exclusive of a third, who recovered from the operation but died of thoracic disease about two months after, and before he had left the hospital. The cases which died are the two youngest on the list, their ages respectively being six-

teen and twenty months. One of them died a few days after the operation, from inflammation of the pelvic cellular tissue. Difficulties had attended the attempt at extraction, and the calculus had not been removed, since in consequence of the staff slipping from its place, the bladder had not been entered. It is worthy of note that the fatal event had no connection with the too free incision of the prostate, since the latter had only been notched in its anterior margin. The second case died without known cause about 48 hours after a successful operation. The infant was found to have granular degeneration of the kidneys, and it was believed that the chloroform had exerted an evil influence.

Above the age of fourteen.—8 cases, with two exceptions, all adult men, upwards of 50, and one of them aged 71; four of them died. A lad aged 18, from whom a calculus impacted in the urethra had been removed, died shortly afterwards, in consequence of previously existing and very advanced disease of the bladder, ureters, and kidneys. A man aged 25, very ill at the time of the operation, died on the tenth day from phlebitis and pyæmia. A man aged 64, from whom five stones had been removed, died afterwards from inflammation of the pelvic cellular tissue. A man, aged 71, from whom two stones had been removed, died of pelvic inflammation with peritonitis.

Operations for Hernia.—During the half year the operations for strangulated hernia have been 54 in number, and the fatality therefrom 16 or 1 in 3.4. We will examine first the evidence they afford as to the much debated question of sac-opening. Out of the above number the sac was opened in 35, with 11 deaths, or 1 in 3.2; it was not opened in 19, with a fatality of 5, or 1 in 3.8. We are not able to state with precision the duration of strangulation in quite all the cases, but of 29 of those in which the sac was opened its average was 59 hours; while of 17 of those in which the sac was left unopened, its average was but 42 hours. In one of the fatal cases in which the sac had not been opened death was from bronchitis, and not from the abdominal lesion; and in three of those in which it had been opened death was almost inevitable at the time of the operation, in one from gangrene of the bowel, and in two from internal strictures. It does not appear, therefore, that from the past six months' experience any conclusions can be deduced in favour of the modern practice of endeavouring to accomplish reduction without opening the sac; and there are several collateral circumstances which, if borne in mind, make this yet more apparent. It must be remembered, in the first place, that among those cases in which the sac was finally opened, are many in which the operator endeavoured to avoid doing so, and only adopted that expedient after more or less prolonged endeavours to reduce without had failed, and consequently the parts had undergone much unnecessary handling. Secondly, those selected for reduction without opening of the sac, have always been the more promising cases, it being accepted as a rule of practice, to examine the intestine whenever there is considered to be risk of its being gangrenous or inflamed past recovery. On such a mere dribble of statistical data as that afforded by 54 cases, it would, of course, be absurd to attempt to ground any positive conclusion, especially on so important a question in practical surgery. It must, however, be considered as to a certain extent neutralizing the deductions of the advocates of Petit's operation, since those deductions are not themselves grounded on very extended observations.¹

¹ In case an old edition (the fourth) of *Druitt's Surgeon's Vade Mecum* should be in the hands of any of our readers, we must take this opportunity of correcting a statement regarding Mr. Luke's experience of hernia operations which has found its way into that generally accurate work. It is to the effect, that out of forty cases operated on since Mr. Luke adopted Petit's method, only two had done badly; and is so palpably too good to be true, that the exposure of its fallacy is, perhaps, scarcely necessary. Mr. Luke's real statistics on this point are given in an excellent paper by himself in vol. xxxi. of the *Medico-Chirurgical Transactions*, and show (speaking roughly), in cases in which the sac had been opened, a fatality of 1 in 3.1; and in those in which it was not opened, a fatality of 1 in 8.4. Mr. Luke has kindly furnished us with a statement of the cases in which he has operated since the publication of that paper (up to the beginning of 1853), which are 47 in number. Out of the whole, 16, or 1 in 3, died; out of 21 in which the sac was opened 11, or more than one-half, died; while of 26 in which it was left unopened, only 5, or less than one-fifth, died.

There are other points in the prognosis of a hernia case probably of much more vital importance than the mere question as to whether or not the sac has been opened. Such, for instance, as the duration of strangulation, the tightness of the constriction, the amount and violence of the efforts at taxis previously resorted to, the size of the tumour, and the health of the patient. We regret that we are not able to make a strict analysis of the cases under consideration in respect to each of these several heads; we will do so, however, as regards the first and most important of them. The length of time which strangulation had endured before the operation, is known, as we have already stated, in 46 of the cases. Of these the period was less than 12 hours in 11 cases, only 2 of which ended in death, and in 1 of those the rupture was of enormous size, and several feet of bowel exposed during the reduction. Between 12 and 24 hours, there were 8 cases with 2 deaths. Between one and two days, there were 4 cases, with 1 death. Between two and three days, 1 case successful. Between three and four days, 7 cases, with 3 deaths. Between four and five days, 2 cases, both successful. Between five and six days, 2 cases, with 1 death. Between six and seven days, 2 cases, both of which died. These facts exhibit clearly enough the evil influence exerted by lengthened strangulation; and the lesson becomes yet more forcible, when it is recollected that, in many of those in which the period extends over several days, it is probable that the constriction was at first not very tight, while those in which operation was done early are those in which, from the occurrence of very severe symptoms at the outset, the sufferer was obliged at once to obtain efficient advice. It is impossible, perhaps, to express too strongly the importance of not losing time in cases of strangulated hernia. If moderate efforts at the taxis, made under chloroform, and after a warm bath, have failed, the loss of time involved in the trial of other expedients is attended by much greater risk of evil than promise of benefit.

Amputations.—The number of capital amputations performed during the period specified has amounted to 84, out of which there have been 63 recoveries and 21 deaths.

Of the Thigh.—21; recovered, 13; died, 8. Primary amputations after accident, 2, with 1 recovery and 1 death. Secondary amputations after accident, 4, with 1 recovery and 3 deaths. Amputations for diseased knee-joint, 13, with 9 recoveries and 4 deaths. Amputations for ulcerated leg and for painful stump, 2, 1 each; both recoveries.

Of the Leg.—26; recovered, 15; died, 11. Primary, 13, with 6 recoveries and 7 deaths; 4 of these were double amputations, all of which ended fatally. Secondary, 2, both of which recovered. For diseased ankle-joint, diseased tarsus, senile gangrene, or ulcerated leg, 11, with 7 recoveries and 4 deaths.

Of the Upper Extremity.—25, all of which recovered; 1 was through the scapula, 3 were at the shoulder-joint, 11 were through the upper arm, and 9 through the forearm.

Of the Hand or Foot.—12; of which 10 recovered and 2 died—both the latter of tetanus; 3 were amputations at the ankle-joint; 1 Chopart's operation; 1 tarso-metatarsal; and 1 of the entire foot, excepting the astragalus; 2 were at the wrist-joint; and 4 through the carpus, the greater part of the hand being removed. The cases which ended in tetanus were both primary amputations; one of them of part of the hand, and the other of the foot, at the astragaloscaphoid joint.

Among the causes of death, speaking of the amputations generally, we may notice the following: Shock of the operation in 4 cases, 3 of which were primary amputations of both legs; phlebitis and pyæmia, ascertained in 2 cases, and supposed in 3 others; exhaustion from long-continued discharge, etc., 4; secondary hemorrhage occurred in 5, and was the cause of death in 3. The difference in result of amputations performed on the upper and lower extremity is well illustrated by the above facts. Thus we have in the latter, taking only operations done below the knee, a fatality of 1 in 2.6, and among those on the upper extremity, although 4 of them were at or above the shoulder-joint, a fatality of only 1 in 31. This is probably in part a mere coincidence, and the disproportion might not be found so extreme on generalization of a larger number of facts.—*Med. Times and Gaz.* March 18, 1854.

MIDWIFERY.

53. *Double Uterus—Twins.* By M. HOHL.—A delicate woman, aged 30, *en-ciente* for the second time, became subject to slight and transitory hemorrhages during the first three months of pregnancy. At the beginning of the seventh month, hemorrhage returned, and recurred in fourteen days. Both sides of the abdomen were convex and distended, but there was depression along the mesial line, from the umbilicus to the pubes. Here percussion caused a tympanitic sound; on either side there was dullness. The uterus, which could be examined through the abdominal parietes, was divided into two parts, an inch and a half above the symphysis pubis, the right being larger than the left. Both were convex externally, concave internally, and presented the usual characters of the gravid uterus. The two cornua extended above the level of the umbilicus, and were sufficiently large for the hand to feel different parts of the fœtus; in one, the beatings of the fetal heart were heard. The vagina was normal; the neck of the uterus single, short, but larger than natural; it was about an inch wide. The two cornua could be felt superiorly opening into the cavity right and left. The orifice of the right cornu contained the placenta. Across the opening of the left cornu, the other placenta was felt. This, then, was a case of twins in a bicorned uterus. The two placentaë were implanted in the neck; one was disengaged, and the hemorrhage resulting from this disposition was sufficiently serious to call for interference.

M. Hohl determined promptly to bring on delivery. He introduced the hand first into the right uterus, seized the feet, and dragged away the child without difficulty; the placenta followed at once. He repeated the same experiment, successfully, on the opposite side. The two infants, who weighed about three pounds, died after a few inspirations.

The woman suffered from an attack of pleurisy, but she recovered in four weeks.

The accuracy of the diagnosis was verified subsequently by examination both with the hand and the uterine sound.—*Med. Times and Gaz.* April 22, 1854, from *Prager Vierteljahrschr.* 1853. Bd. 4.

54. *Chloroform in Midwifery.* By S. W. J. MERRIMAN.—Mrs. Manning, aged 28, Irish, rather depressed from want of proper comforts, and from a harassing cough, was found by me about 3 A.M., Nov. 21, 1848, in strong labour with her fourth child, though only third pregnancy. The os uteri was fully dilated, the bag of waters half way down the vagina, and the head lying in the natural position (vertex to right acetabulum) above the pubes; the pains very frequent and severe, all indicative of a speedy termination to the labour.

The membranes broke almost immediately, but the head did not descend as I expected; it seemed as if the pains propelled it against the pubes, and not in the axis of the brim.

This condition having lasted an hour without any progress being made, though the patient's sufferings were very great, I requested the assistance of Mr., now Dr., Hawkesley, and submitted to him the question, whether we should administer chloroform, then recently much vaunted as a safe agent in parturition.

Both of us had strong feelings against its indiscriminate use; but it seemed to us that, if anæsthesia was ever to be induced with propriety in a natural labour, this was a fair case in which to try its efficacy. It would, we hoped, relieve the patient from those pains which agonized her so much, while the natural labour pains would proceed in their regular course, and shortly bring the child into the world. Dr. Hawkesley, therefore, administered the chloroform by a mouth-piece, closely resembling that used by Dr. Snow, and the patient speedily passed into the third stage of narcotism of that eminent physician. For a moment the pains ceased, but they presently returned with the same strength as previously, and forced down a scalp tumour, but did not propel the head itself, though it frequently appeared to descend a little. Moderate pres-

sure, applied at first by the hand, and afterwards by means of a binder, was used to direct the head more backwards, but ineffectually.

Two hours elapsed in this manner, at the end of which we determined to try the effect of ergot, in inducing a consentaneous action of the whole of the uterine fibres. Three successive doses of the infusion of ergot were therefore given, but, instead of an increase of pains, they subsided, and presently ceased altogether. At this time a pain, of which the patient had previously complained, appeared to be settled in the abdomen, over the fundus uteri. She had retchings from time to time, bringing up not more than a teaspoonful of dark fluid. She was exhausted, but there was no collapse; the pulse was quiet, and of a fair strength. We gave her forty minims of Battley's laudanum, and left her at half-past 8 A. M.

At half-past twelve noon, I saw her again; she had had no pains, but complained much of tenderness of the abdomen over the fundus uteri; the vomitings continued as before, but rather more fluid was thrown up; the pulse small but regular; still nothing which could be called collapse. I was somewhat alarmed at not being able to feel by the "toucher" any part of the child's head; but, as the case did not appear to be very urgent, and I had to attend some patients at a considerable distance, I called upon Dr. Hawkesley to request him to visit the poor woman in a short time. This, however, he was prevented from doing till nearly 5 P. M., when he gave her two grains of opium and two of calomel, and ordered her saline draughts, with tinct. opii $\mathfrak{m}\mathfrak{x}$, every two hours. I now returned, and, after consulting together, we applied to Dr. Chowne, the principal physician to the Charity, who very promptly attended, passed his hand into the uterus, which he found largely dilated, but not ruptured, seized the child by the feet, and completed the delivery (footling) at about 6 $\frac{1}{2}$ P. M. The placenta followed forthwith, and the uterus contracted firmly as its contents were withdrawn, but there was no attempt at any contractions to expel the child or the placenta.

My patient required very great attention on my part, and I am happy to say that by degrees she recovered sufficiently to go out. The details of this subsequent treatment are not necessary here, and are given at full length in *The Record*; suffice it to say, that it consisted mainly of salines with more or less laudanum in each dose, some calomel, and several enemata to act on the bowels, and dispel wind which distended her very much. The vomiting gave place to purging on the evening of the 24th, and an improved condition began to show itself in her about that time. On the 26th, milk appeared in the breasts; on the 29th, chalk and catechu became necessary to stay the diarrhoea, and shortly afterwards she had a tonic consisting of infusion of gentian with sal volatile.

"Jan. 11, 1849.—She has been twice down to the next floor, and talks of going out next week; feels a bearing down and pain in the left groin after standing a little, but this appears to be the effect of weakness; she has a little leucorrhœa, and reports a slight catamenial period a few days ago." About a year afterwards, I heard from a neighbour that Mrs. Manning had been placed in a lunatic asylum, but my informant could give me no other particulars.

The fact that chloroform can and does induce inertia of muscular fibre, both voluntary and involuntary; when given in a sufficient dose, is now well-established; I have, therefore, no hesitation in expressing my belief that the relaxation of the uterus in the case of Mrs. Manning was brought about through the inertia caused by deep inhalation of the chloroform, and I was anxious to bring the case before the Fellows of the Society, as a proof that chloroform may produce alarming results in midwifery practice, and that, if given at all, it should be in small quantities only. Time, however, did now allow any lengthened details, and I was obliged to be content with a very brief allusion to the case.

In my *Arguments Against the Indiscriminate Use of Chloroform in Midwifery*, published in July, 1848, I advocated its omission altogether, as in olden times, but said that I could conceive cases in which its use might be allowed. The case I have described seemed to afford an opportunity for testing the soundness of my conceptions, and the confidence which could be placed in the panegyrics bestowed upon its use by accoucheurs in various parts of Great Britain.

The result of my experiment I have described, and it was of so decided a character, that I have not used chloroform since, except in some cases of operation; and in several instances in which I have been compelled to use the forceps to deliver the child, the arrest having been at the outlet, I have performed the operation without any resort to anæsthesia, and I have never had cause to regret my indisposition to place my patient in what I must call an unnatural condition.

The more I hear and see of the use of chloroform in midwifery, the more I am convinced, that though it may occasionally be useful, and even desirable, in the small quantities now administered in London, in bringing about a relaxation of a rigid os uteri, as mentioned by Dr. Gream; or in larger doses to relax the rigid contraction round the body of a child impacted in the pelvis in an arm presentation, where turning must be accomplished, or the child be eviscerated, its administration is not desirable in ordinary cases; and that whenever it is administered, great attention should be paid to its effect on the sensorial functions, lest a serious injury should ensue to the patient, a caution which can only be properly carried out by the administration of the anæsthetic being confided to a second practitioner, who shall not take part in the superintendence of the actual birth.

P. S. Dr. Chowne has just informed me, that Mrs. Manning "devoted herself (after regaining her strength) to religion, fasting, etc.; that she appeared odd in her ways, but remained sane up to the end of four months. At the end of two months more, making six from her confinement, she was in a lunatic asylum."—*Med. Times and Gaz.* April 22, 1854.

55. *Cases of Hysteria and Spurious Pregnancy.*—Dr. ALEXANDER KEILLER related the following remarkable case at a meeting of the Medico-Chirurgical Society of Edinburgh, April 5, 1854. Dr. K. first saw the patient, Elizabeth Berry, on the 14th September, 1838, in Dundee. She was then nineteen years of age, and her symptoms were those of uterine derangement, with hysteria. The abdomen was tympanitic; pain was complained of in the right side; there were acid eructations; the bowels were confined, and the catamenia suppressed. On inquiry, it was found that she had always been a delicate child, and even in infancy had suffered from an enlarged belly. The catamenia appeared in her fifteenth year, and had become irregular, apparently in consequence of a severe bruise which she had received shortly after their appearance. He lost sight of her till about nine months afterwards, when he learned that she was considered to have been in labour for many days, and that she was attended by an unqualified practitioner, who regarded the case as one of such extreme difficulty, that the Cæsarean section was proposed as the only means of relief. On the 25th June, he was asked to see her, and rather to his surprise found that he could not detect any uterine tumour. The os was completely closed, and the usual mammary signs were wanting. The uterus was of the usual unimpregnated development, and of course the idea of pregnancy was at once dismissed from his mind. The abdomen, however, was enlarged, and tender to the touch. The pulse was 120, full and bounding. The girl was evidently in a state of nervous excitement. He ordered an opiate and turpentine enemata, etc. The abdomen continued to enlarge, and the nervous symptoms to increase in severity, notwithstanding the liberal use of narcotics and other remedies. The pain was not relieved, and her supposed labour screams disturbed the whole neighbourhood. On August 20, she commenced to vomit bloody mucus, which, from its accumulation, prevented sleeping and threatened suffocation. The menstrual discharge was noted to have appeared on the 15th September, but, as usual, to have been a mere show. The pulse was still 120. Four ounces of bloody mucus were passed from the mouth. On the 22d of the same month, she was carefully examined. When erect, the abdomen presented an enormous enlargement, but no fluctuation or enlarged organ could be detected. Its left side was larger than the right. There was marked spinal irritation, and the left lower extremity was found slightly contracted on the trunk. In the upright position the pulse was 160. The contraction of her limb increased in extent, and she soon began to vomit her food. All her symptoms, however, began to decline in severity in

March of the following year, 1839; and forty-eight weeks after the commencement of her illness, she was able to walk out a little in the open air. She was speedily reduced to her former state of helplessness in consequence of a sudden fright, from the averments of a quack that her belly contained a living viper. Again various plans of treatment were put in requisition without success, and change of air was insisted upon, which was always attended with marked relief to the general symptoms, though the size of the abdomen remained unchanged. She returned from one of these removals in Feb. 1841, so well, as to be able to resume her former duties at a mill: the abdomen was decreased in size, though still enlarged; the catamenia were irregular as before; and her aspect very hysterical. In a fortnight, her old symptoms reappeared, and compelled her again to take to bed. After a short stay in the Dundee Infirmary, she once more went in quest of change of air, and was not seen by Dr. Keiller till 1849, when she consulted him in Edinburgh on account of the threatened recurrence of her former complaints, her abdomen being very large and tympanitic. [During the eight years which had elapsed, she had, it seemed, become pregnant, and been delivered of a living child. Her abdomen continued so large during this her true pregnancy, that it had not been suspected till the child made its appearance.] She was sent into the Maternity Hospital on the 8th of June, 1849; and on the 10th, his attention was drawn to the supposed fact that she was passing *per urethram*, a liquid exactly resembling a mixture of urine and writing ink. The catheter was used, and pale urine was drawn off. On the 11th, she was examined by Dr. Simpson, who was inclined to refer a dulness in the left iliac fossa, observable on percussion, to the uterus having been tilted over. Chloroform was now administered (for the first time, Dr. Keiller believed, in the diagnosis of such a case), and the abdomen became quite flat. The uterus was ascertained to be of natural size, but the bladder was very large and distended with pale urine. On the effects of the chloroform passing off, the abdomen regained its former enlargement. The chloroform was again administered a few days after, and similar effects followed; the spinal column being easily traced through the abdominal parietes. She remained for a month under treatment of a varied character, and left for Dundee, but slightly, if at all, improved. About two years ago, he had again seen her; but, as since then she had escaped his notice, he could not at present give any account of her state of health.

In conclusion, Dr. Keiller stated that in a somewhat similar case at present under his care in the Royal Infirmary, chloroform had been attended with similar results, and that the abdominal tumour had also diminished during the action of galvanism. It was his intention on some future occasion to bring this interesting subject more fully before the Society.

Dr. W. T. Gairdner had perused an essay lately published by Mr. More O'Ferral, of Dublin, on *Abdominal Tympany*, in which a similar observation on the use of chloroform in the diagnosis of such cases was made, but with no mention of the previous investigations of Dr. Simpson. It was interesting, however, to find a collateral and independent testimony to the value of the agent.

The President said, that his own observations on the use of chloroform in the diagnosis of such cases would be found in the same journal, at least three or four years before the appearance of Mr. O'Ferral's paper. He believed that cases of spurious pregnancy were often met with in practice; for many a married lady would acknowledge, when questioned, that she had once or twice thought herself pregnant when, as the result showed, she was not. He had in his own practice seen several cases in which ladies had removed into town with their entire establishments, in the full belief of their approaching confinement, and where he had had the disagreeable duty of informing them that they were not in the family way. Shortly before his predecessor, Dr. Hamilton, died, one lady, in her anxiety to reach Edinburgh, had had the roads cleared of snow for a long series of miles, to enable her to accomplish the journey, which proved after all an useless one, as Dr. Hamilton pronounced her not pregnant. These were instances in which patients progressed onwards to near the full term of pregnancy, suffering all the usual symptoms and discomforts of that state. Sometimes phenomena exactly like those of labour came on at the full term. This

seemed to have occurred in Dr. Keiller's patient. Dr. Simpson had seen several analogous instances of spurious pregnancy terminating in spurious parturition. In one of the first cases of this kind which he had witnessed, and which occurred in the Maternity Hospital of Edinburgh, he had been suddenly called from lecture to see it; the case being reported by the house surgeon, a man of remarkable acuteness, to be one of placenta prævia, and requiring the operation of turning. The patient had the phenomena of labour present, with severe menorrhagia; but there was no child to turn, as she was not pregnant. Besides the case of pseudo-pregnancy in which, as in the above, the patient went on with the usual symptoms of pregnancy till near to or up to the full period of labour, there were other varieties of this curious morbid state. In some, for example, the affection lasted only for a few months: in others, they occasionally continued far beyond nine months, and became as it were chronic in their character. As to the symptoms themselves, they consisted of the presence of more or fewer of all the usual sympathetic symptoms of pregnancy, as swelling of the abdomen, nausea and sickness, a feeling of quickening and motion of the child, etc. Dr. Keiller had stated that the mammary signs were not well marked in his patient. Sometimes, however, they were; and he (Dr. Simpson) had sketches illustrative of this fact, executed by the patient herself during a state of spurious pregnancy, and in whom, in a subsequent veritable pregnancy, her first, the areolæ did not present a deeper tint than they had done during her pseudo-pregnancy. As to the sensations which had been described in such cases, they were very frequent, and sometimes there were true motory contractions in the abdominal walls. He saw, about a year ago, along with Drs. Moir and Weir, a patient who had been sent in from the country on account of a supposed difficulty in the delivery, the woman having (according to her statement) been in labour for three days. In this case, there were very strong motions visible in the abdominal walls; so much so that the husband, who was present during the consultation, declared, on their affirming that there was no child, that then there must be an animal inside his wife. Dr. Simpson was inclined to believe that the malady was connected with the ovary. In one aggravated case which had fallen under his observation, there was marked ovaritis, and the ovary subsequently suppurated. Again, it was observed that, although menstruation did occur in the cases which had been noticed, yet that it was much scantier than usual, and sometimes the catamenia were wanting for several months. It was well known that the complaint was not peculiar to the human female. Harvey had long ago remarked that, in hounds who were well fed, many of the phenomena of pregnancy, such as swelling of the abdomen, and the presence of milk in the mammæ, occurred both subsequently to unsuccessful sexual intercourse, and also frequently after seasons of heat, but during which there was no intercourse with the male: kittens and other young animals were frequently stolen by the animal, to make the semblance of a litter. The curious observation had also been made by Harvey, that the animals were liable to the diseases of bitches which had recently been delivered. Dr. Simpson stated some similar facts in the case of the domestic cow. The symptoms of spurious pregnancy often occurred in the virgin female dog. And he (Dr. Simpson) believed that, when some of the cases of the so called hysteria in unmarried females were inquired into, they would be found, as he thought he had repeatedly seen, to be really symptoms only of spurious pregnancy. Dr. Keiller had alluded to the retraction of the limb as having been a well marked symptom in his patient. He (Dr. Simpson) had seen a case many years before the introduction of chloroform, in which the symptom was a prominent one. The lady had been under treatment in Paris, and, on her return to Scotland, a surgeon examined her, and recognized, as he supposed, the presence of a large ovarian tumour—not an uncommon mistake, as, in the records of ovariectomy, six cases are detailed in which no tumour could be found after the abdomen was opened. Dr. Simpson was consulted, and, on percussing, he found the abdomen quite tympanitic, and of course negatived the tapping, and the presence of any ovarian tumour. The leg in this case was much drawn up, and he was informed that, while in Paris, the heel was for a time closely applied to the back of the neck. About two years ago, he saw a lady, along with Mr. Syme, whose limb was

strongly and permanently retracted, and presented the appearance of hip-joint disease. He forgot whether there was abdominal fulness in this case, or not. The appearances were so deceptive, that a practitioner had used the exploring needle for the purpose of evacuating matter. As soon as the patient was brought under the influence of chloroform, however, the limb was readily straightened for the first time for many long months, and the case was at once ascertained to be one of "hysterical disease" of the joint. With regard to the nature of the abdominal swelling in spurious pregnancy, he had tried various experiments to ascertain its cause, but in vain; and as yet he could come to no decided conclusion on the subject. It had been suggested that, while the patient was deeply under the influence of chloroform, the contained air escaped unobserved; but, in one very marked case in the hospital, he had a tube passed *per anum*, its nozzle being kept under water; but not a bubble of air escaped. The diaphragm he was inclined to suspect to be a chief agent in the production of the swelling. One patient who had a small fibrous tumour of the uterus, and a rounded tympanitic abdomen, could greatly diminish the prominence of the abdomen by drawing herself up, and, on allowing the diaphragm to fall, the fulness and the appearance of pregnancy returned. The chloroform, he believed, acted by relieving the muscles, diaphragmatic and abdominal, from the influence of reflex action, and permitting their relaxation. In one case, he (Dr. Simpson) had also seen the Cæsarean section proposed, though in somewhat different circumstances from the case of Dr. Keiller. The patient was dying from another disease, but spurious pregnancy was present; the patient was positive about the child being alive, and he had been asked to operate after the mother's death to save the child. On a vaginal examination, etc., the uterus was found small and unimpregnated. The notion of some living animal being contained within the abdomen, in such cases (as in Dr. Keiller's patient) was not unfrequent. On two or three occasions, such a belief had been expressed to himself. Dr. Keiller had classed the interesting case which he had communicated under the head of hysteria; but some of the cases to which he (Dr. Simpson) had alluded could scarcely be properly included under that designation. The phenomena were common to the females of our domestic quadrupeds, and in them would not be designated hysteria. Dr. Simpson suggested that a series of experiments should be made on the bitch during the occurrence of the anomalous pregnancy, to ascertain, by examination of the state of the ovaries and uterus, the true nature of the cause. He thought that some cases of membranous dysmenorrhœa might be referred to the same category as the one under consideration; for in some patients, besides the throwing off of a membranous structure or maternal decidua, the other constitutional symptoms were also present. In some cases, the similarity to true pregnancy was very marked even in special and minor details. In a case from Ayrshire, which he had lately seen along with Dr. Taylor, a peculiar eruption of intense prurigo was present, which the patient declared had hitherto only appeared when she was really pregnant. Dr. Simpson concluded by asking if any member could suggest a *rationale* of the occurrence of the abdominal swelling, or explain the peculiar effect produced by the inhalation of chloroform?

Dr. Matthews Duncan had lately seen a case where the explanation of abdominal swelling was completely made out. A young lady had been long in bad health; her chief symptoms were referable to the left hypogastric region, which was very tender, dull on percussion, and extremely distended. Called in consultation, he recommended the production of deep anæsthesia as a diagnostic aid. Long before the girl was insensible, he remarked the great anterior arching of the lumbar vertebræ, which, as insensibility came on, disappeared; the spine coming to touch the mattress, from which it was before far removed. At the same time (no doubt from relaxation of the diaphragm and anterior abdominal muscles), the abdominal distension disappeared, and also the left iliac fulness, to the great relief of her anxious friends.

Dr. Duncan thought it very important to distinguish cases of spurious pregnancy from cases of false pregnancy. Real cases of the former disease were common in the lower animals, but, he thought, not very common in woman. From these, where not only was there no deception, but the real existence of

many of the signs of pregnancy, should be carefully separated cases of false pregnancy. These last were of various kinds. In some, the female simply wished to deceive; in others, she was herself simply deceived, easily undeceived, and the phenomena found were explainable without supposing spurious pregnancy. In a third class of false pregnancies, we had a good example of the so called electro-biological state. The female's mind was impressed with an unshakable conviction that she was pregnant. He produced under this morbidly strong and erroneous conviction some of the symptoms of pregnancy; and these symptoms and that conviction were maintained in the characteristic electro-biological or Mesmeric style. This state might sometimes originate without discoverable cause, or be traceable to a distinct source, as, for example, to connection with a male—a circumstance which, as he just learned, occurred in Dr. Keiller's case, and might afford some clue to its pathology.

The President had made some experiments on the subject in the wards of the hospital, and had satisfactorily ascertained that, in the cases he had examined, the prominence of the abdomen did not depend upon any arching of the spine.

Dr. Keiller concurred with Dr. Simpson in reference to the agency of the spine. In the case at present under his care in the hospital, and to which he had alluded, particular attention had been paid to the position of the spine; and he could not detect any connection between it and the abdominal swelling. He believed, with Dr. Simpson, that the peculiar appearance was mainly due to the action of the diaphragm, and he had observed that patients labouring under the disease could voluntarily increase the size of their abdomen by a little effort. In reference to the case which he had just read, he begged to correct a slight error in the remarks of Dr. M. Duncan. The girl had had no connection with a man prior to her illness, but her friends, unable otherwise to account for her remarkable appearance, had violently attempted to impress her, while in a state of nervous excitement, with that belief.

Dr. Montgomery, of Dublin, in a note which he had lately received, stated that he had seen a number of cases of the kind, and that one somewhat similar to the one just read to the Society was recorded in his book *On the Signs, etc., of Pregnancy*, p. 173. Dr. Montgomery went on to say, that the "whole subject was full of wonders, almost justifying Harvey's strange idea that the conceptions of the brain and uterus were to a certain extent identical, and might change places!"—*Assoc. Med. Journ.* April 21, 1854.

56. *Rupture of an Ovarian Cyst during Pregnancy—Recovery.*—The following case of accidental rupture of an ovarian cyst occurring during pregnancy, and followed by spontaneous cure, was narrated to the Edinburgh Obstetrical Society by Dr. GIBSON, of Dundee. Mrs. L., a lady of very delicate constitution, and the mother of two children, but who, previous to my becoming her medical attendant, had miscarried repeatedly, and undergone at different times a long course of treatment by an eminent practitioner in one of our metropolitan cities, on account of chronic inflammatory ulceration of the uterine neck, aborted again on the 27th July, 1849, at the ninth or tenth week of pregnancy, and had flooded profusely before I could reach her residence some miles off in the country.

I found reason for believing this abortion to have been the result, like the others, of a renewal of the same chronic inflammatory mischief as formerly, and had accordingly to subject her to the appropriate treatment for some length of time, ere I had the satisfaction of seeing her restored to a fair measure of health and exemption from suffering. One very profuse menstruation which meanwhile occurred at the end of seven weeks from the time she miscarried, having appeared very materially to contribute to the relief of her uterine state, and to hasten on her amendment. When, in the month of October, and just at the period when the catamenia should again have made their appearance, she was suddenly seized, after a walk that fatigued her, with violent pain in the lower part of the back, affecting chiefly the left side at first, but soon stretching round to both hips, and attended with intense sickness at stomach and harassing frequency of calls to make water. After three or four days of this suffering, she found the abdomen had suddenly become enlarged at its lower part,

presenting a tolerably firm round tumour of about the size of an orange, situated towards the left iliac region, and sufficiently movable to admit of its being displaced and pushed about to a certain extent.

On the 30th of October, that is, fifteen or sixteen days after all this had commenced, I was requested by her husband for the first time to see her, the pain, nausea, and irritability of stomach having been meanwhile incessant. I then found that the tumour had increased in volume to about the size of a fist, was perfectly smooth on its surface, and of a well-defined globular form, still inclined towards the left iliac region, but readily movable; tender on pressure, which at the same time aggravated the pain and nausea, and attended with a constant distressing sense of internal stretching, distension, and weight.

Altogether it appeared to me to be an ovarian enlargement of one kind or another, though the suddenness of its origin, and the symptoms attending it, seemed at the moment opposed to that view; while my patient's own conviction was strong of her pregnancy, in spite of the entire novelty of her feelings and sufferings, and whatever the tumour might be.

Nor did a careful vaginal examination at this period throw much light on the matter, though I found the body of the uterus somewhat enlarged, the cervix elongated and somewhat voluminous, and the os so open as readily to admit the point of the finger, while they were all very tender on pressure. Only the fear of her pregnancy, to which the absence of the catamenia imparted some likelihood, deterred me from using the uterine sound.

By the end of November the tumour had attained, at first very rapidly, latterly more slowly, at least the size of two fists; was round as a melon, firm but somewhat elastic to the feel, and still perfectly movable from side to side, though it generally occupied exactly the mesial line as she lay on her back, and ascended about an inch beyond the umbilicus above; while below, it admitted of the fingers being thrust deep into the abdomen between it and the pubis.

The uterus also by this time had itself undergone a very notable increase in volume, its firm, round fundus being now easily discovered to have risen to a level with the brim of the pelvis; while on a vaginal examination, the cervix and os were found situated considerably higher than formerly, and the body of the organ was felt to have expanded so much as nearly to fill up the cavity.

On the whole, the hitherto perplexing ambiguities of the case seemed to be now clearing up. The fact of my patient's pregnancy was becoming more and more probable; while it was scarcely possible to suppose the tumour to be any other than an ovarian cyst, which the symptoms gave too much reason to fear had inflamed and might be undergoing a process of slow suppuration.

Meanwhile, the patient herself was now miserably dejected in spirits, got almost no sleep, was tormented with thirst night and day, and an increasingly obstinate state of the bowels; while the pulse was generally small, soft, and feeble, and she was for the most part very chilly, with occasional very transient flushes of heat. But as she resolutely declined being further subjected to treatment of one kind or another, and expressed her settled desire just to let things take their course, I now for some time in a great measure discontinued my professional visits, though I heard of her frequently, and, I confess, was under constant apprehension of the cyst sooner or later giving way into the abdominal cavity, and thus, as a matter of course, almost speedily causing her death.

Matters, nevertheless, continued essentially in the same state as before, till the beginning of February 1850, when all doubt of her pregnancy having, meanwhile, in due time been settled by the occurrence of quickening, my patient, in the act of suddenly starting up from the sofa, felt conscious of violently straining herself. The immediate effect of this was a great aggravation of all her previous suffering, which in a few hours reached a pitch of absolute agony, accompanied by extreme faintness and sickness at stomach. The following morning, however, her intense suffering abated, and that rather suddenly, when, from sheer exhaustion, she fell into a prolonged and sound sleep, from which she awoke much recruited, and in all respects better. It was now found that the tumour had quite disappeared, and that a great diminution had taken place in the size of the belly. Far, too, from any of the evils arising

which I had dreaded so long, her health from this time improved very speedily, and she went on in comparative comfort till the 27th of June, when I was summoned to attend her accouchement. Her labour proved perfectly easy and natural, and she gave birth to a very fine boy of the average size; not a trace being to be afterwards found of the tumour, nor a single untoward symptom occurring, while she had also so completely recruited her strength as to be able to continue nursing for the usual period.

Thus terminated what appears to me a case of very singular interest; spontaneous cure and recovery taking place under circumstances so apparently desperate as the rupture of a large ovarian cyst into the abdominal cavity, and that during pregnancy. For while it will not be doubted, I think, that it *was* by sudden rupture that the tumour in this case disappeared—the only way of accounting, in fact, for what occurred in the beginning of February, I satisfied myself, by the entire absence at the time or afterwards of any outward discharge, that it was into the sac of the peritoneum it had emptied itself, and not by venting its contents into any of the natural outlets with which the walls of the cyst might have contracted adhesions.—*Monthly Journ. Med. Sci.* May, 1854.

57. *On some of the Diseases included under the term "Prolapsus Uteri," their Diagnosis and Treatment.*—Dr. SNOW BECK read before the Medical Society of London, April 1, a paper on this subject. The author first alluded to the practical importance of those diseases classed under the term "prolapsus uteri," and the acknowledged frequency of their occurrence. The division into "perfect prolapsus" and "imperfect prolapsus" was adopted as being the best suited to the short limits of the paper, which precluded him from entering further into the subject than merely sketching some of the chief points. On the subject of perfect prolapsus, he gave the definitions and descriptions of some of the most esteemed authors, to show the disease meant by the term; thus: "A tumour, often very large, hanging out between the thighs, and the vagina turned inside out, constitutes the external covering. In the sac thus formed, especially if of long standing and large, there is contained the bladder, rectum, and some portion of the small intestines, the mesentery being stretched, and the omentum occupying any vacant space." Could such a tumour fairly be called a prolapsus of the uterus?—and, in reply, the author stated he had examined the physical characters of a similar protrusion, which were found to answer to those of large hernial protrusions in other parts of the abdomen. He then put and discussed the question as to the name which ought to be applied to such a tumour occurring in any other part of the walls of the abdomen, which led him to the conclusion that the diseases termed prolapsus of the uterus were in fact hernial protrusions, occurring through the vaginal outlet of the pelvis, the uterus being a portion only of the contents of the sac. This position was further strengthened by pointing out that the annoyance arising from these protrusions arose only from the mechanical impediment to progression, which rendered their analogy to hernia complete. On considering the subject of imperfect prolapsus, he again quoted the symptoms given by the same authors, to show the diseases implied by the term. The chief symptoms were found to be a sensation of fulness in the pelvis, weight, and bearing-down, dragging from the loins and umbilicus, more or less pain in the back, extending round the groin, great distress from attempting to stand or walk, which was much worse in the evening and in the morning, and more or less vaginal discharge; these symptoms being attended with much constitutional disturbance, and ending in "a broken constitution." These symptoms differed so essentially from those attributed to perfect prolapsus, that they could not be considered to apply to the same diseases, differing only in the degree of the displacement. On further analysis, the symptoms of imperfect prolapsus were shown to be chiefly arising from those inflammations of the vagina, which had been much overlooked in treating of the diseases of females. The author then drew the practical deductions—(a) that, contrary to the received opinions, displacements of the healthy uterus are not followed by any notable inconvenience to the female; (b) that, when symptoms arise, they are

the consequence of some inflammatory affections of the uterine organs, which constitute the essential disease, the displacement being only an accidental accompaniment; (c) that the diseases termed perfect prolapsus were really hernial protrusions, occurring through the vaginal outlet; and (d), that those included in the denomination imperfect prolapsus, were inflammatory affections of the uterine organs, and chiefly of the vagina. These distinctions were pointed out as very important in regard to the treatment, inasmuch as the hernial protrusions (perfect prolapsus) required, as in other similar cases, mechanical contrivances for their support; whilst in the inflammatory affections the same mechanical means were actually injurious, the proper treatment being that calculated to reduce the inflammation. The various means employed in giving support to those hernial protrusions were glanced at, their principles of action pointed out, as well as the objections to each. The inefficiency of all kinds of support introduced into the vagina was shown to be practically acknowledged by their being very seldom employed in the present day; whilst the proper means of supporting an hernial protrusion (complete prolapsus) was considered to be by pads properly applied to the perinæum, and efficiently retained there. Great stress was laid upon these pads being adapted to each individual case, as in the examples of hernia occurring in other situations; for, from a want of a similar precaution, this method of treatment had fallen into considerable disrepute. The various operations recommended for the relief of these affections were further considered as inapplicable, or only of benefit in exceptional cases. In the treatment of these inflammatory affections, included under the term incomplete prolapsus, all mechanical interference was pointed out as being injurious, whilst the proper means of relief consisted in removing the inflammation present, by general or local remedies, as each case might require.

Mr. BROWN was somewhat surprised that the author had not alluded to the affections which were commonly mistaken for prolapsus of the uterus, such as the prolapsus of the bladder or rectum, bringing down the wall of the vagina before them. He regarded prolapsus uteri to be dependent on three causes—first, when the uterus, being congested and enlarged, fell into the vagina from its own weight; secondly, when, from repeated childbearing, the walls of the vagina had become so relaxed that they did not give the uterus sufficient support; and, thirdly, when, from a portion of the perinæum or of its muscles being lost, the base of the vagina was not supported, and the uterus fell. He differed from the author of the paper, respecting what he called “partial” and “complete” prolapsus, and regarded the terms used by Dr. Blundell as clearer and better, prolapsus having been defined by him as a falling of the womb within the vagina; procidentia, when the uterus was extruded from that passage. He agreed with the author in condemning mechanical support, such as a pessary, in cases of prolapsus, as by this means the vagina became dilated, and the original cause of the displacement was made worse. The uterus might be prevented falling, it was true, but ulceration, sloughing, or other mischief might result. Support given to the perinæum by a properly-adjusted bandage in mild cases, and the plastic operation on that region, which he had recommended in more severe ones, he believed to be the best means of treatment. Mr. Brown then referred to several cases of vagina-rectocele, and prolapsus of the bladder in which this operation had been successful. Cases of hernia, such as those referred to in the paper, were, he believed, very uncommon, and not likely to be mistaken for prolapsus of the uterus.—*Lancet*, April 8, 1854.

58. *Vaginal Cystocele mistaken for Prolapsus Uteri—Operation—Cure.*—Mr. J. B. BROWN read before the Medical Society of London (April 1, 1854) the following example of this:—

Mrs. L., aged 29, consulted me in May, 1853. She had delicate features, fair hair, pale complexion, slightly tinged with red, light eyes, small, frequent pulse. She was easily excited by mental emotions, was liable to periodical disorders of the digestive organs, and somewhat inclined to melancholia, denoting the nervous and bilious temperaments combined.

She stated that she was married five years ago, nine months after which she

conceived, and that nearly immediately afterwards she began to suffer from nausea and retching; these distressing symptoms continued for the first three months; they ceased for the next three months, at the end of which time they returned, and continued up to the time of her confinement. Immediately on the commencement of the second attack, she began to feel a heavy bearing-down pain, accompanied with difficulty of micturition and slight protrusion of a tumour from the vagina, gradually increasing in size and severity as her pregnancy advanced, totally incapacitating her from going about, or standing for more than half an hour together; in fact, to use her own words, she was obliged, while washing the family linen, to sit down five or six times, so totally unable was she to stand; she was obliged to wear a napkin constantly, which caused chafing and other serious inconveniences. She continued in this wretched condition till after her accouchement, when she applied for relief to her medical attendant, who informed her that she had a prolapsus of the womb, for which there was no cure. This state of affairs was allowed to remain for upwards of four years, when, on becoming pregnant for the second time, she applied to me. I placed her under the care of my friend Mr. Moullin for her confinement, which took place on August the 3d.

Mr. Moullin says, in his notes of her case, "that he was sent for at one o'clock A. M. in great haste. On his arrival, he perceived that labour had commenced, and that the pains were becoming rather frequent; and, on examination, he found a large tumour, nearly the size of a newly-born child's head, completely out of the vagina, giving, at first sight and feel, the idea that the head was expelled. This tumour proved to be the bladder distended with urine. The head of the child was low down in the pelvis, and, pressing against the bladder, completely prevented micturition, for which there was great desire. The catheter was used, and upwards of a pint of urine drawn off; the empty bladder pushed back as far as possible, thereby allowing the head to descend, when the labour was completed in a few minutes."

Mr. Moullin observes, "he feels perfectly convinced, that, had the case been mistaken, or relief not immediately afforded, the bursting of the bladder must have been the inevitable consequence."

Everything proceeded favourably, no other untoward symptom appearing to prevent a speedy recovery. She now became anxious that something should be done to insure permanent relief from her former state of suffering.

Accordingly, on September 5, while she was under the influence of chloroform, I performed my usual operation for vaginal cystocele, as described in the *Medical Times and Gazette*, April 16, 1853, with this addition, that, where the tumour projected rather more than usually into the vagina, I removed a portion of the vaginal mucous membrane, about an inch in length transversely, and about three-quarters of an inch antero-posteriorly, bringing the edges together by two interrupted sutures, and thereby considerably lessening the size of the tumour. The perinæum had been partially ruptured by her previous labour, which this operation entirely remedied. There was a small serous cyst, the size of a pigeon's egg, in the left labium, which gave some inconvenience in the preliminary steps of the operation. During the first three days, she had one of her customary bilious attacks, accompanied with hysteria, which soon yielded to ordinary treatment. On the fourth day, the quill sutures were removed, when the new perinæum was found perfectly united.

On the ninth day, all the other sutures were removed, and all the incisions found well united. The same plan of after-treatment was adopted as in the case already published, and with equal success.

March, 1854.—I have lately had an opportunity of examining this patient, and it is impossible to imagine a more perfect cure; her general health and spirits have also improved; indeed, she is quite well in every respect.

I have lately operated on another case very similar to this, of a lady who came home from India because she had been laid up five years for prolapsus of the womb, and in which the operation was equally successful. I also have had, in hospital and private practice, during the last twelve months, eight cases of either vaginal cystocele or vaginal rectocele, all mistaken for prolapsus uteri, and all have been cured by operation.—*Med. Times and Gaz.* April 8, 1854.

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

59. *Order of Succession in the Putrefactive Process.* By Dr. CASPER, of Berlin.—The following observations are drawn from the experience of many years by the above-mentioned author, and are of much interest in their medico-legal bearings. They do not comprise any cases in which the normal condition of the organs in question was altered by wounds or other injuries.

1. Of all the internal organs, the *larynx* and *trachea* are the first to exhibit the changes of decomposition. If the person have not died by asphyxia or laryngitis, the whole extent of the mucous membrane of the trachea will be found perfectly pale, while the body remains fresh, or exhibits a few green spots upon the abdomen. As soon, however, as this green colour begins to spread over the abdomen, but before any change has taken place in any of the other internal organs, the mucous membrane of the larynx and trachea will have become discoloured, acquiring a cherry-red colour, which gradually with the progress of decomposition settles into a dark brown. Of many hundred bodies examined, of persons of all ages, and with his attention directed particularly to this point, the author declares that he has never found a single exception to the rule here stated.

2. The *brain* in new-born children, and in those under two years of age, is often found soft and pulpy, while the body appears quite fresh; the decomposition of this organ follows very closely upon that of the tracheal mucous membrane. In the early stages of general putrefaction in young children, the brain will be found already completely dissolved, and when the head is opened, flows out as a pale-red, thin, gruel-like substance.

3. The *stomach*, although when compared with some organs, early attacked by the putrefactive process, resists it longer than those above mentioned. The first traces of it are seen in the greater curvature in the shape of irregular spots of a dirty red colour, through which generally the purple colour of the veins is seen. In doubtful cases of poisoning, this observation is of consequence, in order to avoid a hasty judgment. With the process of decomposition these spots become blended with each other, until the whole stomach is of an uniform colour. The author says that he has never observed, as a result of putrefaction, a separation of the mucous membrane from the muscular coat, as is seen after the ingestion of corrosive poisons.

4. The *intestines* come next in order to the stomach, in regard to the period at which the process of putrefaction shows itself in them; but it follows the same course in the one as in the other. Where this process has reached its highest degree—as for example in bodies which have lain for months in the water, the stomach and intestinal canal are of a blackish green colour, pasty consistence, and entirely disorganized.

5. The *spleen*, in the majority of cases, comes next in order, although in some cases, for reasons not well understood, this organ will exhibit marks of decomposition before either the stomach or intestines. It then becomes soft and easily broken, and, when a little more decomposed, has a bluish-green colour, is so soft that its substance can be scraped away with the handle of the scalpel.

6. *Mesentery.*

7. The *liver* is often found firm, fresh, and of its usual colour several weeks after death. Putrefaction begins upon its lower surface, and shows itself in a change of colour; the liver becoming green upon the surface, and then gray and black. At the same time the parenchyma becomes soft.

8. The *brain, in adults*, resists decomposition longer than the liver. The first traces of it are usually seen upon its basis, this surface becoming of a green colour, which gradually spreads and penetrates from the cortical into the medullary substance. The striking difference in the period at which the brain is attacked in children and in adults is explicable by the natural difference of consistence at the two ages. It is not until several months after death that the adult brain becomes fully reduced to that reddish paste which is seen at so early a period in the brains of children. Still, the organ becomes so soft-

ened in from two to three weeks after death, that a tolerably exact anatomical examination becomes impossible, a fact which is of serious importance in the case of bodies not discovered at an early period, and where death is supposed to have resulted from drowning or hanging.

9. The *heart* is slow to putrefy; it will often be found quite fresh when the intestinal canal is considerably advanced in putrefaction. It softens gradually, the *columnæ carnæ* first, and becomes brittle and of a greenish gray colour, like all the organs.

10. The *lungs* show signs of decomposition about the same time as the heart, sometimes sooner. In bodies which have already attained the highest degree of putrefaction, as seen in the separation of the cuticle, dark-green colour, strongly offensive odour, &c., the lungs will often be found perfectly well preserved. This undeniable fact forms a powerful argument against the theoretical objections made by some to the value of the hydrostatic lung-test. For when the lungs of a new-born child, whose body is still fresh, or only beginning to putrefy, are found to swim upon the surface of the water, the abstract objection that their buoyancy may be due to putrefaction, is contradicted by anatomical experience, which shows that the lungs are not attacked by putrefaction at such an early period. The first traces of this process in the lungs, is seen in vesicles from the size of a millet-seed to that of a bean, caused by the formation of gas under the pleura; these are so easy of recognition as to constitute a perfectly simple diagnostic sign of the occurrence of putrefaction, and to explain the cause of the buoyancy of the lungs. These vesicles are at first isolated and single; they gradually, however, increase in number until the whole lobe, and particularly the inferior surface of the lower lobe of each lung is thickly sown with them. With the progress of decomposition the lung becomes soft, of a dark colour, and loses its consistence.

11—14. The *kidneys*, *urinary bladder*, *œsophagus*, and *pancreas*, are said to succeed each other very nearly in the order in which they are here arranged.

15. The *uterus* resists putrefaction longer than any of the organs here mentioned. The importance of this fact in cases where pregnancy is alleged to have existed at the time of death, was illustrated in a case examined by Dr. Casper, in which, although the greater part of the body had been converted into adipose, and the bones were loosened from their attachment, the uterus was found perfectly fresh and firm. (For an abridgment of this case, *vid. Am. Journ. Med. Sci.* July, 1852, p. 269.) The same is true at the earliest age. It is not without importance even then, since, as the author observes, in Berlin, the bodies of new-born children which have been thrown into privy wells, sewers, or buried in cellars or gardens, are often not found for a long time after death, and when the organs generally are so much decomposed as to be no longer recognizable. In such cases the *uterus* was found in a state of preservation.—*Gerichtliche Leichen-Oeffnungen. Zweites Hundert*, Berlin, 1853.

60. *Suicidal Suffocation*.—An artillery soldier, under treatment in hospital, destroyed himself by thrusting a linen compress in his throat. He was under the close observation of a special attendant, having attempted two days before to commit suicide by throwing himself out of a window. Medical aid, although speedily obtained, was too late to be of use.—*Prag Vierteljahrset*, 1853, 2 Bd. p. 111.

61. *Mistaken Personal Identity*.—Dr. KINLOCK, of Drumoak, Aberdeenshire, relates a case of mistaken identity under extraordinary circumstances. The body of a man between sixty and seventy years of age, was found slightly imbedded in the sand, on the bank of a river; both eyes had been picked out by hooded crows, but decomposition had made no progress. The left ear, and the first finger of the left hand were wanting, having the appearance of having been lost in early life. The body was conveyed to a suitable place, and persons were requested by advertisements to come and identify it. After some time, two young women claimed it as the body of their father, who, they stated, was a lawyer; that he was in the habit of leaving home for two or three weeks at a time without informing them where he went, and that he had

lost the left ear and first finger of his left hand. They apparently recognized the clothes and the body, and gave vent to expressions of grief on the event. Subsequent doubts in the mind of one sister were overruled by the confident affirmations of the other. The funeral took place accordingly, and was attended by the daughters and friends of the supposed deceased lawyer. Returning from the funeral, the boatman of the ferry which they had to cross asked them for whom they were in mourning, and upon receiving their answer, laughingly informed them that he had, only half an hour before, ferried their father over alive and well, and directed them where they would find him; which, to their great joy, proved to be true. Whose was the body they had buried in the churchyard at Drumoak has not been discovered.—*Brit. and For. Med.-Chir. Rev.* from *Edinburgh Monthly Journal*, February, 1854.

62. *Recovery after taking a large Dose of Prussic Acid.*—Mr. W. H. BURNAM, of Wath-upon-Deerne, has communicated to the *Lancet* a very interesting history of the recovery of his father from accidental poisoning by prussic acid. The following abstract is from the *Brit. and For. Med.-Chir. Rev.*, April of this year. Mr. Burnam, Sr., took by mistake a drachm of Scheele's acid instead of diluted acid. In a few seconds, he perceived by the bottle the mistake he had made; he immediately swallowed half an ounce of aromatic spirit of ammonia, with a little water, and then called to his son and told him what had occurred. He spoke hurriedly, and breathed deeply. Mr. W. H. Burnam immediately administered some solution of crystals of sulphate of iron, trusting to the ammonia previously swallowed for the formation of an insoluble compound of the acid with the oxides of iron. This was two minutes after the poison had been swallowed; from this time, for twenty minutes, Mr. Burnam had no recollection of anything that was taking place. Respiration became deeper and slower. Four minutes after taking the poison, cold douche was freely employed, and more solution of sulphate of iron with spirits of ammonia administered. Vomiting took place; a slight convulsive shudder occurred; the cold effusion was persevered in, with the occasional administration of spirits of ammonia. In twenty minutes he began to exhibit signs of returning consciousness. In about fifteen minutes later, he was able to walk up stairs to bed. Perfect recovery took place. The patient was about sixty years of age, and of a strong constitution.

By chemical analysis, Mr. W. H. Burnam found that the quantity of the acid which his father had taken contained 2.4 grains of anhydrous acid. Mr. Burnam observes, that this is the largest recorded quantity taken, and followed by recovery. It is also a matter of interest in this case, that the time at which insensibility came on is so exactly known, viz. two minutes after the poison was swallowed.

63. *Upon the Legal and Social Rights of Malformed Beings.*—Dr. J. BIERBAUM states that all imperfectly developed beings may be divided into monstrosities and the malformed. Under the term monstrosity, the author understands only those who cannot maintain individual existence when separated from the mother, and whose death is therefore occasioned by birth. The malformed, on the other hand, can enter upon life in spite of their imperfect development, even although the term of existence should not be long. Lawyers make a distinction between monsters and partenta, or ostenta. The former are born of a woman, but possess no human head, rather that of some lower animal, and hence are regarded as scarcely human. Portenta, or ostenta, are those having, indeed, a human head, but imperfectly developed or deformed. To this class belong all those beings with badly formed organs of generation, or with an abnormal number of limbs. Such beings acquire not only the rights which belong to men, but entail, when they die, the same rights upon others. The first glance will show that such an arrangement leads to fallacies and difficult questions. Every sort of monstrosity has a right to baptism if it lives. The canon law which deprives monsters, the produce of intercourse with beasts (!), of baptism, admits of the following formula in doubtful cases: "Si tu es homo te baptizo." When twins are united together by some bond of attachment,

either they have each an individual existence, or one is dependent for life upon the other. Both are entitled to baptism. The canon law directs, in the second baptism, the use of the words, "If you be not already baptized, I now baptize you," &c. The right to nourishment and support belongs to all monstrosities as long as they live. To take away the faint expression of life may be a punishable offence, but cannot under any circumstances amount to murder. The right to be considered as belonging to this or to that sex comes into consideration at the period of baptism, education, doubtful paternity, the possibility of marriage. Cases are upon record in which both external and internal organs of generation were wanting. A complete hermaphrodite never existed; but there is often an imperfect condition of the sexual organs which renders the question difficult of decision. The Prussian law gives to the parents the right of determining the point as regards education; but after the age of eighteen the individual may form a separate conclusion.—*Med. Times and Gaz.* May 27, 1854, from *Henke's Zeitschrift*, xxxiv. 1, 1854.

MISCELLANEOUS.

64. *Meteorological Changes in relation to Epidemic Diseases.*—Dr. RICHARDSON read before the Epidemiological Society (March 6, 1854) a paper written by himself and Dr. MOFFATT on this subject. The authors opened the subject by referring to the labours of Hippocrates, Sydenham, Arbuthnot, and some other writers, and then passed on to consider,—1st. The general influence of the seasons; and, 2d. The connection that exists between epidemical diseases, and the following meteorological conditions—temperature, humidity of the atmosphere, electric conditions of the atmosphere, the force and directions of the wind, and the presence of agents supposed to be foreign to the atmosphere.

As the result of their labours, the authors have arrived at the following conclusions:—

1. That all those diseases which prevail at certain periods in a marked degree, and at other periods are as markedly absent, should be called epidemical; or, in other words, that the idea of the communicability of a disease should not alone and solely carry with it the idea of an epidemic disease.

2. That the influence of the seasons on all the epidemic diseases is so marked, that a series of laws bearing on this subject might easily be wrought out from observations sufficiently correct and extensive.

3. That, according to the observations of Dr. Moffat, the maximum of all diseases takes place when the wind is in the South, or equatorial, or ozone, points of the compass; and the minimum, when it is in the North, or polar points, or points where ozone is absent.

4. That the maximum of diseases for individual points of the compass occurs at N. W. and S. E.

5. That some diseases are almost peculiar to certain points of the compass.

6. That temperature does not seem to exert so much direct influence over diseases, as do the changes indicated by the oscillations of the barometer.

7. That ozone presents itself at certain periods, and, as it would seem, under the influence of particular laws; that certain diseases are peculiar to ozone periods, but that it need not be inferred that ozone gives rise, *per se*, to those diseases.

8. That the greatest number of diseases (according to Dr. Moffat's observations) take place with a medium degree of humidity of the atmosphere; and the greatest number of deaths with a minimum degree of humidity.

9. That the force of the horizontal movement of the wind would seem, from Mr. Richardson's present observations, to exert no influence on the spread of disease.

10. That, in order to arrive at precise results as to the effects of meteorological changes on epidemic diseases, observations both of meteorological pheno-

mena and of diseases ought to be steadily conducted at various points of the country, including areas of not more than twenty miles; and that the daily results thus obtained should be forwarded for arrangement to a public officer resident in London—a Registrar-General of Medico-Meteorological Observations.

Dr. Sibson said that there was one point in which he felt induced to differ from the conclusions arrived at by the authors of the interesting paper which had just been read, viz—as to the influence of temperature. The Registrar-General's Reports showed that this had more influence on mortality than all other meteorological changes put together. To take a single example; in the last week of 1853, the temperature was considerably below the average, and the deaths were 1,400; in the corresponding week of 1852, the temperature was eight or nine degrees above the average, and the deaths were only 900. The mortality was at its minimum, with a temperature of about 53° Fahr. It was lower when this average was reached during the spring months than during the autumn. He believed that, in fact, the laws of mortality would be found to hinge on the differences of temperature, which were themselves closely connected with changes in the winds. It must not be forgotten, however, in reading the Reports of the Registrar-General, that they refer only to *deaths* from disease, and do not give us any information with respect to the prevalence of the diseases themselves. In connection with meteorological observations, it was important to have accurate information concerning the position and physical geography of the places where the observations were taken. The character of the winds, for example, was much influenced by the position of the station where they were observed with respect to the coast. It would also be interesting to know the number of days in which each wind was observed. From some observations by the Astronomer-Royal, Professor Airey, it appeared that westerly and southwesterly winds were the commonest in this country, while those during the prevalence of which the mortality was apparently higher—viz., northwest and southeast—were much less common. This would make it more difficult to obtain correct averages. Probably, however, a different series of winds was prevalent in that part of the kingdom where the observations had been made on which this paper was founded; and, indeed, on referring to the table of the number of days on which each wind was noticed, he found that the northwest winds were commonest, then S. W., S., and S. E.

Dr. Snow said that during the last few months southeast and northwest winds had been very prevalent. He saw nothing in this paper contradictory to the reports of the Registrar-General; it was well known that the number of deaths from a disease was not regulated by the mere numerical prevalence of that disease; and that the same weather which was bracing and congenial to the young and robust was fatal to the old and infirm.

Dr. Gavin Milroy observed that from these tables, it was very evident that the diseases most affected by changes in the seasons were cholera, diarrhoea, and dysentery, which were all much more prevalent in the autumn; the same was true of yellow fever in the West Indies and at Gibraltar. It had been observed in Jamaica, and, indeed, over the whole world, that before the outbreak of cholera, the atmosphere had been remarkably stagnant, and when winds did at length come, they blew from unusual quarters; the rains, too, had been heavier than in ordinary years.

Mr. Richardson said that it was not intended to deny the great indirect influence of change of temperature with which many other meteorological changes were intimately connected. A very full account of the geology of the district where the observations had been made was given by Dr. Moffat in the *Athenæum*, during the course of last year.—*Med. Times & Gaz.* March 18, 1854.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

A Case of Labour, in which the Rupture of the Membranes, and Reclosure of the Os Uteri occurred ten hours after its commencement. By JOSEPH PARRISH, M. D.—On the 16th of May last, I was requested to see a lady in her fourth confinement. I had visited her several times during the month or two previously, and discovered nothing unusual in her symptoms. She is a lady of refinement, of intellectual and physical development—strongly sensitive to mental emotion and bodily pain. On the day of her lying-in, my first visit was about six o'clock in the morning. The pains were short, sharp, and distinct. On examination by the touch, the os uteri was found but slightly dilated. I encouraged my patient to be cheerful and hopeful, and left her for a few hours. At 9 o'clock, the os was found to be dilated still more, and the membranes occupying the space embraced by its circumference. The pains were more severe, and confined principally to the pubic region. At 3 P. M. the dilatation had progressed, perhaps to the extent of an inch and a half, in the diameter of the os uteri; the vaginal walls were well relaxed, and abundantly supplied with their mucous secretion. My patient had taken ether in her last labour, under the direction of the late lamented Dr. Samuel McClellan, and desired very much to have it again. Finding no contraindication to its use, present in her case, and having frequently administered it with advantage in other cases, she commenced the inhalation of it, a little after three o'clock, by means of a funnel, or nest-shaped towel applied to the mouth and nostrils. At this time, all that seemed necessary to afford speedy relief, was the relaxation of the rigid os uteri, the pelvis being ample, and the vagina well lubricated and distended; hence, I was led to anticipate a favourable termination of the case soon after the complete yielding of the system to the anæsthetic influence of the ether. The moment anæsthetization was accomplished, a strong uterine effort commenced, accompanied by the discharge of the liquor amnii. Hearing the waters gush from the womb, I made an examination immediately, expecting to find that the constricted neck had given way, and that the head was already engaging in the inferior strait. But, to my surprise, I could neither find the presenting part of the fœtus, nor could I reach the margin of the womb. In vain did I attempt for several minutes to digitate its locality, and finding myself in a doubtful place, I determined to wait until nature might be kind enough to reveal to me this trick of her fancy, such as she had never shown me before.

The labour was now more energetic, and somewhat painful, though considerably modified by the ether; another examination revealed only a confirmation of the previous one; and as I could not “discover hidden things out of darkness,” I rested yet longer in the confidence that there would be a safe termination, for the uterine efforts seemed to be vigorous, and I could feel the muscular efforts of the little prisoner, as it was writhing beneath the abdominal integuments, in seeming resistance to the fitful throes of the organ that held it. Trying to impart to the patient a measure of the same kind of trust, without informing her further upon the subject, I waited and wondered, until

again pressing my finger to the extent of its capacity, there was felt just at its point what was supposed to be the closed os uteri. Still waiting and hoping, a few minutes more convinced me that with the sudden effort of the womb, that wrung the membranes so as to rupture them, the os uteri was closed by a strong spasmodic effort, and the presenting part of the fœtus driven back again into the body of the womb. Attributing this peculiar, and (to me) entirely novel phenomenon, to irregular action of the uterine fibres—to *clonic* spasm—I administered a teaspoonful of a camphorated solution of morphia, with a view of arresting the spasmus, and allowing the dilating and expulsive forces to be properly exercised. At this time the rigid and indurated os uteri was felt by the finger, closed and firm. In about half an hour, the rigidity gave way, the dilatation answered readily to every pain, and in less than half an hour more, a living, healthy child was born.

The peculiarities of this case are these: Ten hours were occupied with slow, yet constantly progressing dilatation. Immediately upon the presentation of the great relaxant, ether, the dilatation ceased, and the womb shut up its mouth. It remained closed nearly three hours, and then suddenly expanded, and allowed the child to be born. The danger in the case was, in the greater measure, on the side of the child; it was robbed of its liquor amnii, and was subjected, for several hours, to the irregular pressure of an irritable womb, causing not a little apprehension of its death. In taking a retrospect of the case, the suggestion is naturally presented to the mind, that there may have been a spontaneous evolution of the fœtus. If the head had been fairly engaged with the membranes, and the contained liquid within the circumference of the os, it might not have returned so readily, as the presenting portion seems to have done in the instance before us, upon the evacuation of the waters. Perhaps the shoulder, or some other portion of the trunk—or, it may have been the breech—presented, and was, with the sudden closure of the outlet, and by the same effort, turned upon itself, and the head brought down in the second position of the vertex. My obstetrical reading has not furnished a record of a similar example that I can call to mind; and my obstetrical practice has not given me the opportunity of making such observations as have marked the history of the one just narrated. The reason for making it public is simply to add what may be useful or interesting to the records of science.

Philadelphia, June, 1854.

Hemorrhage from the Bowels in a new-born Infant—Recovery—By W. B. YOUNG, M. D., of Middleton, Md.—I notice, in the January number of this Journal, the report of a fatal case of hemorrhage, from the bowels of a new-born infant; and as I have had a similar case, in which the result was favourable, and which I attribute to a different course of treatment, I am induced to offer an account of it.

On the 18th Nov., 1853, I was requested to attend Mrs. R., in her third confinement; her labour was easy and of less duration than her previous ones, and without any difficulty whatever. The child was born in the full term of utero-gestation, and was considerably smaller than its predecessors; it was a female, and although of a delicate appearance, was considered a healthy child. Its bowels were sufficiently active without medicine, and its dejections were of the usual appearance until the expiration of forty-eight hours from birth, when it had frequent and most profuse discharges of blood. The attendants supposed that there could not be less than a quart of blood discharged. I estimated less than that quantity, but could not determine precisely the

amount. The discharge of blood had been for twelve hours previous to my visit. The child's pulse was barely perceptible, and it presented an anemic appearance. I did not think it possible for it to live. I immediately ordered a weak brandy toddy and half a grain of prussiate of iron, every half hour until the evacuations presented a different appearance. In two hours after taking the first dose, the child had an evacuation without blood; the medicine was then given every hour for twelve hours; then once in six hours for two days, when it was not necessary to continue the iron, but the toddy was used for a week longer. The child is now a fine fat little girl. I considered the exudation of blood to be too high in the alimentary canal to be reached by enemata, and feared that they might, by constringing the lower bowels, distend the portion above with blood. Had the iron failed, I confess I should have been at a loss as to what treatment to pursue. But as it had never failed me in any hemorrhages, either active or passive, I felt certain if the bleeding could be arrested the iron would do it.

Tapeworm expelled by Infusion of Pumpkin Seeds. By D. LEASURE, M.D., of Newcastle, Pa.—Mary —, aged 28, unmarried, has been delicate all her life, and for fifteen years subject to severe cramping pains of the abdomen, accompanied sometimes by obstinate vomiting. About ten years since, she noticed that she passed portions of tapeworm, of lengths varying from a single joint up to many feet, and, if the statements of the patient and her mother are to be relied on, sometimes half filling an ordinary chamber-mug. Her mother had also, at an early period of her life, been a victim to a tapeworm, which had been expelled by a secret *vegetable remedy*, probably *male fern* given her by a worm doctor.

My attention was called to Mary's case some time in last February, while in attendance on her sister for another disease; but, from causes not necessary to mention, I did not prescribe till last week. I had intended to use the male fern or kousso, or both; but not having access to either of them in a fresh state, I determined to wait till they could be procured from Philadelphia. While thus waiting, I noticed in one of the journals a report of a case of *tænia* expelled by the use of emulsion of pumpkin-seeds. Curiosity, more than the expectation of success, prompted me to give it a trial. I directed a *pint* of the bruised seeds to be infused in three pints of soft boiling water, and left over night, the whole to be taken during the next day, the patient fasting in the mean time.

On the morning of the 9th of May, the patient commenced its use, and in the afternoon experienced the most violent cramps and pains in the bowels for several hours; and on the morning of the 10th she passed eleven feet of the parasite, including the head, as proved by observation under the microscope. The animal was entirely dead when voided from the bowel, and is a most beautiful specimen of a perfect *tænia*.

A Remarkable Case of General Emphysema. By E. K. BEAVER, M.D., of Worcester, Montgomery Co., Pa.—I was summoned to visit Mr. Snyder, æt. 61, on the second day of November, 1853. The messenger informed me that he had an apoplectic fit three days ago, and was now greatly swollen. When I reached the house, on placing my hand upon his wrist, I found a peculiar cracking sensation, which, by the least pressure, gave way. This utterly confounded me. By placing the tips of my fingers on one of his cheeks, which were enormously distended, the same sensation was perceptible, and by the least pressure all disappeared for a moment. The eyelids were greatly

distended, which, by renewed pressure, enabled him to see for a few seconds, when they refilled again. I found that this emphysema had extended as far down as the thighs, filling slightly the scrotum. The motion of all his limbs was natural; the tongue was protruded with some difficulty, on account of the stiffness of the surrounding parts; yet by no means paralytic; he complained of no pain, with the exception of great soreness in his right side. I examined it, and found a place the size of a breakfast-plate ecchymosed; very near in the centre of this, midway between the sternum and the spinal column, in the intercostal space, between the sixth and seventh ribs, a small spot, the size of a half dollar, was exceedingly tender, causing great pain by the least pressure; whilst the surrounding parts were normal in sensibility, with the exception of the cracking; pulse very irregular, and 100; breathing very difficult, ascribing it to a feeling of great pressure on his chest, seeming to be altogether voluntary; saying that he slept but little on this account, for the past two nights. I was told that he went up stairs three days previous, and, whilst in the act of pulling on his boot, he heard a peculiar crack, and a feeling as if something had given away, and then fell senseless on the bed before him; after laying half an hour, he slowly recovered; twelve hours afterwards this swelling commenced, first on the right breast, and then continuing to the extent indicated when I saw him. He had considerable cough. I gave him small doses of tinct. opii camph.

3d. Visited S.; he had slept but little last night; emphysema extending over every part of the body; cheeks and eyelids more distended than yesterday; has passed no urine since last evening, on account of the enormous distension of the scrotum and the surrounding areolar tissue pressing against the urethra; in order, if possible, to relieve him of this, I took a lancet and made punctures in the scrotum, when the air escaped with a noise which was audible anywhere in the room. I also punctured the eyelids, cheeks, arms, hands, neck, and breast, with the most favourable results, relieving him greatly of difficult respiration, and also enabling him to see and swallow with ease. Finding his pulse very irregular and weak, I gave him small doses of carb. ammon. at short intervals; ordered friction, with warm vinegar, to his spine.

4th. Feels much relieved; frequently pressed the renewed air out of the punctures which were made yesterday; had two stools last night, to which my attention was directed, being bloody, with traces of pus; had a few hours rest; is able to swallow with ease; appetite good; cough very severe by spells; pulse more regular and firmer; continued treatment.

5th. Had three watery stools last night, mixed with pus and blood, with griping pain previous to their passage; has good hopes of recovery; accumulation of air in the areolar tissue not near so great; slept some last night; pulse like yesterday; pain in his right side considerably lessened; asking me, very curiously, why such large quantities of gas escaped per ano, "reminding him of a blowing horn?" I was rather inexplicit in answering; increased the anodyne; recommended slight dry friction over the whole body; gave him iodid. potass. gr. ij, dissolved in comp. syrup sarsaparilla, three times a day.

I now saw him at irregular intervals for two weeks. Finding his lungs affected with tubercles, and his general appearance cadaverous, I placed him on cod-liver oil; encouraged nourishing diet; and, contrary to what I expected, he is entirely relieved of the emphysema, and says, "he feels as well as he did before the attack."

DOMESTIC SUMMARY.

Vesico-Vaginal Fistula of Seven Years' Duration cured in Thirteen Days.—Dr. J. MARION SIMS records (*New York Medical Times*, May, 1854), the following case of vesico-vaginal fistula cured by the method devised by him and described in the number of this Journal for January, 1854.

The subject of the case was the wife of a professional gentleman of South Carolina; the fistula resulted from a tedious labour, in which the child's head remained impacted in the inferior strait for twenty-four hours. "The incontinence of urine began about eight days after delivery, up to which time the catheter was occasionally called for. There was no water discharged during the twenty-four hours of impaction. Attempts were made to introduce the catheter, but without effect. The fistulous opening was, at first, large enough to admit easily a No. 9 catheter, but under repeated cauterizations, for upwards of seven years, it gradually diminished to about the size of a common probe. In April, 1845, four months after the reception of the injury, Mrs. H. was placed under the care of the distinguished Professor of Surgery in the Medical College of the State of South Carolina, who used the nitrate of silver, and then the actual cautery, till about the middle of August, but with no benefit. She then visited New York, and was placed under the care of Dr. Mott; the mention of whose name is sufficient guarantee that all was done for her that science and art could at that day suggest. After remaining in New York about three months, and submitting to the repeated application of the actual cautery, she returned home in the same hopeless condition. From May, 1845, to December, 1852, seven years and a half, the actual cautery was used, on an average, about every four or five weeks.

"Mrs. H. came under my care in February, 1853. The fistulous opening was just above the neck of the bladder, a little to the right of the mesial line, and altogether favourable for a successful operation. It ran diagonally through the walls of the bladder, thus forming a sort of sinuous canal of a valvular character. When lying on the side or sitting quietly, the urine escaped in very minute quantities, enough, however, to render life anything but pleasant or desirable. When lying on the back, it dripped very freely, indeed quite as rapidly as secreted; also while walking. But if, by perfect quietude on the side, the urine was retained till the bladder became unpleasantly distended, then she could walk without its involuntary discharge. The philosophy of this is plain enough. The accumulated urine, by its distension, acted mechanically on the narrow, diagonal, sinuous canal, compressing its sides, and thus closing it as a valve.

"The operation was performed on the 2d of February, 1853. The fistulous track was hooked up, and the vesical septum transfixed, when, by a gentle sweep of the bistoury, a transverse oval opening was made entirely through the coats of the bladder, large enough to admit the end of the index finger. Two silver sutures were passed, and secured by leaden clamps in the usual way, the patient put to bed, and the catheter applied—the whole operation not lasting over twenty minutes. The usual regimen to insure constipation was enjoined. The suture apparatus was removed on the thirteenth day; the cure being complete and permanent. In three days more, Mrs. H. was walking about the house; and in a week she made the journey home, some five hundred miles, without the least inconvenience."

Vesico-Vaginal Fistula successfully treated.—Dr. N. BOZEMAN, of Montgomery, Ala., relates (*New Orleans Medical and Surgical Journal*, May, 1854), an interesting case of vesico-vaginal fistula treated by the method devised by Dr. J. Marion Sims, formerly of Montgomery, and now of New York.

The subject of the case was a coloured woman, 30 years of age, the mother of eight children. Her health had always been good until the birth of her last child, in August, 1852. Her last labour continued for sixty hours when delivery

was effected by craniotomy. A few days afterwards it was discovered that the urine passed through the vagina.

Upon examination, in December, 1853, when the patient was admitted into Dr. B.'s infirmary, he found the following condition of parts: "Uterus retroverted and slightly prolapsed; vagina quite capacious, but very much altered in appearance by the abnormal position of the womb; its anterior wall only two and a half inches, while its posterior seemed to be of the usual length. Running vertically through the anterior and a portion of the posterior lip of the os uteri, was to be seen a cleft an inch or more in length. At the anterior extremity of this cleft was the fistulous opening, partially filled by a herniated condition of the mucous coat of the bladder. It was triangular in shape; its base resting against the neck of the womb, and its apex presenting forward and sufficiently large to admit the index finger. The consequences of such a disease are too well known to require a notice in this short report."

On the 11th of March, Dr. B. operated as follows: "The patient was placed upon a suitable table on her knees and elbows. The parts were now brought into view by the lever speculum and a reflected sunlight. The edges of the fistula, formed partly by the anterior lip of the os uteri, were then freshened by means of a delicate tenaculum and small scalpel. This being done, the next, and what I considered the most difficult step was to be taken, namely: applying the sutures and clamps in such a way as to effect perfect apposition, and thereby obtain union by the first intention. Here I had the neck of the womb, an unyielding tissue, to deal with; and to remedy this, it was necessary that the compensating parts should be so adjusted that the mechanical contrivance employed would not defeat the object sought to be obtained, by creating an undue amount of inflammation. From the nature of the parts, therefore, I determined to place the clamps longitudinally binding their upper extremities so as to be applied one on each side of the anterior lip of the os uteri, thus enabling me to secure the upper suture in the most advantageous way. The clamps were one inch in length, and contained four openings for the sutures. These were of silver wire, and were lodged one after another in their respective places, by being attached to the end of a silk thread carried through at a proper distance from the edges of the fistula, by a short, straight, and spear-pointed needle. A clamp was now secured to the distal extremities of the wire and made to occupy its place. On the proximal ends, the other clamp was passed down to its place. Traction then being made upon these ends of the wire, perfect apposition of the denuded edges was effected. This was now maintained by compressing with a pair of strong forceps, a small perforated shot previously slid down upon each wire to the clamp. After this, the wire was cut off close to the shot, and the patient put to bed. The self-retaining catheter was next introduced into the bladder and there allowed to remain, excepting when its removal became necessary to clear the mucous and earthy deposit from it. This was required twice a day. During the whole treatment the patient was kept upon her back, and the bowels prevented from acting by the free use of opiates. On the fifteenth day I removed the suture apparatus, and was most happy to find union of the parts perfect. The patient now has entire control over her urine, and says she feels as well in this respect as she ever did."

Dr. B. very honourably remarks: "I take great pleasure in saying, that the success I have had in this case, I attribute entirely to the advantages derived from those principles of treatment laid down by Dr. Sims. Every surgeon who has or may make an application of them in the treatment of such cases as the above, must feel a sense of pride in awarding to him the praise he so justly deserves."

Varicocele successfully treated.—Dr. JURIAH HARRISS, of Augusta, Ga., relates (*Southern Med. and Surg. Journ.* April, 1854) the following case of varicocele successfully treated by the method of Ricord.

The subject of the case was a free mulatto boy, whose left testicle was very pendant and large; the tumour was reducible by pressure, unresisting, and compressible, and feeling like so many "earth-worms in a sack." Dr. H. stated

to him the nature of the affection; and upon his informing him that it was very much in his way, and was a very serious inconvenience to him in his daily labour, which was that of a mason, although he wore a suspensory bandage—and that he wished very much to marry, but could not get his consent so long as he had this uncomely appendage—and, finally, as it had evidently affected his morale, an operation was advised. “I explained to him the nature of the operation (for he was very intelligent) and the risks he had to run. He decided on being operated upon the following morning, and insisted that it should be done immediately. With the kind and efficient aid of Drs. Rossignol, Mackie, and Barry, I operated upon the 24th. The process I adopted was that of M. Ricord. The bundle of varicose veins was separated from the cord, and a needle, armed with a double ligature, was passed under the veins—a second needle, armed in like manner, was passed above the bundle of veins, through the same cutaneous openings as the first. This was done by letting the veins slip through the fingers, and making the second needle enter the opening of exit of the first, and pass out at the point of entrance made by the first needle. This left a free extremity of one ligature and a loop of the other upon each side. The free ends were passed successively through the loops, and drawn upon. This, of course, formed a subcutaneous ligature of the veins, without including a particle of skin. The free ends were then secured upon small pieces of gum catheter, which completed the operation.

“The pain induced by drawing the ligatures around the bundle of veins; lasted some fifteen or twenty minutes, when it passed away, and the patient suffered none afterwards, save a little soreness, which supervened upon the inflammatory action, set up to obliterate the vein.

“Dr. Mackie, who had the kindness to attend him during my temporary absence from town, informed me that he had little or no fever during the continuance of the ligature. About six days after the operation I saw him, at which time he had no fever, but the tunica vaginalis of the left side was largely distended with a serous effusion; but little tenderness of the part. We ordered the application of tinct. iodine, which removed the serum in three or four days. Owing to my absence, the ligatures were not removed until the eighteenth day. They could have been removed on the eleventh or twelfth day. During the whole of this time he was kept in bed. When I called to remove the ligature, the patient informed me that he had had, for some days past, venereal desires—the first for a long time. He was much more cheerful than before, and very grateful. The ligatures were removed without pain, which is one of the beauties of this operation. There was a large lump at the point at which the ligatures had been applied, probably the effusion of coagulable lymph, which blocked up the veins; below this point the veins were empty and felt like mere cords. He was ordered to wear a suspensory bandage for at least a month.

“I heard from him a short time since, when he said he felt better than he ever had, and could labour with much more ease to himself, than previously to the operation.”

Efficacy of Cod-Liver Oil in Consumption.—Prof. Wood remarked to the College of Physicians of Philadelphia that he had looked to the obituary tables accompanying, from year to year, the reports on meteorology and epidemics, with deep interest, in reference to an important therapeutical question—the efficacy of the cod-liver oil in the treatment of pulmonary consumption. The oil has been almost universally employed in this disease; and, during the first years after its introduction, a most striking effect was observed—the number of deaths from consumption diminishing surprisingly. Now, there appeared to be no other cause to which this diminution of deaths could be attributed, excepting the use of cod-liver oil. Still, Dr. Wood had been fearful of attributing too much to the influence of this agent, inasmuch as it was known to have the effect of postponing the fatal event—of prolonging without eradicating the disease—and hence, might cause the mortality from consumption to be thrown into future years. Dr. Wood had looked with some solicitude to the report for the past year, for a solution of this question; and he was happy to find the augmentation in the deaths from consumption, in 1853, no greater than is in-

licated by the report. This speaks very favourably for the remedial powers of cod-liver oil. There was to be anticipated an increase in the mortality from consumption during the past year, as the postponed mortality of the disease in former years would be thrown upon this. Hence, from a decrease in the proportion of deaths from consumption, since the period when it used to be between a sixth and a seventh of the whole mortality, we have a right to infer that we have gained something from the use of the oil in that disease; probably that we have cured by it one in every eight cases, with the anticipation of a still larger proportion hereafter.—*Summary of Trans. Coll. Phys. Philad.* vol. ii. N. S.; No. 4.

Lead Diseases treated by the Iodide of Potassium. Dr. H. S. SWIFT states (*New York Medical Times*, Feb. 1854) that the treatment of saturnine poisoning by iodide of potassium, has been tried in the New York Hospital in 23 cases, and with highly satisfactory results. "In 13 instances the urine was submitted to chemical analysis, and the investigation has established the fact that the lead may be eliminated from the system by the iodide of potassium, and found in the urine. In no case was the lead detected before the administration of the remedy. The chemical analyses were made by Prof. Outram, and the results of his experiments are perfectly reliable.

"All the patients began to improve rapidly after this treatment was adopted, though they had previously resisted the ordinary means. No bad effects resulted from the long-continued use of the remedy. In two cases, as M. Melsens suggests may occur, the symptoms were at first slightly aggravated—one of them was profusely salivated while under treatment, and the other slightly so. One patient also suffered from coryza and gastric disturbance for a few days; but the treatment was only suspended for a short time. One patient was under the influence of the iodide of potassium for six months, one for five and a half, and another for four months.

"In case 6, the urine was examined shortly after the treatment was commenced, and merely a trace of lead was detected. The quantity sensibly increased, until it was clearly shown both in the urine and saliva; and as the patient convalesced, it disappeared entirely, and the iodide of potassium was found abundantly in the saliva. In case 5, we did not suspect the existence of lead-poisoning until after the patient had been put upon treatment for constitutional syphilis. While under this treatment, a well-defined 'blue line' appeared upon the gums. The urine was then examined, and found to contain lead.

"Of the twenty-three cases treated by the iodide of potassium, sixteen have been discharged cured, and three so far relieved as to be able to resume their ordinary duties; four are still under treatment, and are gradually improving. Thirteen of the patients suffered from lead colic, complicated with neuralgia, arthralgia, &c., four had paralysis of the wrists, and in six the paralysis was general."

Membranous Croup successfully treated by Iodide of Potassium.—Dr. J. D. GRISCOM related to the Philadelphia College of Physicians (Feb. 1854) the following case:—

An intelligent boy two and a half years old, had been somewhat hoarse for several days, when symptoms of dyspnoea supervened, and Dr. G. was requested to see him. He found him with a pulse somewhat quickened, an anxious expression of countenance, a *suppressed, suffocative* cough, and a laboured respiration—noisy, but not resonant. During twenty-four hours these symptoms all slowly but steadily augmented, and left no doubt in the mind of Dr. G. that the little patient was passing through the incipient stage of membranous croup. Leeches, sinapisms, warm baths, and emetics (ipecac. and alum), had failed to give more than temporary relief. In looking over the resources of our art for a means of modifying the disposition to pseudo-membranous deposit in the larynx, iodide of potassium, the use of which, although without precedent in this disease, as far as Dr. G. was aware, was selected, and administered in doses of two and a half grains every three hours.

Prof. Wood now saw the child in consultation, and had no hesitation in confirming the diagnosis. When the exhibition of the salt was commenced, the child's condition was truly distressing. The chest was heaving, the nostrils expanded, the skin of the face assuming a dusky hue—the countenance bearing that imploring expression, which, when once seen in this disease, is seldom forgotten. On consultation, it was, with some hesitation, agreed to rely on the iodide of potassium another twelve hours. At the end of that time, the child had vomited what strongly resembled plastic lymph, and the onward progress of the symptoms was certainly arrested. A very slow improvement took place from this time. The remedy was continued about ten days, at the end of which time the little patient was entirely convalescent.—*Summary of Trans. Coll. Phys. Philad.* vol. ii. N. S.; No. 4.

Cold as an Anæsthetic Agent.—Dr. THOS. WOOD, of Cincinnati, states (*Western Lancet*, April, 1854), that he has used cold, as recommended by Mr. Arnott, of London, for preventing pain in surgical operations in various cases. In most of these it has met his expectations, but in others it entirely failed or but partially prevented suffering.

His experience has led him to the following conclusions:—

“The degree of cold required to destroy the sensibility of a part is but a little above that of the freezing point of water, and it must be obvious to all that this degree of temperature cannot be with impunity extended over a large surface of the body, or made to penetrate to a depth much below the surface in any of the more vital regions; hence its use is naturally restricted to the minor and more superficial operations, and can never take the place of chloroform or some of its kindred agents, in operations involving the deep tissues of the body.

“To operations upon the surface, such as removing small warts, tumours, and nævii, or other excrescences from the skin, it seems peculiarly adapted; and for destroying the pain in the extraction of diseased or offending nails from the fingers or toes, it is far preferable to chloroform. First, it is to be preferred for the reason that in its use there is no danger of fatal injury to the constitution, as in chloroform, for its effects are purely local and circumscribed; and, second, because the insensibility is more complete than is ordinarily obtained by chloroform, and is fully equal to the most overwhelming dose. I have repeatedly witnessed the most perfect composure of countenance in my patients, while a nail of the toe or finger was rudely torn, with a strong forceps, from its matrix, without the least exhibition of a sense of pain, or a consciousness of the progress of the operation, except from sight.

“It acts well on the skin, where a small portion is to be removed; but in plastic operations I would not use it, as the refrigeration necessary to remove the pain might so destroy the vitality of the flap, that direct union (upon which success in these operations wholly depends), would not take place.

“It has failed in my hands to be of any service in removing hemorrhoidal tumours, although, according to some of the European surgeons, it has answered well for them in operations on the anus.

“In one case I attempted to remove a string of venereal vegetations from around the verge of the rectum, but was unable to get the part sufficiently chilled to even lessen its sensibility, and I was finally obliged to use the chloroform before the operation was completed.

“The failure was, doubtless, owing to the rapid supply of heat to the part from the highly vascular organs in its vicinity, and the difficulty of conducting it away rapidly enough to reduce the temperature in the tumours to near the freezing point; and this difficulty, in all probability, will ever prevent its successful use in operations on the painful tumours of the anus and rectum.

“Cold has also failed to give much relief from the pain of opening paronychia.

“While it numbs the surface of the finger, it does not lessen the sensibility toward the bottom of the wound, even when the application is made a considerable time previous to the use of the bistoury.

“To chill a part that is to be subjected to an operation, for instance, a finger or toe, it is only necessary to get some pounded ice or snow, and mix with it

some common salt, and apply it, taking care to not extend it much beyond the region to be operated on. From three to four minutes will mostly suffice to remove all feeling from the attachments of a toe nail.

"It is no matter how rapidly the temperature is reduced, but after the operation, it is very essential that it should be raised cautiously and slowly up to the vital standard again. To effect this object safely, a towel or large cloth should be saturated with cold ice-water, and allowed to remain on the part until reaction is fully established.

Apparatus for Accommodation of the Eye to Distinct Vision at Different Distances.—Dr. VON ROTH read to the Biological Society of New York (March 24, 1854), the following description of this apparatus:—

"The motive apparatus of the lens really consists in two muscles, so arranged as to antagonize each other—which are constantly present in birds and the human species, and which, by their situation and attachments, are evidently intended to move the lens. They are called by Dr. Von Roth the *protrahens* and *retrahens lentis*. They are composed, in birds, of striped, and in the human species, of unstriped, muscular fibre.

"These muscles form two flattened bands, lying between the sclerotic and choroid, and extending circularly round the entire circumference of the membranes.

"The *protrahens lentis* occupies the space between the iris and the great circumference of the lens. It originates from the inner surface of the sclerotic, at its anterior edge, along the whole extent of the canal of Schlemm, and is inserted into that part of the choroid which is attached to the lens, as the 'ciliary body.' The existence and situation of this muscle has been long known; but a variety of opinions have been entertained with regard to its character and function, and it has been known accordingly under a variety of names. It is spoken of by Bock, Wilson, and others, as the 'ligamentum ciliare;' Brücke calls it the 'tensor choroidæ;' Todd and Bowman describe it as the 'ciliary muscle;' while Bochdalek, again, calls it the 'ciliary ganglion,' and denies altogether its muscular character.

"The *retrahens lentis* originates also from the sclerotic, somewhat further back than the commencement of the ciliary folds in the choroid, and, running forward, is inserted into the ciliary body opposite the insertion of the *protrahens*.

"These muscles may be most easily demonstrated in the following manner. The eye of any large bird is to be taken and divided by longitudinal sections, two lines or more apart, running through the whole thickness of the membranes. A section is then to be placed under water, the hyaloid membrane and retina removed, and the pigment brushed away from the choroid with a camel's hair pencil. The preparation is then to be fixed upon one of its edges, and the choroid drawn from behind forward, and made tense in this position. In this manner the *retrahens* is, with a little practice, very readily brought into view.

"In order to show the *protrahens*, the iris is to be separated from its attachment to the sclerotic, and drawn backward with the ciliary process; by which the *protrahens* muscle is stretched and well brought into view. If the choroid is now returned to its natural situation, it can be readily seen that the place where the two muscles, separated only by a narrow interval, are inserted into the ciliary body, is marked by a fine line. After the pigment has been entirely removed, this line can be seen on the inner surface of the choroid, encircling the ciliary body; and it is easy to make sure that the insertion of the muscles corresponds to the larger circumference of the lens. If the choroid is too violently stretched the muscles tear away from it, and remain usually attached to the sclerotic, as narrow bands, with finely serrated free edges. These fragments, brought under the microscope, give an elegant view of striped muscular fibres, mixed with elastic tissue.

"After having satisfactorily demonstrated the existence and situation of these muscles, and particularly of the *retrahens*, in the eye of the bird, a similar preparation may be made of the human eye. The arrangement is here, how-

ever, somewhat different. In the first place, the *retrahens* cannot be so distinctly demonstrated as a continuous band completely encircling the ciliary body; but in its place are to be seen only bundles of fibres, of varying thickness and consistency, which are coloured with brown pigment, and are not to be distinguished, by the naked eye, from the fibres of areolar tissue which run between the choroid and sclerotic. The only difference is that here, in the zone corresponding to the situation of the *retrahens* in birds, the bundles of fibres are thicker than elsewhere. The microscope shows these bundles to be composed of elastic tissue and unstripped muscular fibres.

"The action of these muscles may be easily understood from their situation and direction. Since they are firmly attached to the ciliary body, and this last is adherent to the capsule of the lens by the *zonula Zinnii*, it follows that the *protrahens*, in contraction must draw the lens forwards towards its point of attachment to the sclerotic, which necessarily remains immovable; while the contraction of the *retrahens* carries the lens backwards toward its own point of attachment.

"There remains one serious objection, however, which has been made to the supposition of any movement of the lens in the interior of the globe. The question is very properly asked, What becomes of the aqueous humour when the lens is advanced? and, what fills the space which has been left behind it? The aqueous humour is not compressible, and cannot escape behind the lens; for the opening which was imagined by Hueck to give passage to it, has no existence; and it appears from Senff's minute measurements (*Wagner's Handwörterbuch*, Part 16, p. 302), that we cannot suppose any variation in the convexity of the cornea.

"It is the belief of Dr. Von Roth that the variation in capacity of the different cavities of the globe is compensated by the action of the ciliary processes. The structure of these bodies, and the fan-shaped arrangement of the *zonula Zinnii*, which covers them, are both favourable to this supposition. As their vascular tufts project into the posterior chamber, they will be compressed by the aqueous humour when the lens advances, and deliver their blood into the vessels of the choroid behind; so that their partial emptying will give space for the aqueous humour just in proportion as this is encroached upon by the advancing lens. Neither can any vacuum be produced behind the lens, since the blood which has left the ciliary processes fills the vessels of the choroid, and exerts a lateral pressure on the vitreous humour."—*New York Medical Times*, May, 1854.

Double Monstrosity.—Prof. H. V. N. MILLER gives (*Southern Med. and Surg. Journ.* Feb. 1854) the following description of a rare form of monstrosity:—

"These children, named Milly and Christian, born in North Carolina, of African parentage, are females, now about two years old. They are united posteriorly. The os coccygis of each seems to be bent backwards and to become continuous with that of the other. The lower portion to probably one-third of its extent, of the sacrum of each, is in like manner joined by bony union to the corresponding portion of the sacrum of the other, forming, with the muscles attached to them and the general integument, a firm band of two or three inches in diameter, but so short that the nates of each child are pressed against those of the other. They are thus united back to back, but not exactly parallel; there is a slight inclination to the right side of the one and to the left side of the other. In consequence of this obliquity they lie more comfortably upon one side than the other, and from having been from birth constantly laid in this position, their heads are not symmetrical; the bones of the cranium having apparently yielded to the continued pressure in one direction.

"If the children be separated as widely as the uniting band will permit upon the side *towards* which the inclination exists, the hips (one of each child) united as above described, closely resemble the buttocks of a normal child, and between them is situated a common anus. If the legs of either child be separated, the vulva is brought into view; the upper part of which is not unusual, but at the lower portion it unites with the vulva of the other child, thus forming a common vulva consisting of the conjoined, upper or pubic halves of two vulvæ. Upon separating the labia of this conjoined vulva the entrance to the vagina of each

child is shown to be perfectly distinct, separated externally, however, but by a thin septum. The urethræ are also distinct.

"If the children be separated upon the side which admits of freest motion, the common vulva, just described, is disclosed, looking, when viewed in this direction, like a transverse sulcus, below which, at the distance of about an inch, is the anus common to both children.

"No means have been employed of determining how far up the rectum is the junction of the two alimentary canals, but it is probably above the internal sphincter, inasmuch as the children feel the desire to go to stool and actually discharge their feces at the same time. This, however, is the only thing which they do in common. The urine is discharged, separately and at different times. In all other respects they are perfectly distinct; hunger and thirst may be felt by one and not by the other: one may be sick and the other well; one suffered severely from teething, the other but little. One of them is a larger, stouter child than the other, but not perceptibly more intelligent. Their intellectual operations are as distinct as though no union existed; they amuse themselves together as do other children—sometimes become angry and resort to blows, and even at their early age are very ready, each to accuse the other of faults committed between them. They are still too young to determine what will be for them the easiest mode of progression. They can stand and walk a few steps either laterally or forward for one of them, while the other follows by a backward movement. The side step is that which most probably they will ultimately adopt."

Twins of Different Colour.—Dr. A. F. ATTAWAY, of Madison County, Geo., records (*Southern Medical and Surgical Journal*, June, 1854) the following remarkable example of this:—

Mrs. C——, a white woman, the mother of three children, gave birth to twins on the 16th of January, an interval of an hour intervening between the births.

The first born was very dark, and had every appearance of being of African paternity. Not being willing to suggest such a thing, I tried to explain the matter, by attributing the colour to cyanosis. At the expiration of one hour, the second child was born, and had very light-coloured hair, fair skin, and blue eyes, which made the contrast very striking.

The condition of the mother and children was such, that they required medical treatment for several weeks, during which time I marked the great difference between the children with peculiar interest.

After the recovery of the woman and her children, seeing the African characteristics more and more developed, I asked the mother to give me a correct relation of the circumstances connected with her conception, &c.

After some hesitation, she gave me the following history of her case: She said that five days after the cessation of her last menstruation, she had sexual intercourse with the white man, whom she considered the father of the white child. Three days thereafter, making eight days after menstruation, she cohabited with a negro man, who she said was the father of her other child. She assured me that this was the only coitus she had with the negro man for more than one month after she menstruated. If this be true, she conceived at that time.

The precise period of her other conception is less definite, in consequence of the fact that she had connection with the father of her white child, at different times, during the month following her last menstruation.

Report of the Committee on Medical Education to the American Medical Association, in 1853.—In a review of this report in our contemporary, the *Buffalo Medical Journal* (March, 1853), we find the following remarks:—

"It seems a work of supererogation to defend clinical teaching, but this report seriously underrates its advantages, and the *American Journal of Medical Sciences* 'fully coincides' in its remarks 'on the total inefficiency and absurdity of the system of clinical instruction, as now generally pursued in the hospitals of the United States.' We believe that we are not alone—more—that the intelligent sentiment of a vast majority of the profession is with us, in

conceding to clinical teaching the first rank in the means of the student's advancement.

* * * * *

"We notice this report thus formally, because it comes from a high source; it bears the apparent approval of the highest medical organization in our country; its author is a man of age and talent, who has earned a right to a careful consideration of any views he may promulgate, by a long and distinguished career of honorable effort; and, finally, because it bears the indorsement—hasty and unconsidered we hope—of the great name of the *American Journal of Medical Sciences*."

This last sentence contains two important errors:—

First. It is not correct to say that the report bears the apparent approval of the Association. The practice of the Association has always been to publish every report made to it from committees, even when two reports from the same committee advocating opposite opinions are presented; and furthermore, the Association has ordered the following disclaimer to be prefixed to each volume of its Transactions: "The American Medical Association, although formally accepting and publishing the reports of the various standing committees, holds itself wholly irresponsible for the opinions, theories, or criticisms therein contained, except when otherwise decided by special resolution."

This last has never, we believe, been done except in a single instance, that of the Report of the Committee on Medical Education, of 1851. (See Vol. IV. p. 32.)

Second. No one, who will attentively read the review of this report in the *American Journal*, can infer that the reviewer designed to undervalue clinical teaching. On the contrary, we esteem it *essential* to a complete medical education, and hold the opinion that no one who has not had the benefit of a proper and ample course of clinical studies should be admitted to practice medicine. At the same time, we are compelled to admit that the clinical instruction imparted in most of the hospitals of our country is altogether inadequate.

—
Note from Dr. La Roche.

DEAR DOCTOR: You will greatly oblige me by inserting the following lines in the forthcoming number of your Journal.

In the volume I lately issued on Pneumonia, I rather inadvertently included our countryman, Dr. Charles A. Lee, among the opponents of the doctrine of Malaria. (See p. 111.) For this I owe him an apology. How I came to commit the error, it is impossible for me now to state—possibly from the displacement, among my papers, of a note containing a reference to his letter to Dr. Forry on the subject of the Dew Point, and intended to be used elsewhere. As a proof that I did not so place Dr. Lee intentionally I may state, that on page 164 reference is made to his belief in the existence of aerial intangible poison as a cause of fever. Being aware that Dr. Lee would not feel flattered at being held up as a disbeliever in the malarial origin of that class of diseases, I think it my duty to remove, as far as I possibly can, the impression which my inadvertency may have created as regards this matter in the minds of the readers of the volume in question.

Very truly, your friend,

R. LA ROCHE.

To I. HAYS, M. D.

FISKE MEDICAL PRIZE QUESTION.

THE TRUSTEES OF THE FISKE FUND,

AT THE

ANNUAL MEETING OF THE RHODE ISLAND MEDICAL SOCIETY,

Held at Providence, June 7, 1854, announced that the premium of Fifty Dollars offered by them in 1853, for the best dissertation on *Neuralgia, its history and best mode of treatment*, had been awarded to the author of the dissertation bearing the motto—

“What is writ is writ,
Would it were worthier!”

And upon breaking the seal of the accompanying packet, they learned that the successful competitor was CHARLES W. PARSONS, M. D., of Providence, Rhode Island.

They propose the following subjects for 1855 :—

I. CROUP.

II. THE EFFECT OF CLIMATE ON TUBERCULOUS DISEASE.

For the best Dissertation on the first subject, the Trustees will pay Fifty Dollars, and on the second One Hundred Dollars.

Every competitor for a premium is expected to conform to the following regulations, viz :—

To forward to the Trustees, on or before the 1st of May, 1855, free of all expense, a copy of his dissertation, with a motto written thereupon, and also accompanying a sealed packet, having the same motto inscribed upon the outside, and his name and place of residence within.

Previously to receiving the premium awarded, the author of the successful dissertation must transfer to the Trustees all his right, title, and interest in and to the same, for the use, benefit, and behoof of the Fiske Fund.

Letters accompanying the unsuccessful dissertations will be destroyed by the Trustees, unopened, and the dissertations may be procured by their respective authors, of the Trustee to whom they were directed, if application be made therefor within three months.

JOSEPH MAURAN, M. D., Providence,	} Trustees.
ARIEL BALLOU, M. D., Cumberland,	
HIRAM CLEVELAND, M. D., North Providence,	

S. AUG. ARNOLD, M. D., *Secretary*.

GRADUATES OF THE UNIVERSITY OF PENNSYLVANIA.

At a Public Commencement of the University of Pennsylvania, held April 1, 1854, in the Musical Fund Hall, Locust Street, the degree of DOCTOR OF MEDICINE was conferred by the Provost, HENRY VETHAKE, LL. D., upon the following gentlemen; after which an Address was delivered by WILLIAM GIBSON, M. D., Professor of Surgery.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF ESSAY.
Atkinson, Archibald, Jr.	Smithfield,	Isle of Wight,	Va.	Respiration.
Avera, Thomas H.	Eagle Rock,	Wake,	N. C.	Mental and Physical Education of Females.
Baker, Joseph H.	Tarboro',	Edgecomb,	N. C.	Syphilis, Primary and Constitutional.
Balliet, Louis B.	North White Hall,	Lehigh,	Pa.	Management of Children during Teething.
Baltzell, Wm.	Frederick,	Frederick,	Md.	Physiology of Vision.
Banks, Thomas L.	Raleigh,	Wake,	N. C.	Water as a Therapeutic Agent.
Barnard, John D.	Savannah,	Chatham,	Ga.	Epidemic Dysentery of 1853 in Georgia.
Barry, Jasper A.	Clarksville,	Red River,	Texas,	Dyspepsia.
Bean, James M.	Philadelphia,		Pa.	Acute Pleurisy.
Bell, Charles	Chester,	Rockingham,	N. H.	Necrosis.
Bell, John	Chester,	Rockingham,	N. H.	Tuberculosis.
Bellamy, Charles E.	Columbus,	Muscogee,	Ga.	Trephining.
Benner, Henry D.	Philadelphia,		Pa.	Symptoms and Treatment of Scarlatina.
Bibb, George R.	Russellville,	Logan,	Ky.	Pneumonitis.
Bishop, C. Stanhope	Philadelphia,		Pa.	Pulmonary Consumption.
Black, Wm. Thomas	Pulaski,	Giles,	Tenn.	Cholera Infantum.
Blair, James M.	Boalsburg,	Centre,	Pa.	Alcohol.
Blount, Richard H.	Woodville,	Perquimans,	N. C.	Causes of Impeded Respiration.
Bown, Edwin T.	Brantford,	Brant,	Ca. W.	Progressive Development from a Common Origin.
Braxton, Tomlin	Old Church,	Hanover,	Va.	The Objects and Methods of Physiological Inquiries.
Bright, Robert S.	Murfreesborough,	Rutherford,	Tenn.	Pleurisy.
Brooke, John	Spread Eagle,	Chester,	Pa.	Physiology of Death.
Brown, Aurelius P.	Fort Royal,	Warren,	Va.	Physiology of Vision.
Budlong, James	Pawtucket,	Bristol,	Mass.	Inguinal Hernia.
Butt, Albert	Norfolk,	Norfolk,	Va.	Inguinal Hernia.
Byrne, Albert C.	The Plains,	Fauquier,	Va.	Physiology of the Brain with a Refutation of Phrenological Doctrine.
Cain, James F.	Hillsboro',	Orange,	N. C.	Cod-liver Oil.
Caldwell, Julius A.	Salisbury,	Rowan,	N. C.	Acute Dysentery.
Case, Wm. E.	Lahaska,	Bucks,	Pa.	Hygiene.
Cochran, James L.	Shippensburg,	Cumberland,	Pa.	Enteric Fever.
Cocke, Wm. H.	Portsmouth,	Norfolk,	Va.	Yellow Fever.
Collins, Benjamin M.	Wrightstown,	Bucks,	Pa.	Enteric Fever.
Cook, Philip H.	Franklin,	Williamson,	Tenn.	Epidemic Dysentery of 1853 in Williams Co., Tennessee.
Davidson, Samuel T.	Nashville,	Davidson,	Tenn.	Gunshot Wounds.
Day, David	St. Paul,	Ramsey,	Min.	Medicine among the Indians.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
De Benneville, Jas. S.	Philadelphia,		Pa.	Fractures.
Detwiller, John J.	Easton,	Northampton,	Pa.	Hemorrhaphylia.
Dillard, George P.	Henry C. H.	Henry,	Va.	Enteric Fever.
Drake, John R.	Nashville,	Nash,	N. C.	Acute Peritonitis.
Draper, Lemuel J.	Milford,	Kent,	Del.	Therapeutic Effects of Common Water.
Dunn, J. Gay	Prairie Point,	Noxubee,	Miss.	Cynanche Trachealis.
Eastman, Thomas J.	Platteville,	Grant,	Wis.	Some of the Differences observed between the Saxon and Negro.
Easton, Thomas S.	Mobile,	Mobile,	Ala.	Yellow Fever.
Eichelberger, Henry S.	Winchester,	Frederick,	Va.	Anatomy of the Teeth.
Engelhard, Leopold	Wheeling,	Ohio,	Va.	Action of the Narcotics.
Eskridge, Thomas	Hays's Creek,	Carroll,	Miss.	Colo Rectitis.
Faircloth, Siley E.	Speight's Bridge,	Green,	N. C.	Abortion.
Findlay, W. Spiller	Abingdon,	Washington,	Va.	Medicine—a Science.
Fishbourn, Cyrus D.	Womelsdorf,	Berks,	Pa.	Enteric Fever.
Fletcher, John R.	Shoal Ford,	Limestone,	Ala.	Water as a Therapeutic Agent.
Fletcher, Richard M.	Shoal Ford,	Limestone,	Ala.	Enteric Fever.
Forwood, Wm. S.	Darlington,	Harford,	Md.	Hemorrhagus Uteri.
Foulke, Joseph	Springhouse,	Montgomery,	Pa.	Electricity.
Foulke, William	Philadelphia,		Pa.	Hemorrhoids.
Fowler, Josiah C.	Rolesville,	Wake,	N. C.	The Liver.
Fithian, Enoch	Greenwich,	Cumberland,	N. J.	Medicine.
Galt, Francis L.	Augusta,	Richmond,	Ga.	Enteric Fever.
Galt, James D.	Norfolk,	Norfolk,	Va.	Cholera Infantum.
German, Daniel, Jr.	Franklin,	Williamson,	Tenn.	Autocrateia.
Gill, Cleophas J.	Forrestville,	Wake,	N. C.	Tetanus.
Gillespie, James H.	Louisa C. H.	Louisa,	Va.	Menstruation.
Glenn, Robert W.	Gilmer's Store,	Guilford,	N. C.	Scarlatina.
Green, Charles B.	Garrettsburg,	Christian,	Ky.	Diagnosis.
Green, James B.	Quakertown,	Bucks,	Pa.	Puerperal Convulsions.
Greenlee, Elisha G.	Fancy Hill,	Rockbridge,	Va.	Pneumonia.
Griffin, William K.	Milton,	Laurens,	S. C.	Necrosis.
Grube, Franklin	Pottstown,	Chester,	Pa.	Abuse of Tobacco.
Grymes, Alfred	New York,		N. Y.	Tetanus.
Hancock, Flemming J.	Partlows,	Caroline,	Va.	Cholera Infantum.
Harris, Hartwell H.	Milldale,	Warren,	Miss.	Pernicious Fever.
Hartswick, John G.	Boalsburg,	Centre,	Pa.	Chronic Rheumatism.
Hatton, James L.	Portsmouth,	Norfolk,	Va.	Pathology and Treat- ment of Variola.
Hay, William G.	Warrior Stand,	Macon,	Ala.	Water as a Therapeutic and Hygienic Agent.
Hibbett, James L.	Gallatin,	Sumner,	Tenn.	Physical Diagnosis.
Higgins, Archibald A.	Princeton,	Mercer,	N. J.	Typhus Fever.
Higgins, George J.	Tannaqua,	Schuylkill,	Pa.	Medicus Verus.
Hill, Joseph R.	Yazoo City,	Yazoo,	Miss.	Therapeutic Applica- tions of Cinchona.
Horner, Albert A.	Helena,	Philips,	Ark.	Intermittent Fever.
Hornsbey, Samuel P.	Woodville,	Wilkinson,	Miss.	Opium.
Howle, Thomas P.	Richmond,	Henrico,	Va.	Rubeola.
Hudson, William O.	Locust Dale,	Madison,	Va.	Acute Pneumonia.
Hunter, George B.	Franklin,	Williamson,	Tenn.	Anatomy and Physio- logy of the Heart.
Hunter, William D.	Honey Brook,	Chester,	Pa.	Inguinal Hernia.
James, George W. C.	Bedford,	Bedford,	Pa.	Physiology of Respira- tion.
Jenks, P. Frederick	Newtown,	Bucks,	Pa.	Neuralgia.
Johnson, George	Frederick,	Frederick,	Md.	Pernicious Fever.
Jones, Richard W.	Marion,	Smyth,	Va.	Menstruation—Practi- cally considered.
Kearney, Thomas	Jefferson,	Caddo Parish,	La.	Marsh Miasm.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Keasbey, John B.	Salem,	Salem,	N. J.	The Progress of Medicine.
Kephart, Thomas P.	Montgomery,	Montgomery, Pa.		Physical Education of Females.
Kline, Wm. O., Jr.	Philadelphia,		Pa.	Ophthalmia Purulenta.
Knox, Wm. F.	Brownsville,	Fayette,	Pa.	Phthisis Pulmonalis.
Kuhn, Louis De B.	East Berlin,	Adams,	Pa.	Arsenical Poisoning.
Lane, John M.	Vicksburg,	Warren,	Miss.	Remittent Fever.
Lea, James T.	Oakland,	Chatham,	N. C.	Pernicious Fever.
Longaker, Henry D.	Perkiomen Bridge,	Montgomery,	Pa.	Pleurisy.
Ludlow, Benj. Chambers	Cincinnati,	Hamilton,	Ohio,	Formation of Callus.
M'Bride, Isaac	Philadelphia,		Pa.	Amenorrhœa.
M'Lean, Lucius A.	Greenwood,	Carroll,	Miss.	Remittent Fever.
Macon, Gideon H.	Warrenton,	Warren,	N. C.	Enteric Fever.
Martin, Robert B.	Elizabeth City,	Pasquotank,	N. C.	Lobar Pneumonia.
Miller, Abraham S.	New Market,	Shenandoah,	Va.	Fractures of Tibia and Fibula.
Miller, Geo. McClellan	Wilmington,	New Castle,	Del.	Infantile Convulsions.
Minis, David, Jr.	Beaver,	Beaver,	Pa.	Special Anatomy of the Negro.
Minturn, Edward	Philadelphia,		Pa.	Menstruation.
Mitchell, S. B. Wylie	Philadelphia,		Pa.	Fractura Complicata.
Moore, Samuel	Clarksville,	Red River,	Texas,	Oleum Terebinthinæ.
Morris, James Cheston	Philadelphia,		Pa.	The Blood-cells.
Morisey, Samuel B.	Clinton,	Sampson,	N. C.	Syphilis.
Newman, John W.	Salem,	Fauquier,	Va.	Typhoid Fever.
Oberholtzer, Levi	Lionsville,	Chester,	Pa.	Cold as a Therapeutic Agent.
Paschall, James P.	Oxford,	Granville,	N. C.	Oleum Terebinthinæ.
Pierce, George A.	Providence,	Providence,	R. I.	Morbus Brightii.
Pinkston, W. D.	Warrensburg,	Johnson,	Mo.	Typhoid Fever.
Powell, William L.	Roxobel,	Bertie,	N. C.	Typhus Fever.
Priestly, Henry I.	St. John,		N. B.	Acute Dysentery.
Price, Wm. B. K.	King George C.H.,	King George,	Va.	Scarlatina.
Rankin, Davis	Newark,	New Castle,	Del.	Opium.
Rhoads, John W.	Milton,	Northumberland,	Pa.	The Duties and Responsibilities of the Physician.
Richmond, Wm. C.	Oxford,	Lafayette,	Miss.	Fractures.
Riddick, Richard H.	Gatesville,	Gates,	N. C.	Erysipelas.
Rixey, Samuel R.	Culpepper C. H.	Culpepper,	Va.	Enteric Fever.
Roane, T. Walter	Covington,	Tipton,	Tenn.	Pathological Anatomy of Malignant Epigenises.
Ryan, Fred. B.	Holly Springs,	Marshall,	Miss.	Pernicious Fever of the Southwest.
Sawyer, J. Iredell	Edenton,	Chowan,	N. C.	Bloodletting.
Scarborough, Ira	Snow Hill,	Greene,	N. C.	Rubeola.
Schlemm, Wm. E.	Maiden Creek,	Berks,	Pa.	Indolent Ulcers.
Shackleford, Robt. B.	Stony Point,	Albemarle,	Va.	Calorification.
Sharp, Edward S.	Salem,	Salem,	N. J.	Polypus of the Uterus.
Shaw, Henry J.	Ryan's Store,	Montgomery,	Tenn.	Dysentery.
Silvester, Richard J.	Norfolk,	Norfolk,	Va.	Tetanus.
Simington, Robert S.	Milton,	Northumberland,	Pa.	Apoplexy.
Skillern, Samuel R.	Pulaski,	Giles,	Tenn.	Enteric Fever.
Smiley, A. Jackson	Columbus,	Franklin,	Ohio,	Homœopathy as a System of Practical Therapeutics.
Smith, George B.	Nashville,	Nashville,	Tenn.	Syphilis.
Smith, Josiah	Mt. Surprise,	Luzerne,	Pa.	Pneumonia.
Steele, Stephen H.	Ripley,	Lauderdale,	Tenn.	Dysentery.
Steward, Robert	Natchez,	Adams,	Miss.	Light in its Relations to Life.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Stiles, R. Cresson	West Chester,	Chester,	Pa.	Animal Heat.
Stone, James B.	Franklin,	Williamson,	Tenn.	Puerperal Peritonitis.
Stribling, Robert M. Jr.	Markham,	Fauquier,	Va.	Physiology of the Psychological Faculties.
Swanson, Edward	Franklin,	Williamson,	Tenn.	Terebinthina.
Taylor, Bushrod	Winchester,	Frederick,	Va.	Conjunctivitis.
Thomson, Andrew W. Jr.	Unionville,	Union Dist.,	S. C.	Palpitation of the Heart.
Thornton, Byron	Lahaska,	Bucks,	Pa.	Physiology of Secretion.
Thruston, Stephen D.	Hickory Ford,	Gloucester,	Va.	Functions of Female in Reproduction.
Treichler, Samuel K.	Clayton,	Berks,	Pa.	Incised Wounds.
Tupper, Nathan	Amherst,	Cumberland,	N. S.	Puerperal Fever.
Uhler, Jonathan K.	Philadelphia,		Pa.	Benzoic Acid.
Updegrave, Silas	Norristown,	Montgomery,	Pa.	Conduct of a Natural Labour.
Vaughan, J. Franklin	Wilmington,	New Castle,	Del.	Yellow Fever from local sources of Infection.
Walker, Lewis L.	Philadelphia,		Pa.	Vitality of the Blood.
Walker, Wm. E.	Clinton,	East Feliciana,	La.	Criminal Jurisprudence of Insanity.
Wall, Asa	Winchester,	Frederick,	Va.	Stricture of the Urethra.
Ware, James B.	Cedarville,	Cumberland,	N. J.	The Science of Medicine.
Warren, John A.	Delta,	Coahoma,	Miss.	Fractures.
Warren, John H.	Princeton,	Mercer,	N. J.	Acute Gastritis.
Warren, Wm. C.	De Kalb,	Kemper,	Miss.	The Philosophy of Stimulation.
Watson, William H.	Bedford,	Bedford,	Pa.	Bedford Waters.
Watson, Robert C.	Hotel,	Bertie,	N. C.	Gastritis.
Watts, Jesse A.	Galt's Mills,	Amherst,	Va.	Hygienic Education of Children.
Webb, Stephen M. D.	Sumterville,	Samter,	Ala.	Enteric Fever.
White, Lorenzo	Big Spring,	Wilson,	Tenn.	Cholera Infantum.
Williams, John M.	Farmington,	Davie,	N. C.	Scarlatina.
Wills, Wm. Thaddeus	Brownsville,	Haywood,	Tenn.	Digestion.
Wilson, Rezin B.	Philippi,	Barbour,	Va.	Simple Acute Dysentery.
Windle, John	Fairville,	Chester,	Pa.	Enteric Fever.
Withers, George W.	Lancaster,	Lancaster,	Pa.	Puerperal Convulsions.
Withers, Michael M.	Lancaster,	Lancaster,	Pa.	Acute Hepatitis.
Woodson, Everette W.	Garrettsburg,	Christian,	Ky.	The Duties, Trials, and Pleasures of the Physician.
Woolston, Elijah B.	Vincent Town,	Burlington,	N. J.	Subcutaneous Vaccine Pustule.

At the Commencement held July 1, 1853, the Degree of Doctor of Medicine was conferred on the following gentlemen:—

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Crouse, Henry	Simcoe,	Norfolk,	Canada,	Delirium Tremens.
Dortch, Caleb W.	Memphis,	Shelly,	Miss.	Anatomy, Physiology, and Hygiene of Respiratory Organs.
Foulke, Antrim	Philadelphia,		Pa.	
Holmes, Stillman	Plymouth,	Plymouth,	Mass.	Dyspepsia.
Suddards, Henry	Philadelphia,		Pa.	Smallpox.
Thomas, Andrew J.	Smithfield,	Isle of Wight,	Va.	Dyspepsia.

Total, 177.

UNIVERSITY OF PENNSYLVANIA, MEDICAL DEPARTMENT.

EIGHTY-NINTH SESSION (1854-55).

The Lectures will commence on Monday, October 9, and terminate in the middle of March ensuing.

Theory and Practice of Medicine,	By GEORGE B. WOOD, M. D.
Anatomy,	" JOSEPH LEIDY, M. D.
Materia Medica and Pharmacy,	" JOSEPH CARSON, M. D.
Chemistry,	" ROBERT E. ROGERS, M. D.
Surgery,	" WILLIAM GIBSON, M. D.
Obstetrics and the Diseases of Women and Children,	" HUGH L. HODGE, M. D.
Institutes of Medicine,	" SAMUEL JACKSON, M. D.

Clinical Instruction will be given at the Pennsylvania Hospital.

Clinical Instruction in Medicine and in Surgery by the Professors of the Medical Faculty, assisted by W. W. GERHARD, M. D.

The Course of Dissections will be conducted by the Professor of Anatomy, aided by the Demonstrator.

Amount of Fees for Lectures in the University,	\$105
Matriculating Fee (paid once only),	5
Hospital Fee,	10
Practical Anatomy,	10
Graduating Fee,	30

JOSEPH CARSON, M. D., *Dean of the Medical Faculty,*
332 Walnut Street, between Twelfth and Thirteenth Streets.

F. B. DICK, *Janitor, University.*

MEDICAL COLLEGE OF THE STATE OF SOUTH CAROLINA.

The annual Course of Lectures in this Institution will commence on the first Monday in November, in Charleston, on the following branches:—

Anatomy by	J. E. HOLBROOK, M. D.
Surgery by	E. GEDDINGS, M. D.
Institutes and Practice by	S. HENRY DICKSON, M. D.
Physiology by	JAMES MOULTRIE, M. D.
Materia Medica by	HENRY R. FROST, M. D.
Obstetrics by	THOS. G. PRIDLEAU, M. D.
Chemistry by	C. U. SHEPARD, M. D.
Demonstrator of Anatomy by	FRANCIS T. MILES, M. D.
Prosecutor of the Professor of Surgery by	J. F. M. GEDDINGS, M. D.

CLINICAL LECTURES.

D. T. Cain, M. D., Physician to the Marine Hospital and Clinical Instructor, lectures twice a week on the diseases of that Institution.

H. W. Desaussure, M. D., Physician to the Hospital of the Almshouse, at which Lectures are delivered twice a week on Diseases, the diagnosis discriminated, and the student indoctrinated in their treatment.

Demonstrative Instruction in Medicine and Surgery at the College Hospital, by the Professor of the Medical College.

The fees for the Course and the expenses of boarding, are much the same as in the other cities of the Atlantic coast.

The Faculty of the College take pleasure in calling the attention of the friends of the Institution to its present prosperous condition—the last class exceeding any of former years. They have been enabled, by the liberality of the Legislature at its last session, to make such alterations in extending and improving the College building as will promote materially the comfort of those in attendance on the lectures.

The Anatomical Theatre has been enlarged, and completely renovated, and such changes made as will secure free ventilation, with a pleasant arrangement of the seats. They confidently believe that it will not suffer in comparison with any like structure in the United States—the edifice, with its appurtenances, being as commodious and attractive as any such establishment in the country.

They have also been enabled to make considerable additions to the Museum of the College.

HENRY R. FROST, M. D., *Dean.*

June 10, 1854.

JEFFERSON MEDICAL COLLEGE

SESSION OF 1854-5.

The regular Course of Lectures will commence on Monday, the 9th of October, and continue until the first day of March. The ANNUAL COMMENCEMENT for conferring degrees will be held *early in March*, instead of at the end of the month, as heretofore.

ROBLEY DUNGLISON, M. D.,	Professor of Institutes of Medicine, &c.
ROBERT M. HUSTON, M. D.,	Professor of Materia Medica and Gen. Therapeutics.
JOSEPH PANCOAST, M. D.,	Professor of General, Descriptive, and Surg. Anatomy.
JOHN K. MITCHELL, M. D.,	Professor of Practice of Medicine.
THOMAS D. MUTTER, M. D.,	Professor of Institutes and Practice of Surgery.
CHARLES D. MEIGS, M. D.,	{ Professor of Obstetrics, and Diseases of Women and Children.
FRANKLIN BACHE, M. D.,	
	Professor of Chemistry.

ELLERSLIE WALLACE, M. D., Demonstrator of Anatomy.

Every Wednesday and Saturday in the month of October, and during the Course, Medical and Surgical cases will be investigated, prescribed for, and lectured on before the class. During the past year, *seventeen hundred and fifty-eight* cases were treated, and *three hundred and thirteen* operations performed in the presence of the class. Amongst these were many major operations—as amputation of the thigh, leg, mammae, &c., lithotripsy, trephining.

The Lectures are so arranged as to permit the student to attend the Medical and Surgical practice and Lectures at the Pennsylvania Hospital.

On and after the 1st of October, the dissecting rooms will be open, under the direction of the Professor of Anatomy and the Demonstrator.

FEES:

Matriculation, which is paid only once,	\$ 5
Each Professor, \$15,	105
Graduation,	30

The number of Students during the last Session was 627; and of Graduates 270.

R. M. HUSTON, M. D.,

Dean of the Faculty, No. 358 Arch Street, and at the College.

PHILADELPHIA, June, 1854.

UNIVERSITY OF LOUISVILLE.

MEDICAL DEPARTMENT.

The Eighteenth annual Course of Lectures in this Department will commence on the 30th of October next, and terminate on the last of February, under the following arrangement:—

BENJAMIN R. PALMER, M. D.,	Professor of Descriptive and Surgical Anatomy.
LUNSFORD P. YANDELL, M. D.,	Professor of Physiology and Pathological Anatomy.
SAMUEL D. GROSS, M. D.,	Professor of the Principles and Practice of Surgery.
HENRY MILLER, M. D.,	Professor of Obstetric Medicine.
LEWIS ROGERS, M. D.,	Professor of Materia Medica and Therapeutics.
J. LAWRENCE SMITH, M. D.,	Professor of Medical Chemistry and Toxicology.
AUSTIN FLINT, M. D.,	Professor of the Theory and Practice of Medicine.
T. G. RICHARDSON, M. D.,	{ Demonstrator of Anatomy and Dissector in Pathological Anatomy.

The fee for admittance to the Lectures of each Professor, is \$15 (\$105 in all), payable invariably in advance. Matriculation and Library fee together, \$5. Graduation fee, \$25. Practical Anatomy and Dissection, \$10—tickets to be taken at least once before graduation. Rooms open from 1st October.

A preliminary course of Lectures will be delivered, without additional charge, during the month of October.

Clinical instruction is given twice a week at the Louisville Marine Hospital. Tickets \$5, to be taken once before graduation.

A Clinique has been established in connection with the University, at which cases are examined, prescribed for, and lectured upon in presence of the class.

Good boarding can be procured at \$3 a week.

L. P. YANDELL, M. D., *Dean of the Faculty*.

June 14, 1854.

PENNSYLVANIA COLLEGE, MEDICAL DEPARTMENT.

NINTH STREET BELOW LOCUST.

SESSION OF 1854-5.

By the authority of the Board of Trustees of Pennsylvania College, of Gettysburg, Pa., the Faculty of the Medical Department at Philadelphia is constituted as follows:—

DAVID GILBERT, M. D.,	Obstetrics and Diseases of Women and Children.
ALFRED STILLÉ, M. D.,	Theory and Practice of Medicine.
JOHN NEILL, M. D.,	Principles and Practice of Surgery.
J. M. ALLEN, M. D.,	Anatomy.
JOHN J. REESE, M. D.,	Medical Chemistry and Pharmacy.
FRANCIS G. SMITH, M. D.,	Institutes of Medicine.
JOHN B. BIDDLE, M. D.,	Therapeutics and Materia Medica.

The Lectures will commence on Monday, October 9, and continue until the 1st of March ensuing. A Medical and Surgical Clinic is held regularly twice a week at the College and at the Pennsylvania Hospital. Second-course students receive tickets to the latter institution free of expense.

FEES.—Matriculation (paid once only)	\$ 5 00
For each Professor's ticket	15 00
“ Graduation	30 00
“ Practical Anatomy	10 00

The Dissecting-Rooms will be opened in the month of September.

For further information, apply to

JOHN J. REESE, M. D., *Registrar*,
No. 122 South Ninth Street.

GENEVA MEDICAL COLLEGE.

The next Course of Lectures, under the new organization, will commence on Wednesday, October 4, 1854, and continue sixteen weeks.

FACULTY.

WILLIAM SWEETSER, M. D., of New York,	Institutes and Practice of Medicine.
THOMAS RUSH SPENCER, M. D., of Geneva,	Materia Medica and General Pathology.
JOEL E. HAWLEY, M. D., of Ithaca,	Principles and Practice of Surgery.
CHARLES AP A. BOWEN, M. D., of Geneva,	Anatomy and Physiology.
JOHN TOWLER, A. M., Professor in Hobart Free College,	Chemistry and Pharmacy.
FREDERICK HYDE, M. D., of Cortlandville,	Obstetrics, Diseases of Women and Children, and Medical Jurisprudence.

The fees for all the Lectures are \$62. Matriculation fee, \$3. Board can be obtained from \$1 50 to \$3 a week. Degrees are conferred at the close of the Course, and in July. Further information may be obtained from either of the Faculty resident at Geneva.

J. TOWLER, *Dean of the Faculty*.

ALBANY MEDICAL COLLEGE.

Two full Courses of Lectures are delivered annually. The next *Fall Course* commences on Tuesday, the 5th of September, 1854; the next *Spring Course* on the third Tuesday of February, 1855. Both Courses continue sixteen weeks. Degrees are conferred at the close of each term. Fees for a single Course, \$60; for both Courses (payable in advance), \$100. Graduation fee, \$20. A spacious Hospital has been opened in immediate connection with the College, to which students are admitted free of charge. Materials for dissection are abundant, and furnished to students on the most moderate terms. Cliniques are held in the College regularly, and a large number of operations performed.

ALDEN MARCH, M. D.,	Professor of Surgery.
JAMES MCNAUGHTON, M. D.,	Professor of the Theory and Practice of Medicine.
JAMES H. ARMSBY, M. D.,	Professor of Anatomy.
THOMAS HUN, M. D.,	Professor of the Institutes of Med. and Materia Medica.
AMOS DEAN, LL. D.,	Professor of Medical Jurisprudence.
HOWARD TOWNSEND, M. D.,	Professor of Obstetrics.
EZRA S. CARR, M. D.	Professor of Chemistry and Pharmacy.

COLLEGE OF PHYSICIANS AND SURGEONS, NEW YORK.

The regular Course of Lectures for the Session of 1854-55 will commence on Monday, October 19, and continue five months. A Preliminary Course will, as usual, be delivered, commencing early in September. This course is free.

Fees for Regular Course:—

Matriculation,	\$ 5
Full Course of Lectures	105
Graduation	25

FACULTY.

ALEXANDER H. STEVENS, M. D., LL. D.,	President of the College, and Emeritus Professor of Surgery.
JOSEPH M. SMITH, M. D.,	Professor of Practice of Medicine.
JOHN TORREY, M. D., LL. D.,	Professor of Chemistry and Botany.
ROBERT WATTS, M. D.,	Professor of Anatomy.
WILLARD PARKER, M. D.,	Professor of Surgery.
CHANDLER R. GILMAN, M. D.,	Professor of Obstetrics.
ALONZO CLARK, M. D.,	Professor of Pathology and Physiology.
ELISHA BARTLETT, M. D.,	Professor of Materia Medica.
JOHN A. LIDELL, M. D.,	Demonstrator of Anatomy.
LEWIS A. SAYRE, M. D.,	Prosector in Surgery.
GEORGE T. ELLIOTT, M. D.,	Assistant to the Professor of Obstetrics.
JAMES KNOX,	Janitor.

Owing to the continued ill health of Professor Bartlett, the Faculty have made the following provisional arrangements for the ensuing Session, viz: Professor Smith will deliver the Course on Materia Medica, Professor Clark that on Practical Medicine, resigning the Course on Physiology to Professor John C. Dalton, Jr., of Buffalo University.

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To indicate the reputation which the work has attained, both in this country and abroad, a few notices of the first edition are subjoined.

Dr. Griffith's Formulary is worthy of recommendation, not only on account of the care which has been bestowed on it by its estimable author, but for its general accuracy, and the richness of its details.—*Medical Examiner*.

Most cordially we recommend this Universal Formulary, not forgetting its adaptation to druggists and apothecaries, who would find themselves vastly improved by a familiar acquaintance with this every-day book of medicine.—*The Boston Med. and Surg. Journal*.

A very useful work, and a most complete compendium on the subject of materia medica. We know of no work in our language, or any other, so comprehensive in all its details.—*London Lancet*.

Pre-eminent among the best and most useful compilations of the present day will be found the work before us, which can have been produced only at a very great cost of thought and labor. A short description will suffice to show that we do not put too high an estimate on this work. We are not cognizant of the existence of a parallel work. Its value will be apparent to our readers from the sketch of its contents above given. We strongly recommend it to all who are engaged either in practical medicine, or more exclusively with its literature.—*London Med. Gazette*.

A valuable acquisition to the medical practitioner, and a useful book of reference to the apothecary on numerous occasions.—*American Jour. of Pharmacy*.

A condensed list of the contents, and a few specimen pages, are appended, to show the plan of the work, and the manner in which it has been carried out. It should be added that the FORMULARY proper consists of

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and that the whole of this vast amount of important information is rendered easy of reference by three complete and extended indexes. The first of these, occupying more than twenty pages, is of "Diseases and their Remedies," giving under the name of each disease all the remedies and preparations which have been prescribed for it. The second Index is of "Pharmaceutical and Botanical Names;" while the third, or "General Index," of nearly sixty pages, in small type, presents the whole contents of the book, digested alphabetically under its appropriate heads. By this extended and thorough system of reference, the volume therefore presents all the advantages of all the different plans of arrangement.

TABLE OF CONTENTS.

	PAGE
PREFACES	7, 9
INTRODUCTION	17
WEIGHTS AND MEASURES.....	17
Weights of the United States and Great Britain.....	17
Dublin Weights	20
Foreign Weights.....	20
Measures	24
SPECIFIC GRAVITY.....	28
TEMPERATURES FOR CERTAIN PHARMACEUTICAL OPERATIONS.....	36
HYDROMETRICAL EQUIVALENTS.....	36
SPECIFIC GRAVITIES OF SOME OF THE PREPARATIONS OF THE PHARMACOPŒIAS.....	41
RELATION BETWEEN THE DIFFERENT THERMOMETRICAL SCALES.....	42
EXPLANATION OF PRINCIPAL ABBREVIATIONS USED IN FORMULÆ.....	43
VOCABULARY OF WORDS EMPLOYED IN PRESCRIPTIONS.....	44
OBSERVATIONS ON THE MANAGEMENT OF THE SICK ROOM.....	55
Ventilation of the Sick Room.....	55
Temperature of the Sick Room.....	56
Cleanliness in the Sick Room.....	57
Quiet in the Sick Room.....	57
Examination and Preservation of the Excretions.....	58
Administration of Medicine.....	58
Furniture of a Sick Room.....	58
Proper use of Utensils for Evacuations.....	59
DOSES OF MEDICINES.....	60
Age.....	61
Sex	61
Temperament.....	61
Idiosyncrasy.....	61
Habit.....	61
State of the System.....	62
Time of Day.....	62
Intervals between Doses.....	63
RULES FOR ADMINISTRATION OF MEDICINES.....	63
Acids.....	64
Antacids.....	64

	PAGE
Antilithics and Lithontriptics.....	64
Antispasmodics.....	64
Anthelmintics.....	65
Cathartics.....	65
Enemata.....	66
Suppositories.....	66
Demulcents or Emollients.....	67
Diaphoretics.....	67
Diluents.....	68
Diuretics.....	68
Emetics.....	68
Emmenagogues.....	69
Epispastics.....	69
Errhines.....	71
Escharotics.....	71
Expectorants.....	71
Narcotics.....	72
Refrigerants.....	72
Sedatives.....	72
Sialagogues.....	72
Stimulants.....	73
Tonics.....	73
MANAGEMENT OF CONVALESCENCE AND RELAPSES.....	74
UNIVERSAL FORMULARY.....	75
DIETETIC PREPARATIONS NOT INCLUDED AMONG THE PREVIOUS PRESCRIPTIONS.....	487
LIST OF INCOMPATIBLES.....	492
POSOLOGICAL TABLE OF THE MOST IMPORTANT MEDICINES.....	496
TABLE OF PHARMACEUTICAL NAMES WHICH DIFFER IN THE UNITED STATES AND BRITISH PHARMACOPŒIAS.....	503
OFFICINAL PREPARATIONS AND DIRECTIONS.....	514
INTERNAL REMEDIES.....	514
Powders.....	514
Pills and Boluses.....	515
Extracts.....	517
Confections, Conserves, Electuaries.....	518
Pulps.....	519
Syrups.....	519
Mellites or Honeyes.....	520
Infusions.....	520
Decoctions.....	520
Tinctures.....	521
Wines.....	523
Vinegars.....	524
Mixtures.....	524
Medicated Waters.....	525
Distilled, Essential, or Volatile Oils.....	528
Fixed Oils and Fats.....	533

	PAGE
Alkaloids.....	533
Spirits.....	534
Troches or Lozenges.....	534
Inhalations.....	535
EXTERNAL REMEDIES.....	535
Baths.....	535
Cold Bath.....	535
Cool Bath.....	536
Temperate Bath.....	536
Tepid Bath.....	536
Warm Bath.....	536
Hot Bath.....	537
Shower Bath.....	537
Local Baths.....	537
Vapor Bath.....	538
Warm Air Bath.....	538
Douches.....	538
Medicated Baths.....	539
Affusion.....	539
Sponging.....	540
Fomentations.....	541
Cataplasms, or Poultices.....	541
Lotions, Liniments, Embrocations.....	542
Vesicatories, or Blisters.....	543
Issues.....	543
Setons.....	543
Ointments.....	544
Cerates.....	544
Plasters.....	545
Fumigations.....	546
BLOOD-LETTING.....	547
General Blood-letting.....	548
Venesection.....	548
Arteriotomy.....	550
Topical Blood-letting.....	550
Cupping.....	550
Leeching.....	551
Scarifications.....	552
POISONS.....	553
INDEX OF DISEASES AND THEIR REMEDIES.....	569
INDEX OF PHARMACEUTICAL AND BOTANICAL NAMES.....	587
GENERAL INDEX.....	595

SANTALUM.

RED SAUNDERS.

This is the wood of *Pterocarpus santalinus*, a lofty tree, found in the East Indies. It is imported in logs or billets.

Sex. Syst. Diadelph. decand. *Nat. Syst.* F. baceæ.

Linn. Suppl. 318. Griffith, Med. Bot. 245.

Red saunders is a mild astringent and tonic; but it is chiefly, if not exclusively, employed to impart color.

SAPO.

SOAP.

Soaps are combinations of animal or vegetable oils, with one or more of the alkalies; they are principally used for washing, but some of them are officinal.

SAPO DURUS.

HARD SOAP.

This is officinal under the name of *Sapo*, in the U. S. and Lond. Phs., and is a combination of olive oil and soda.

SPANISH, OR CASTILE SOAP.

This is the officinal soap of the U. S. Ph., and presents two varieties, the white and the marbled, the first of which only should be used.

SAPO MOLLIS.

SOFT SOAP.

This soap is a combination of olive or other vegetable oils and potassa, or animal oils with the same alkali.

SAPO VULGARIS.

COMMON SOAP

Is a hard soap, made from tallow and caustic soda.

Besides these officinal soaps, there are many others, which are used medicinally, or for cleansing purposes.

ALMOND SOAP.

R. Caustic solution
of soda, one thousand parts.
Oil of almonds,
two thousand one hundred parts.

Mix, and keep the mixture for some days at a temperature of 68° F., stirring from time to time, until it acquires the consistence of a soft paste; place in moulds until dry. It should not

be used for medicinal purposes, until it has been exposed to the action of the air for a month or two. *Par. Cod.*

BEEF-MARROW SOAP.

R. Purified beef
marrow, five hundred parts.
Caustic solution of soda,
two hundred and fifty parts.
Common salt, one hundred parts.
Water, one thousand parts.

Put the marrow, with hot water, into a porcelain vessel, and heat, until it is melted; then add the solution of caustic soda, by degrees, constantly stirring, till saponification is complete; then add the salt, slightly stirring, collect the soap which rises, drain it, melt it by a gentle heat, pour into moulds, and allow it to solidify. *Par. Cod.*

SOAP OF TURPENTINE. STARKEY'S SOAP.

R. Dry carbonate of potassa,
Oil of turpentine,
Venice turpentine, equal parts.
Triturate the potassa with the oil, and then with the turpentine, until the mass has attained a proper consistence. Preserve in an earthenware vessel. *Par. Cod.*

Was supposed to be a corrector of the injurious effects of opium, hellebore, &c. It is now principally given in gonorrhœa and dropsy. Dose, eight to ten grains.

AROMATIC SOAP.

R. Concentrated solution of
soap, six ounces.
Oil of bergamot,
" lavender,
" rosemary, each, two drachms.
Mix. Employed in baths, as a tonic and antispasmodic. *Niemann.*

CAMPHORATED SOAP.

R. White soap sixteen ounces.
Boiling water, eight ounces.
Olive oil, six ounces.
Powdered camphor, one drachm.
Dissolve the soap in the water, evaporate gently to the consistence of a soft paste, add the camphor incorporated in the oil, mix well, and pour into moulds.

Said to be useful in chaps and excoriations. *Wetzler.*

COSMETIC SOAP POWDER.

R. White soap, twelve ounces.
Carbonate of potassa, two ounces.
Powdered orris root, three ounces.
" horse-chestnuts, two pounds.

Oil of lavender,
 " bergamot, each, forty drops.
 " lemon, thirty drops.
 " cloves, ten drops.
 Sugar, half an ounce.
 Mix, and form a powder. *Niemann.*

ARSENICAL SOAP.

R. Arsenious acid, two pounds.
 Carbonate of potassa, twelve ounces.
 Camphor, five ounces.
 White soap, two pounds.
 Powdered lime, eight ounces.

Reduce each to powder, and mix. Used as a preservative for specimens of natural history against the attacks of insects. Known as *Beconi's arsenical soap*. *Gannal.*

SOAP CERATE.

R. Solution of subacetate
 of lead, two pints.
 Soap, six ounces.
 White wax, ten ounces.
 Olive oil, one pint.

Boil the saturnine solution with the soap, over a slow fire, to the consistence of honey; transfer to a water-bath, and evaporate until all the moisture is dissipated; add the wax, previously melted with the oil, and mix. *U. S. Ph.*

Soap cerate is cooling and sedative.

CAMPHORATED SOAP LINIMENT.

OPODELDOC.

R. Common soap, sliced, three ounces.
 Camphor, one ounce.
 Oil of rosemary,
 " origanum, each, one fl. drachm.
 Alcohol, one pint.

Digest the soap in the alcohol, on a sand-bath, until dissolved; add the camphor and oils, dissolve, and pour into broad-mouthed bottles. *U. S. Ph.*

CAMPHORATED TINCTURE OF SOAP.

SOAP LINIMENT.

R. Soap, in shavings, four ounces.
 Camphor, two ounces.
 Oil of rosemary, half a fl. ounce.
 Water, four fl. ounces.
 Alcohol, two pints.

Mix the alcohol and water, and digest the soap in the mixture, on a water-bath, till dissolved; filter, and add the camphor and oil. *U. S. Ph.*

SAPONINE. (*For cleaning gloves.*)

R. Powdered
 soap, two hundred and fifty parts.
 Solution of chlorinated
 potassa, one hundred and sixty-five parts.
 Solution of ammonia, ten parts.
 Water,
 one hundred and fifty-five parts.

Mix, and form a paste: a small portion rubbed on a glove with a piece of flannel will cleanse it. *Duvignan.*

GREASE BALLS.

R. Fuller's earth,
 two pounds four ounces.
 Carbonate of soda,
 Soap, each, eight ounces.

Add,
 Yolks of eggs, eight.
 Well beaten with
 Ox gall, eight ounces.
 Levigate thoroughly, form into cakes or balls, and dry. *Lenormand.*

BOLUS OF SOAP.

R. White soap, two scruples.
 Oil of caraway, two or three drops.
 Syrup, sufficient.
 Mix, and make two boluses. They are purgative. *Swedliaur.*

PILLS OF SOAP.

R. White soap, one hundred and twenty-five parts.
 Marsh mallow root,
 powdered, sixteen parts.
 Nitrate of potassa, four parts.
 Beat together till well incorporated, and divide into four-grain pills. *Par. Cod.*

PILLS OF SOAP AND OX GALL.

R. Powdered white soap, two drachms.
 Extract of ox gall, one drachm.
 Mix, and incorporate
 Powdered guaiacum,
 Calomel, each, half a drachm.
 Powdered guaiacum wood, sufficient.
 Mix, and make four-grain pills. In gout, one to two, morning and evening. *Vicq D'Azir.*

COMPOUND SOAP PILLS.

R. White soap, three drachms
 Ammoniac,
 Rhubarb, each, one drachm.

Aloes, ten grains.
Assafetida,
Saffron, each, thirty-six grains.
Mix, and make three-grain pills. Purgative
and alterative; four to six a-day. *Recamier.*
R. Powdered opium, half an ounce.
Soap, two ounces.
Beat into a pilular mass, with water. Dose,
three to five grains. *U. S. Ph.*

SOAP PLASTER.

R. Soap, sliced, four ounces.
Lead plaster, three pounds.
Rub the soap into a semi-fluid state with water;
then mix it with the plaster, previously melted,
and boil to proper consistence. *U. S. Ph.*

ELECTUARY OF SOAP, &c.

R. White soap, one drachm and a half.
Venice turpentine, one drachm.
Seneka,
Marsh mallow, each, two drachms.
Rob of juniper, sufficient.
Mix. Four teaspoonfuls a-day. *Vogt.*

ESSENCE OF SOAP.

R. White soap, twenty-four parts.
Distilled water, thirty-two parts.
Alcohol (.923), sixty-four parts.
Carbonate of potassa, one part.
Essence of lemon, sufficient.
Dissolve the soap in the water and alcohol; add
the potassa and essence, and filter. Used for
the toilette. *Soubiran.*

CAMPHORATED ESSENCE OF SOAP.

R. White soap, three parts.
Camphor, one part.
Spirit of rosemary, sixteen parts.
Dissolve the camphor, and then the soap, in the
spirit. As an embrocation in rheumatic pains,
&c. *Guibourt.*

ETHEREAL SOLUTION OF SOAP.

R. Beef marrow soap, one part.
Acetic ether, five parts.
Dissolve by aid of a water-bath. Used as an
embrocation in rheumatic pains, &c. *Foy.*

CAMPHORATED ACETIC BALSAM OF SOAP

R. Common soap, one drachm.
Camphor, each, one ounce.
Acetic ether, one ounce.
Oil of thyme, ten drops.

Dissolve the soap in the ether with the aid of
heat; add the camphor, then the oil, and filter.
Used as above. *Cottureau.*

CATAPLASM OF SOAP.

R. Common soap, four drachms.
Roasted onion,
Mustard, each, two ounces.
Water, sufficient.
Heat together, and mix into a cataplasm. As
a maturing application to boils, abscesses,
&c. *Foy.*

SOAP SUPPOSITORY.

R. Soap, two ounces.
Common salt, one ounce.
Honey, sufficient.
Mix, and form into conical suppositories, and
oil them on the surface. *Spielmann.*

CLYSTER OF SOAP.

R. Barley water, six ounces.
White soap, one drachm.
Honey, six drachms.
Mix, and dissolve. *Brera.*

SOAP OF COD LIVER OIL.

R. Cod liver oil, two ounces.
Caustic soda, two drachms.
Water, five drachms.
Dissolve the soda in the water, and mix the
solution with the oil. *Deschamps.*

IODURETTED SOAP OF COD LIVER OIL.

R. Soap of cod liver oil, one ounce.
Iodide of potassium, one drachm.
Water, one drachm.
Dissolve the iodide in the water, and add it to
the soap. *Deschamps.*

SOAP WITH SULPHUR.

R. Soap, four ounces.
Sulphur, each, half a drachm.
Oil of bergamot, sufficient.
Water, sufficient.
Beat together. As an application in itch. *Frank.*

ITCH OINTMENT.

R. Brown soap, one ounce.
Common salt,
Sulphur, each, half an ounce.
Alcohol, one fl. drachm.
Vinegar, two fl. drachms.
Chloride of lime, half a drachm.
Rub well together. One-fourth to be used
night and morning, as a friction. It is effectual,
cheap, and inoffensive. *Emory.*

MOHR, REDWOOD, AND PROCTER'S PRACTICAL PHARMACY.

PRACTICAL PHARMACY. Comprising the Arrangements, Apparatus, and Manipulations of the Pharmaceutical Shop and Laboratory. By FRANCIS MOHR, PH. D. and THEOPHILUS REDWOOD. Edited, with extensive Additions, by Prof. WILLIAM PROCTER, of the Philadelphia College of Pharmacy. In one handsomely printed octavo volume, of 570 pages, with over 500 engravings on wood.

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of the present editor, who has the industry and accuracy, and, we would say, conscientiousness requisite for the responsible task.—*American Journal of Pharmacy*, March, 1854.

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THE

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The following works have been received:—

Anatomical and Physiological Observations. By JOHN STRUTHERS, M. D., Fellow of the Royal College of Surgeons of Edinburgh, Lecturer on Anatomy. Part I. Edinburgh: Sutherland & Knox, George Street; Simpkin, Marshall, & Co., London, 1854. (From the Author.)

A Short Exposition of the Circulation and Nervous System, with Reference to Disease and Treatment. By G. HAMILTON BELL, F. R. C. S. E., formerly of the Madras Medical Establishment. Edinburgh. Maclachlan & Stewart. London: John Churchill, 1854. (From the Author.)

Case of Strangulation of the Jejunum released by Gastrotomy. With Observations on the Diagnosis and Treatment of Intestinal Obstructions within the Abdomen. By JOSEPH RIDGE, M. D. Read before the Hunterian Society, June 18, 1854. Reprinted from the Association Medical Journal. London, 1854. (From the Author.)

The Modern Treatment of Syphilitic Diseases, both Primary and Secondary, Comprising the Treatment of Constitutional and Confirmed Syphilis by a safe and successful Method. With numerous Cases, Formulæ, and Clinical Observations. By LANGSTON PARKER, Surgeon to Queen's Hospital, Birmingham. From the third and entirely rewritten London edition. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

A Clinical Introduction to the Practice of Auscultation and other means of Physical Diagnosis, in Diseases of the Lungs and Heart. By H. M. Hughes, M. D., Fellow of the Royal College of Physicians, Assistant Physician to Guy's Hospital, etc. Second American, from the second and revised London edition. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

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The Principal Forms of the Skeleton and of the Teeth. By Professor R. OWEN, F. R. S. &c., Author of "Odontography," "Lectures on Comparative Anatomy," "Archetype of the Skeleton," "On the Nature of Limbs," etc. etc. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

The Principles of Animal and Vegetable Physiology: A Popular Treatise on the Functions and Phenomena of Organic Life. To which is prefixed a general view of the great Departments of Human Knowledge. By J. STEVENSON BUSHNAN, M. D., Physician to the Metropolitan Free Hospital, etc. etc. With one hundred and two illustrations on wood. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

The Pathology and Treatment of Pulmonary Tuberculosis, and on the Local Medication of Pharyngeal and Laryngeal Diseases, frequently mistaken for, or associated with Phthisis. By JOHN HUGHES BENNETT, M. D., F. R. S. E., Prof. of the Institutes of Medicine and Clinical Medicine in the University of Edinburgh, etc. etc. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

Principles of Comparative Physiology. By WILLIAM B. CARPENTER, M. D., F. R. S., F. G. S., Examiner in Physiology and Comparative Anatomy in the University of London; President of the Microscopical Society of London, etc. etc. With three hundred and nine wood engravings. A new American, from the fourth and revised London edition. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

An Examination of the Practice of Bloodletting in Mental Disorders. By PLINY EARLE, M. D., Author of "A Visit to Thirteen Asylums for the Insane in Europe;" "History, Description, and Statistics of the Bloomingdale Asylum for the Insane," etc. New York: S. S. & W. Wood, 1854. (From the Publishers.)

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The Dispensatory of the United States of America. By GEORGE B. WOOD, M. D., Professor of the Theory and Practice of Medicine in the University of Pennsylvania; President of the College of Physicians of Philadelphia; one of the Physicians to the Pennsylvania Hospital, etc. etc., and FRANKLIN BACHE, M. D., Professor of Chemistry in the Jefferson Medical College of Philadelphia; President of the American Philosophical Society, etc. etc. Tenth edition, carefully revised. Philadelphia: Lippincott, Grambo, & Co., 1854. (From the Authors.)

Auscultation and Percussion. By Dr. JOSEPH SKODA. Translated from the fourth edition, by W. O. MARKHAM, M. D., Assistant Physician to St. Mary's Hospital. Philadelphia: Lindsay & Blakiston, 1854. (From the Publishers.)

Materia Medica, or Pharmacology and Therapeutics. By WILLIAM TULLY, M. D. Vol. I. No. 12. October, 1853. Springfield, Mass.: Published by Jefferson Church, M. D., 1853.

Manual of Human Microscopical Anatomy. By A. HOLLIKER, Professor of Anatomy and Physiology in Würzburg. Translated by GEORGE BUSK, F. R. S., and THOMAS HUXLEY, F. R. S. Edited, with notes and additions, by J. DA COSTA, M. D. Illustrated by three hundred and thirteen engravings on wood. Philadelphia: Lippincott, Grambo, & Co., 1854. (From the Publishers.)

Observations on the Cause, Nature, and Treatment of Epidemic Cholera. By A. B. PALMER, M. D., Professor of Anatomy in the University of Michigan. Detroit, 1854.

Paper read before the Medical Society of the State of New York, on, first, Penetrating Wounds of the Abdomen with Punctured Wounds of the Intestines; second, Penetrating Wounds of the Larynx, and their Treatment, with Cases for Illustration. By ALDEN MARCH, M. D., Professor of Surgery in Albany Medical College. Albany, 1854.

On the Reputed Causes of Yellow Fever, and the so-called Sanitary Measures of the Day. By M. MORTON DOWLER, M. D. Fourth District, New Orleans. New Orleans, 1854.

Statistics of Consumption in Roxbury. Read before the Norfolk District Medical Society of Massachusetts, at the Annual Meeting, May 17, 1854. (Printed by vote of the Society.) By B. E. COTTING, M. D. With an Appendix. Boston, 1854.

Observations on Exhaustion from the Effects of Heat. (*Coup de Soleil*.) By H. S. SWIFT, M. D., Resident Physician of the New York Hospital. (From the New York Journal of Medicine.) New York, 1854.

Observations on the Asiatic Cholera, as it appeared in Cincinnati in 1849-50. By THOS. CARROLL, M. D. Reprinted from the Western Lancet for June, 1854.

Remarks on the Medicinal Properties of Bailey's Springs, Lauderdale County, Ala., and Cooper's Well, Hinds County, Miss. By S. C. FARRAR, M. D., of Jackson, Miss. Jackson, 1854.

Poisoned Wounds; their Distinctive Features, Classification, with Remarks upon the Classes and a Special Treatise upon the Nature and Treatment of Wounds resulting from the Bites of Venomous Reptiles, Experiments, &c., being a Report of a Committee to the Medical Association of Missouri. By A. F. JETER, M. D., of Palmyra, Mo., Chairman of Com. Quincy, 1854.

Sixth Annual Report of the Massachusetts School for Idiotic and Feeble-minded Youth. Boston, 1854.

Report of the Sanitary Committee of the Board of Health upon the subject of Slaughter-houses and Soap-boiling establishments in cities—their effects upon Public Health, together with the location of Cow-stables in cities, and the effect of feeding Still-Slops upon the Milk, &c. Made to the Board, Aug. 22, 1854, and unanimously adopted and ordered to be published in pamphlet form for gratuitous distribution. Philadelphia, 1854.

The Transactions of the Iowa State Medical and Chirurgical Society. Third and fourth Sessions, held at Fairfield, May, 1852, and Davenport, June, 1853. Burlington, 1854.

Transactions of the Medical Association of Southern Central New York, at the Eighth Annual Meeting, held at Homer, June, 1854. Auburn, 1854.

Transactions of the Fifth Annual Meeting of the Medical Society of the State of North Carolina, held at Raleigh, N. C., May, 1854. Wilmington, N. C., 1854.

Annual Report of the City Inspector of the City of New York, for the year 1853. New York, 1854.

Cataract and Amaurosis.—Clinical Lectures on some of the Principal Diseases of the Eye, delivered at the New York Medical College, in May, June, and July, 1854. By Isidor Gluck, M. D. New York, 1854. (From the Author.)

These interesting lectures reached us too late for notice in our present No.; they shall receive attention in our next.

The Physician's Visiting List, Diary, and Book of Engagements for 1855. Lindsay & Blakiston. (From the Publishers.)

We take pleasure in announcing the early appearance of this most useful—we had almost said indispensable—little volume.

The following Journals have been received in exchange:—

Annales Médico-Psychologiques, Rédigé par MM. Baillarger, Brierre de Boismont et Cerise. July, 1854.

Revue de Thérapeutique Médico-Chirurgicale. Par A. Martin-Lauzer. June, July, Aug. 1854.

Gazette Médicale de Paris. May, June, July, 1854.

Le Moniteur des Hôpitaux. Rédacteur en Chef, M. H. DE CASTELNAU. June, July, Aug. 1854.

Medical Times and Gazette. June, July, Aug., 1854.

Dublin Medical Press. June, July, Aug., 1854.

The Retrospect of Medicine. Edited by W. BRAITHWAITE, M. D. January to June, 1854.

The Half Yearly Abstract of the Medical Sciences. Edited by W. H. RANKING, M. D., and C. B. RADCLIFFE, M. D. Jan. to June, 1854.

The British and Foreign Medico-Chirurgical Review. July, 1854.

The Indian Annals of Medical Science; a half-yearly Journal of Practical Medicine and Surgery. April, 1854.

The Journal of Psychological Medicine and Mental Pathology. Edited by FORBES WINSLOW, M. D. July, 1854.

Monthly Journal of Medical Science. June, July, 1854.

Association Medical Journal. Edited by JOHN ROSE CORMACK, M. D. June, July, 1854.

The Glasgow Medical Journal. April, July, 1854.

The Medical Chronicle, or Montreal Monthly Journal of Medicine and Surgery. Edited by WILLIAM WRIGHT, M. D., and D. C. MCCALLUM, M. D. July, Aug., Sept., 1854.

The New York Journal of Pharmacy. Edited by THOMAS ANTISELL, M. D., aided by Prof. TORREY, Dr. ENDERLIN, and B. CANAVAN. July, August, September, 1854.

The Medical Reporter. Published under the direction of the Chester and Delaware County Medical Societies. July, 1854.

Iowa Medical Journal. Conducted by the Faculty of the Medical Department of the Iowa University. June, 1854.

Indiana Medical Journal. Edited by Drs. BYFORD and REYNOLDS. June, 1854.

American Journal of Pharmacy. Edited by WILLIAM PROCTER, JR. July, September, 1854.

The American Journal of Science and Arts. Conducted by Profs. B. SILLIMAN, B. SILLIMAN, JR., and JAMES D. DANA, in connection with Profs. GRAY and AGASSIZ, and Drs. BURNETT and GIBBS. July and Sept. 1854.

The New York Journal of Medicine. Edited by S. S. PURPLE, M. D., and S. S. SMITH, M. D. July, September, 1854.

The Medical Examiner. Edited by SAMUEL L. HOLLINGSWORTH, M. D. July, August, September, 1854.

The New York Medical Gazette. Edited by D. MEREDITH REESE, M. D. July, August, September, 1854.

The North-Western Medical and Surgical Journal. Edited by W. B. HER-
RICK, M. D., and H. A. JOHNSON, M. D. May, July, 1854.

Southern Med. and Surg. Journal. Edited by L. A. DUGAS, M. D. July, 1854.

The Southern Journal of the Medical and Physical Sciences. Edited by Drs.
KING, JONES, CURRY, and WOOD. July, 1854.

New York Medical Times. Edited by H. D. BULKLEY, M. D. July, August,
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July, August, September, 1854.

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The Stethoscope. Edited by Drs. ATKINSON, HAXALL, BOLTON LEWIS, etc.
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The Ohio Medical and Surgical Journal. Edited by JOHN DAWSON, M. D.
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The New Hampshire Journal of Medicine. Edited by G. L. HUBBARD, M. D.
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New Orleans Medical News and Hospital Gazette. Edited by Drs. CHOPPIN,
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The American Journal of Insanity. Edited by the Officers of the New
York State Lunatic Asylum. July, 1854.

The Boston Medical and Surgical Journal. Edited by J. V. C. SMITH, M. D.,
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Buffalo Medical Journal. Edited by AUSTIN FLINT, M. D., and S. B. HUNT,
M. D., July, Aug., Sept., 1854.

The Western Journal of Medicine and Surgery. Edited by LUNSFORD P.
YANDELL, M. D. June, July, September, 1854.

Kentucky Medical Recorder. Edited by H. M. BULLITT, M. D., and JOHN
BARTLETT, M. D. June, 1854.

Charleston Medical Journal and Review. Edited by D. J. CAIN, M. D., and
F. PEYRE PORCHER, M. D. July, September, 1854.

The Western Lancet. Edited by L. M. LAWSON, M. D., and T. WOOD, M. D.
July, August, September, 1854.

The Peninsular Journal of Medicine. Edited by A. B. PALMER, M. D., and
E. ANDREWS, M. D. July, August, 1854.

The New Jersey Medical Reporter and Transactions of the New Jersey Medi-
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Editor. August, September, 1854.

St. Louis Medical and Surgical Journal. Edited by M. L. LINTON, M. D.,
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Memphis Medical Recorder. Edited by A. P. MERRILL, M. D., and C. T.
Quintard, M. D. July, 1854.


Nashville Journal of Medicine and Surgery. Edited by W. R. BOWLING, M. D.,
and PAUL F. EVE, M. D. March, April, June, July, August, 1854.

The American Journal of Dental Science. Edited by Dr. C. A. HARRIS, A.
A. BLANDY, and A. S. PEGGOT. July, 1854.

The Virginia Medical and Surgical Journal. Edited by Drs. J. B. McCAN,
J. F. PEEBLES, and G. A. OTIS. No. XVI.

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11

CONTENTS

OF THE

AMERICAN JOURNAL

OF THE

MEDICAL SCIENCES.

NO. LVI. NEW SERIES.

OCTOBER 1854.

ORIGINAL COMMUNICATIONS.

MEMOIRS AND CASES.

ART.	PAGE
I. A Statement of the Appearance of the Parts concerned in a Popliteal Aneurism, which had been cured by Compression. By J. Knight, M. D., Professor of the Principles and Practice of Surgery, in the Medical Institution of Yale College. [With a plate.]	305
II. On Digestion of Fatty Matters by the Pancreatic Juice. By S. Jackson, M. D., Professor of Institutes of Medicine in the University of Pennsylvania	307
III. On the Gastric Juice, and its Office in Digestion. By John C. Dalton, Jr., M. D., New York.	313
IV. On Vital Force—its Pulmonic Origin and the General Laws of its Metamorphoses. By Gordon Hake, M. D., late Physician to the County Hospital, and President of the Young Men's Institute, Suffolk, England.	320
V. On the Climate and Salubrity of Fort Moultrie and Sullivan's Island, Charleston Harbour, S. C., with Incidental Remarks on the Yellow Fever of the City of Charleston. By John B. Porter, M. D., Surgeon U. S. A. (Continued from p. 75.)	341
VI. Extracts from the Records of the Boston Society for Medical Improvement. By Wm. W. Morland, M. D., Secretary.	366
VII. Cases of Paraplegia, caused by Concussion of the Spine. With Remarks. By Charles A. Lee, M. D.	400
VIII. Operation for Laceration of the Perineum. By F. M. Robertson, M. D., Lecturer on Obstetrics in the Charleston Summer Medical Institute. [With a wood-cut.]	404
IX. Cases of Ovariectomy. By Alexander Dunlap, M. D., of Ripley, Ohio.	407
X. Autoplastic Operation for the Removal of the Deformity produced by a Burn. By A. Clarkson Smith, M. D., of Columbia, Lancaster County, Pennsylvania.	411
XI. Cure of Laceration of the Urethra. By Dr. J. Gautier, M. D., of Tuskegee, Alabama.	413
XII. Case of Epilepsy, treated by Ligation of the Common Carotid Artery. By Joseph B. Brown, Assistant Surgeon U. S. Army.	415
XIII. "Neuralgia; its History, Nature, and Treatment." By Charles W. Parsons, M. D., of Providence, R. I.	417

REVIEW.

ART.

PAGE

- XIV. Trial of John Hendrickson, Jr., for the Murder of his Wife by Poisoning, at Bethlehem, Albany County, N. Y., March 6, 1853. Reported by Messrs. Barnes and Hevenor. Albany, 1853. 8vo. pp. 176. 447

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- XV. Transactions of the Medical Association of the State of Alabama, at its Seventh Annual Session, begun and held in the City of Montgomery, Jan. 10-12, 1854. 8vo. pp. 190.
- Transactions of the Medical Society of the State of Pennsylvania, at its Annual Session, held in the City of Pottsville, May, 1854. Vol. IV. Published by the Society. 8vo. pp. 171.
- Transactions of the Medical Association of Southern Central New York, at the Eighth Annual Meeting, held at Homer, June, 1854. 8vo. pp. 120.
- Transactions of the Fifth Annual Meeting of the Medical Society of the State of North Carolina, held at Raleigh, N. C., May, 1854. 8vo. pp. 97. 467
- XVI. Reports of American Institutions for the Insane.
1. Of the Mt. Hope Institution, for the years 1849, 1850, 1851, 1852, and 1853.
 2. Of the Vermont Asylum, for the years 1852 and 1853.
 3. Of the Maryland Hospital, for the year 1852.
 4. Of the South Carolina Asylum, for the year 1852.
 5. Of the Louisiana State Asylum, for the years 1849-50, and 1850-51.
 6. Of the Butler Hospital, for the year 1853. - - - 481
- XVII. On the Structure and Use of the Spleen. By Henry Gray, F. R. S., Demonstrator of Anatomy, and Surgical Curator of the Pathological Museum of St. George's Hospital. London, 1854: 8vo. pp. 380. - - 491
- XVIII. Epilepsy and other Affections of the Nervous System which are marked by Tremor, Convulsion, or Spasm—their Pathology and Treatment. By Charles Bland Radeliffe, M. D., Licentiate of the Royal College of Physicians, Assistant Physician to the Westminster Hospital, Lecturer on Materia Medica and Therapeutics at the Westminster Hospital School of Medicine, etc. etc. London, 1854: 8vo. pp. 144. - - 492
- XIX. A Clinical Introduction to the Practice of Auscultation, and other Modes of Physical Diagnosis, in Diseases of the Lungs and Heart. By H. M. Hughes, M. D., Fellow of the Royal College of Physicians, &c. Second American from the second and revised English edition. Philadelphia, Blanchard & Lea, 1854: 12mo. pp. 304. - - - 498
- XX. Auscultation and Percussion. By Dr. Joseph Skoda. Translated from the fourth edition by W. O. Markham, M. D., Assistant Physician to St. Mary's Hospital. Philadelphia, Lindsay & Blakiston, 1854: 12mo. pp. 380. - - - 499
- XXI. The Pathology and Treatment of Pulmonary Tuberculosis, and on the Local Medication of Pharyngeal and Laryngeal Diseases frequently mistaken for, or associated with Phthisis. By John Hughes Bennett, M. D., F. R. S. E., Professor of the Institutes of Medicine in the University of Edinburgh, etc. etc. Philadelphia, Blanchard & Lea, 1854: 8vo. pp. 130. - - - 501
- XXII. Clinical Lectures on Pulmonary Consumption. By Theophilus Thompson, M. D., F. R. S., Fellow of the Royal College of Physicians, London; Physician to the Hospital for Consumption and Diseases of the Chest, etc. Philadelphia, Lindsay & Blakiston, 1854: 8vo. pp. 259. . 501
- XXIII. On Gout; its History, its Causes, and its Cure. By William Gairdner, M. D. Third edition. London, 1854: 12mo. pp. 400. - - - 502
- XXIV. The Diseases of the Fœtus in Utero, not including Malformations, with an Outline of Fœtal Development. By Henry Madge, M. D., Member of the Royal College of Surgeons, Licentiate of the Apothecaries' Society, etc. etc. London, 1854: 12mo. pp. 200. - - - 502

ART.	PAGE
XXV. Principles of Comparative Physiology. By William B. Carpenter, M. D., F. R. S., Examiner in Physiology and Comparative Anatomy in the University of London; Professor of Medical Jurisprudence in University College, etc. etc. With three hundred and nine wood engravings. A new American, from the fourth and revised London edition. Philadelphia, Blanchard & Lea, 1854: 8vo. pp. 752. - - - -	503
XXVI. The Principles of Animal and Vegetable Physiology: A Popular Treatise on the Functions and Phenomena of Organic Life; to which is prefixed a General View of the Great Departments of Human Knowledge. By J. Stevenson Bushnan, M. D., Physician to the Metropolitan Free School, etc. etc. With one hundred and two illustrations on wood. Philadelphia, Blanchard & Lea, 1854: 12mo. pp. 234. - - - -	504
XXVII. Healthy Skin: A Popular Treatise on the Skin and Hair, their Preservation and Management. By Erasmus Wilson, F. R. S., Author of a "Treatise on Diseases of the Skin," "A System of Human Anatomy," &c. &c. Second American, from the fourth and revised London edition. With illustrations. Philadelphia, Blanchard & Lea, 1854: 12mo. pp. 291. - - - -	505
XXVIII. The Science and Art of Surgery: being a Treatise on Surgical Injuries, Diseases, and Operations. By John Erichsen, Professor of Surgery in University College, and Surgeon to University College Hospital. Edited by John H. Brinton, M. D. Illustrated by three hundred and eleven engravings on wood. Philadelphia, Blanchard & Lea, 1854: 8vo. pp. 908. - - - -	505

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES IN THE MEDICAL SCIENCES.

FOREIGN INTELLIGENCE.

ANATOMY AND PHYSIOLOGY.

	PAGE		PAGE
1. On the Spleen, and some Peculiarities of its Capillary System. By Fuhrer. - - - -	507	4. Blood-Crystallization. By L. Teichmann. - - - -	508
2. Functions of the Liver and Spleen. By Dr. Moleschott. -	507	5. Hämin Crystals. By Teichmann - - - -	509
3. The Influence of Dilution of the Blood on the Urinary Secretion. By Dr. Kierulf. - - - -	508	6. Influence of Cod-Liver Oil and Cocoa-Nut Oil on the Blood. By Theophilus Thompson. - -	509

MATERIA MEDICA AND PHARMACY.

7. Experiments on the Preparations of Squills. By Dr. Chateau. -	510	form and Cold as Anæsthetics. By James Arnett, M. D. - -	512
8. On Delphinin. Dr. Leonides van Praag. - - - -	511	10. Charcoal Coverlet for the prevention of Smell from Gangrenous Sores. By Mr. Wormald. -	514
9. Comparison between Chloro-			

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

	PAGE		PAGE
11. Treatment of Fever by large Doses of Quinia. By Dr. F. Corbyn. - - -	514	18. Tapeworm unsuccessfully treated by the Extract of Male Fern and Kousso. By Dr. George Patterson. - - -	524
12. Treatment of Epilepsy. By Dr. R. B. Todd. - - -	516	19. New Remedy for Tapeworm. By C. Mackinnon, Esq. . .	525
13. Chloroform in Hypochondriasis. By Professor Osborne. -	520	20. Cases of Gangrene of the Lung successfully treated by Inhalations of Turpentine. By Prof. Skoda. - - -	527
14. On the Use of Vegetable and Mineral Acids in the Treatment, Prophylactic and Remedial, of Epidemic Disorders of the Bowels. By J. H. Tucker, Esq. .	521	21. Prurigo Formicans of the Vulva and Anus. By M. Richard. -	527
15. Sulphuric Acid in the Treatment of Cholera and Choleraic Diarrhoea. By Dr. H. W. Fuller.	522	22. Successful Mode of Treating Mercurial Salivation. By Dr. Norman Chevers. - - -	528
16. Strychnine in Cholera. By Prof. W. Fraser. - - -	523	23. Diabetes Mellitus. By Dr. Th. Von Dursch. - - -	529
17. Subnitrate of Bismuth administered in Enemata for the Cure of Diarrhoea and Inflammation of the Colon. By M. Trousseau.	524	24. Betz on the Thyroid Asthma of Young Children. - - -	530

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

25. Modern Philosophy of Cancer. By Robert Druitt, Esq. - - -	531	very early Period. By Henry Smith, Esq. - - -	544
26. Arsenical Injections in Cancer of the Uterus. By Mr. Lloyd.	541	29. Paracentesis Thoracis. By Dr. J. R. Bennett. - - -	545
27. Amputation at the Knee-Joint. By William Fergusson, Esq. -	542	30. Excision of Ovarian Tumours. By Prof. John Erichsen. - -	546
28. Advantages of Operating in certain cases of Hæmiplegia at a		31. Ununited Fractures. By Edward Stanley, Esq. - - -	547

OPHTHALMOLOGY.

32. Fascial Anæsthesia, with Simultaneous Destruction of the Eye. By Dr. R. Taylor. - -	550	34. Ophthalmia in Nursing Women. By Dr. Mackenzie. -	553
33. Complete Amaurosis following Retinitis, successfully treated with Strychnia. By Daniel Griffin, M. D. - - -	552	35. Surgical Removal of Opacities from the Cornea. By M. Szokalski. - - -	554

MIDWIFERY.

36. Paralysis occurring during Gestation and in Childbed. By Dr. Fleetwood Churchill. - -	555	39. Duration of Pregnancy. By Dr. J. Mathews Duncan. - -	564
37. Inversio Uteri taking place during Labour. By Dr. G. Johnston.	560	40. Case of Pregnancy in a Rudimentary Horn of the Uterus, with probable Advance of the Ovum from the right Ovary into the left Horn of the Uterus. By Prof. Rokitsanski. - - -	565
38. Phlegmasia Dolens in the Non-Puerperal State. By Dr. McClintock. - - -	564		

HYGIENE.

	PAGE		PAGE
41. Lead Cisterns and Pipes in a Sanitary point of View. By		Robert Dundas Thompson, M.D. -	566

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

42. Poisoning by Solanin. By Dr. Fraas. -	568	44. Detection of Blood-Stains on a Knife covered with Rust. By M. Daubrawa. -	574
43. Causes of Death from the Use of Anaesthetic Agents. By Dr. K. King. -	569		

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

Puncture of the Bladder through the Symphysis Pubis. By Dr. Brander. -	575	Case of Rupture of the Uterus at the Moment of Delivery. By Huttersly P. Worthington, M.D.	578
Case of Narcotism from Opium. By W. R. Bullock, M.D. -	575		

DOMESTIC SUMMARY.

Case of Total Inversion of the Uterus, in which Extirpation of the entire Organ was successfully Practised. By Dr. E. Geddings. -	579	Nitric Acid as an Anti-Periodic. By Dr. Geo. Mendenhall. -	581
Treatment of Rheumatism. By Dr. Fuller. -	580	Remedial Properties of Simaba Cedron. Dr. S. S. Purple. -	582
Hemeralopia. By Dr. C. S. Fenner. -	581	Amputation at the Shoulder-Joint. By Dr. M. Carnochan. -	582

ERRATA.

Page 331, line 15, *for* 7,688 *read* 7,888.

“ “ “ 19, “ *facts read* fats.

“ 334, “ 33, “ *arrests read* arrest.

“ 336, “ 24, “ *besides which read* besides those which.



FIG. 1.

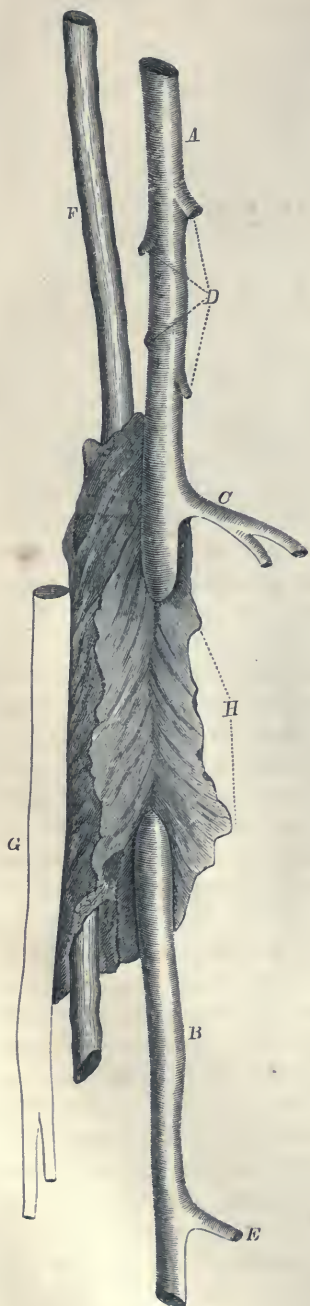


FIG. 2.



Parts concerned in a Popliteal Aneurism after cure by compression.

THE
AMERICAN JOURNAL
OF THE MEDICAL SCIENCES

FOR OCTOBER 1854.

ART. I.—*A Statement of the Appearance of the Parts concerned in a Popliteal Aneurism, which had been cured by Compression.* By J. KNIGHT, M.D., Professor of the Principles and Practice of Surgery, in the Medical Institution of Yale College. [With a plate.]

IN the number of this Journal for July, 1848, taken from the *Boston Med. and Surg. Journal*, of May 10, 1848, will be found the case of H. Johnson, in which a popliteal aneurism was successfully treated by pressure. The case was one of the ordinary kind of popliteal aneurism, the peculiarity of it being the manner in which compression on the artery was made by the fingers of assistants instead of instruments, many of which were tried and discontinued, on account of the severe pain produced by them; and the speedy and easy cure which was effected, the patient scarcely suffering from pain, and the treatment completed in about forty hours.

A statement of the progress of this case, together with the appearance of the parts after death, is of sufficient interest to be recorded. I am not aware that there is any published account of the state of the arteries and other parts concerned in an aneurism, after a cure by compression. If there is any such, it has escaped my notice. I regret that in this case a more perfect examination could not be made; and especially that the vessels could not have been injected before they were removed, so that the number and course of the anastomosing arteries could have been particularly described. This, however, could not be done, and the only means allowed us, was to remove the parts from the popliteal space, and to examine them afterwards, at our own leisure. This was done with a good deal of care, and the accompanying drawing is a correct representation of the arteries and neighbouring parts.

In the statement of the case, it is mentioned that after the cure was effected,

the femoral artery could be felt pulsating naturally down to the point of its passage through the tendon of the adductor longus, and not beyond this, and that large subcutaneous arteries were noticed upon the inside of the knee. At a little later period, the anterior and posterior tibial arteries, which could not be felt for a time, were distinguished pulsating distinctly, but not very strongly. This state of the arteries continued during life. About six months after the cure of the aneurism, he injured the leg by a blow, and there resulted an ulcer over the tibia, which was at times exceedingly troublesome, and continued until his confinement by his last sickness, when it healed. The disease of which he died was a schirrous tumour, involving the anterior portion of the stomach, from the pylorus to the great extremity. He suffered from this about six months before his death, and died exhausted by great pain, vomiting, and inanition. He died about seven years and a half after the operation.

The parts from which the drawing was made, were prepared with much care. The vessels, especially the arteries, were repeatedly inflated under water, that every branch might be discovered and tied, when they were injected with coloured wax; so that I am confident that no arterial branch escaped detection. The disease was on the left leg, and the view given in the drawing is the anterior face of the parts, that which looked towards the bones; the artery being the most deeply seated, the vein upon the right, and the nerve still further to the right, and more superficial, as measured from the surface of the limb. In the drawing, as they are viewed from before, they appear in a position the reverse of this.

It will be seen that the femoral artery is of its full size, quite down to the place of the tumour. From this point up to its passage into the abdomen, either where the pressure was made, or elsewhere, there was nothing unnatural in its appearance. Below the tumour, there is a portion of the artery, about two inches in length, before the first branch at E is given off; there the artery was somewhat smaller than natural, and its coats were slightly thickened. It is commonly and truly in most cases stated, that an obstructed artery becomes impervious to the place of the origin of its first branch. This is true in the present case of the artery above the tumour, while below there is a portion of it two inches in length, with its canal fully open, and of nearly the natural size, before the origin of its first branch. As this was a point to which our attention was especially directed, there can, I think, have been no mistake about it.

The remains of the aneurismal tumour, seen at H, not only occupied the space between the extremities of the obliterated artery, but extended three-fourths of an inch or more upon the arteries above and below, and was closely adherent to the accompanying vein. When freed from the surrounding cellular tissue, to which it adhered firmly, it was about three-fourths of an inch thick, of a tawny hue, slightly tinged with red, and of a dense, firm, almost fibrous texture. I could discover in it none of the peculiar arterial tissue.

DESCRIPTION OF THE PLATE.

FIG. I.—A. Femoral artery, above the tumour.

B. Artery below the tumour. This is shown a little larger than it is in the preparation.

C. Large anastomosing branch passing around upon the inside of the knee.

D. Smaller branches.

E. First branch given off below the tumour.

F. Femoral vein, of the natural size, with many branches, which are not shown.

G. Popliteal nerve.

H. Condensed membrane of a firm fibrous texture, filling up the space between the extremities of the artery, and involving the vein. This was about three-fourths of an inch thick, and was all that remained of the aneurismal tumour. There was nothing of it between the ends of the artery that could be distinguished as arterial texture.

FIG. II.—This shows at A, the size and form of the inside of the artery, the injected material being laid bare by an incision through the coats of the artery.

ART. II.—*On Digestion of Fatty Matters by the Pancreatic Juice.* By S. JACKSON, M. D., Professor of Inst. Med. in the University of Pennsylvania.

THE different digestions or chemical processes to which the different kinds of food—albuminous, oleaginous, saccharine, or amylaceous—undergo in the alimentary canal, have been the subject of repeated investigation and experiment since Spallanzani first demonstrated them to belong to the domain of chemico-physical science.

Prout determined the differences in the nature of the alimentary substances. His observations were followed by the more accurate researches of Mulder, Dumas, Becquerel, Liebig, and the pupils of the Geissen school. They investigated the chemical elementary composition, the modes of production, decomposition, metamorphoses, and other relations of those substances. The way was thus opened for acquiring a more correct knowledge of the kind of chemical process, or digestion, appropriate to each kind of aliment, and the seat or organ in which it occurred.

Approximations are being made to the solution of these interesting problems, which are not devoid of practical importance. They still remain unsettled and obscured from the conflicting statements, as to facts, of different observers, entitled equally to confidence from their intelligence, skill, and truthfulness.

It is not my intention to enter on the general question of the alimentary digestions. I shall confine myself to one alone, that of the oily or fatty matters, respecting which there exist opposing statements as to the observation of a simple phenomenon, which is, however, the principal fact of the process.

MM. Cl. Bernard and Bousingault, and Bouchardat and Sandras, had shown that these substances were converted into an emulsive fluid. This emulsion, absorbed by the lymphatics of the villi (to which the term lacteals, in my opinion, alone is properly applicable), constitutes the milk-like fluid, named chyle, found in the lymphatics of the mesentery after a meal containing oleaginous principles; and at no other time. If the term chyle is restricted to this milk-like fluid, which gave origin to it, then chyle is not the general product of digestion, as it is generally supposed to be, but is the fatty matters of the food in a state of emulsion, and nothing more.

Subsequently, M. Cl. Bernard demonstrated that this emulsion is produced by the action of the pancreatic juice: and he assumed it to be the essential agent in its formation. This demonstration appeared conclusive, from one of the most neat and beautiful observations in physiology.

It is known that in the rabbit, the insertions of the hepatic and pancreatic ducts in the duodenum, are separated by an interval of near six inches. M. Bernard availed himself of this fact to observe, in nature—in situ—the seat and act of the conversion of fats into an emulsion. He accordingly introduced into the stomach of rabbits, after fasting, some oil along with green food, and after the lapse of a period of time, not related, the animals were opened, and the intestines exposed. No chyle or emulsion of fats was found in the mesenteric lymphatics at and below the opening of the hepatic duct, while they were distended with the chylous or fatty emulsion at and below the insertion of the pancreatic duct. Further, when the pancreatic juice is added to sweet-oil or other fluid fat, an emulsion is instantly produced; and this emulsion is very perfect and permanent. The conclusion from this experiment was, that the emulsion of the oil was the immediate result of the action of the pancreatic juice.

The correctness of this observation of M. Bernard, has been denied by MM. Bidder, Schmidt, Lehmann, Donders, and Herbert. It is asserted that M. Bernard was misled by not properly timing his observations. It is stated, that if the animal be examined one or two hours after the ingestion of oil into the stomach, the lymphatics between the biliary and pancreatic ducts are then tinged with white fatty emulsion, which, consequently, is formed independent of the pancreatic fluid. When the examination is delayed for five or six hours after the introduction of oil into the stomach, it is alleged that the emulsion formed in the upper portion of the duodenum has then descended, and the portion absorbed has disappeared from the lymphatics, having passed on to the thoracic duct. By this time, the emulsion having reached the region of the pancreatic duct, and passed into the jejunum, the lymphatics of the mesentery of this part of the intestines are necessarily turgid with emulsified fat. It is

inferred, therefore, that M. Bernard did not examine the animals on which he experimented under the period of five or six hours after giving the oil, and was thus led into a misrepresentation of the true character of the phenomena.

This simple fact, it would seem, ought to be settled without difficulty; but the reports of those who have observed the phenomena are entirely adverse to each other. My engagements in the practice of my profession interfere with experimental researches; they prevent the application of time requisite to success in experiments. Dr. James Corse obligingly offered to perform the experiments for me, submitting to my observations the phenomena on the opening of the animals.

The following are the results of this investigation:—

A number of rabbits were procured. They had been fed for some days on hay. Four of them were deprived of food for three days. Three drachms of fresh sweet-oil were then introduced into the stomach of each animal, through a tube, successively, at nine, ten, eleven, and twelve o'clock, and immediately fed with some peas. At 2 P.M., varying from five to two hours after giving the oil, they were made insensible with sulphuric ether, the abdomen opened, and the intestines and mesentery spread out.

In one only had any emulsion of the oil been formed. In that one the mesenteric lymphatics, commencing half an inch below the pancreatic duct, were turgid with white emulsive fluid. Not a single lymphatic could be seen in the transparent mesentery between the pancreatic and hepatic ducts.

On examining the stomachs, they were found distended, in each rabbit, with a green, comminuted mass of vegetable matter, which must have proceeded from the hay on which they had been previously fed, and had remained undigested. In one, a portion of the oil was free in the stomach. In all the others, most of it appeared to have been absorbed and retained by the large vegetable bolus filling up the stomach.

The above experiments indicate a source of fallacy in making this observation. The food on which the animal has fed may interfere with its passage through the stomach, and cause a longer retention of it there than is suspected.

Second Series of Experiments.—To obviate the cause of failure in the preceding experiments, the remainder of the rabbits were fed for eight days on green cabbage. It was found that they suffered, and two died, under this exclusive aliment. It was necessary to add wheat-bran, which was eaten with avidity, and soon restored them to a good condition.

Four more were selected and kept without food for two days. They were then fed with cabbage saturated with sweet-oil, at intervals, from 9 A.M. until 2 P.M. In this manner oleaginous matter was constantly entering the digestive apparatus for five hours, until the moment of observation. If oils are emulsified in the stomach or duodenum, independent of the pancreatic juice, the lymphatics above the connection of the pancreatic duct, would, of course, be found to contain the emulsion.

The animals were destroyed by the same process as in the foregoing experiments.

The first rabbit was feeding up to the time it was killed. The lymphatics between the hepatic and pancreatic ducts were invisible, except two, from four to six lines above the pancreatic duct. They appeared as faint white lines. Below the pancreatic duct they were distended with chyle.

The second rabbit, under the same circumstances, presented the same appearances. No chyle was contained in the lymphatics until within a few lines of the pancreatic duct, where two or three were faintly visible. Below the duct they were turgid with chyle.

The third, a well-grown rabbit, gave precisely the same results. There was no appearance of chyle in the lymphatics of the mesentery, except within a few lines of the pancreatic duct, while below they were well filled with the emulsive fluid.

The fourth, also a large, strong animal, presented corresponding appearances. No chyle in the lymphatics, from the commencement of the duodenum until within a few lines of the pancreatic duct; below it they were distended with milky emulsion.

The above experiments corroborate the observations of M. Cl. Bernard, as respects the phenomena in rabbits, and do not accord with the statements of the German physiologists, MM. Bidder and Schmidt, Lehmann, and Frerichs.

An experiment relied on by MM. Schmidt, Bidder, Lehmann, and Frerichs, in opposition to M. Cl. Bernard, is wholly inconclusive. After tying the pancreatic duct in one instance, and the small intestines below the pancreatic duct in another, milk, or milk with olive-oil, was injected into the intestines, "and the lacteals were filled with white chyle." This was to be expected. What is the necessary condition for the absorption by the lacteals of the villi of fatty substances? That they exist as an emulsion. Milk is a natural emulsion, holding a fatty substance (cream) in suspension, and consequently in the condition for absorption. Milk forms an emulsion with oils, and creates the same condition. Still more extraordinary is the experiment of Frerichs, quoted against M. Bernard by Lehmann, in which he injected an *emulsion of oil and albumen* into the ligated intestine, and its absorption was considered as a refutation of M. Cl. Bernard's doctrine.

The albumen of egg forms a most perfect and a persistent emulsion with oils. The pure pancreatic juice does the same. This I have witnessed, and can state that the oil does not "soon separate again on the surface," as Lehmann asserts. The pancreatic juice, as Lehmann shows, differs very slightly from pure albumen. No other intestinal fluid possesses this character so strongly, and no other is as well adapted to emulsify the fats of our aliment.

The cystic bile is viscid from containing a larger amount of mucus derived from its mucous membrane, and by agitation forms an emulsion with oils. This emulsion has always appeared to me very inferior to that obtained from pure pancreatic juice, or albumen of egg. Neither is it as persistent, the oil

beginning in a few hours to separate. The hepatic bile is much thinner and contains much less mucus. It is unknown whether it will form an emulsion with fats. It may be considered as very doubtful. No positive facts are known as to the quantity of cystic bile that enters into the duodenum during digestion. The probability is, that it is very small in comparison to the amount of freshly secreted hepatic bile coming directly from the liver. This view gives additional confirmation to M. Bernard's doctrine.

The liquid albuminose resulting from the digestion of the albuminous principle of the food, which is found often in the intestinal canal during digestion, may, from its accidental presence, concur occasionally in the formation of the emulsion of oils. The pancreatic juice is, in the present state of our knowledge of the facts, the only intestinal humour that can exercise this office as a permanent function.

I recall a circumstance, now some twenty years past, an interesting fact in this discussion, which, at the time, was inexplicable. I assisted Dr. John Webster, late Professor of Anatomy in Geneva Medical College, in the autopsy of one of his patients, who had died after long suffering, and in a state of extreme marasmus, from severe and intractable dyspeptic symptoms. Cream had been the principal nutriment for several weeks, as rarely any other food would be retained. The disease proved to be a scirrhus of the pancreas. The colon was filled with a large quantity, some two or three pounds, of a yellow fatty substance, resembling a mixture of yellow ochre and butter. A large proportion of the fatty bodies of the cream had undergone no emulsive action from the absence of the pancreatic juice, and were not absorbed.

M. Cl. Bernard came to the conclusion that the fats were decomposed by the pancreatic juice. This is not probable. The emulsion of fats is a physical, not a chemical process. It consists of the mechanical division of fats into the minutest globules, each coated with a delicate film of the emulsifying body—albumen in chyle—casein in cream. If the experiment of Bouchardat and Sandras be correctly reported, it is conclusive on this point. They gave to a dog a considerable quantity of sweet almond-oil. After some hours the animal was killed. Some of the oil, unchanged, was found in the stomach and intestines. The lacteals were filled with white chyle, as also the thoracic duct. A portion obtained from this last was treated with sulphuric ether, and the almond-oil was obtained unchanged in its properties.

The principal fact relied on by the above German physiologist to controvert M. Bernard's theory of fatty decomposition by the pancreatic juice, is of no value. Butter was given to cats and dogs, and butyric acid was sought for, but not found in the intestines. This is considered as proof that the butter was not decomposed. It proves nothing of the kind. Butyric acid readily decomposes. It cannot resist the activity of the numerous chemical reactions at work in the alimentary canal during digestion. Lactic acid is being formed incessantly in the lungs and intestines from the metamorphosis of

glucose, sugar, and starch; it is as speedily decomposed. The same occurs with the bile. In the fecal matters, in a normal state of the bowels, very little of the biliary constituents, other than the colouring matter, is to be found. The conjugate biliary acids—glyco-cholic and tauro-cholic—disappear and are destroyed. Glycocol is not found; taurin, occasionally detected in the middle of the alimentary tract, disappears at the lower portion. The albuminous materials of the food that pass beyond the stomach, are metamorphosed in the intestines. In this conflict of molecular and chemical reactions, it would be impossible for butyric acid to exist. Its absence is no evidence that it was not eliminated.

The strongest objection to M. Bernard's doctrine is, that in an emulsion of oil formed with pure albumen the fatty body is not decomposed. An emulsion is a physical, not a chemical process.

It consists in the reduction of oil to the minutest particles—globulets—each covered with a coat of albumen when it is the emulsifying body. This is all that is required to impart to fats the capacity for absorption into, and their passage through tissues.

The adoption of M. Bernard's doctrine involves an inexplicable difficulty. If the neutral fats are decomposed by the pancreatic juice before, or at the time of absorption, as they are found to exist in the adipose tissue in their original state of neutral fats, they must be recomposed again in the blood, or at the instant of their secretion. To accomplish this change there must be a generation of oxide of lipyl and glycerin, the bases of neutral fats. It is difficult to understand how it can occur. It is true, Mulder has offered a very ingenious hypothesis to show the possibility of the formation of those bodies, in the economy, from lactic acid. But the whole matter is so entirely hypothetical, in its present state, that it does not comport with the more rigid canons of modern medical philosophy, and cannot be accepted.

The state of our knowledge may be summed up in the following conclusions:—

1. Liquid fats are not miscible with the aqueous albumino-saline fluid—liquor sanguinis—with which all the vascular tissues are saturated; it cannot enter their pores, and consequently cannot be absorbed.

2. Liquid fats, when emulsified by albumen, are reduced to minute particles, each coated with albumen. In this state they are miscible with the liquor sanguinis, moistening the tissues, can enter their pores, and are then capable of absorption. This is the sole condition requisite for the absorption of fats.

3. The white milk-like fluid, named chyle, is this emulsion of the fatty matters of the food mixed with the ordinary lymph always contained in the lymphatics of the alimentary canal, and other abdominal organs and mesentery. The molecular base of Gully is the microscopic appearance in the chyle of the minute globulets of fat coated with albumen.

4. Albumen forms a perfect and persistent emulsion with oils. The pancreatic fluid is a saturated albuminous solution, and forms with oils an emulsion equally as perfect and permanent as that of albumen.

5. The pancreatic juice is the only highly albuminous fluid in the alimentary canal, that can accomplish the formation of a perfect emulsion; and the opinion of M. Cl. Bernard, that this process is one of its functions, is, it appears to me, sustained.

6. The observations of M. Cl. Bernard, that the formation of the emulsion of fats in rabbits, is at and below the pancreatic duct, and not above it, is confirmed by the experiments reported in this communication. And, further, that the experiments on rabbits are the most reliable, as being a true exemplification of the natural process, unattended with violence and torture to the animals, more or less disturbing in their effects.

7. That M. Cl. Bernard's view of the decomposition of fats by the pancreatic juice is not proved, is opposed by the nature of the process, and by analogy with other emulsions: it is unnecessary to the accomplishment of the absorption of fats, and introduces other and complicated processes that are unknown to exist, and are mere hypotheses.

ART. III.—*On the Gastric Juice, and its Office in Digestion.* By JOHN C. DALTON, JR., M. D., New York.

THOUGH the gastric juice has been more fully and perhaps more successfully studied than any other of the digestive fluids, there are still some contradictory statements with regard to it, even among authors of the first eminence. Since the admirable observations of Dr. Beaumont on the person of Alexis St. Martin, the secretion has been examined, on the lower animals, by Blondlot, Schwann, Wasmann, Bernard, Lehmann, and others, whose researches have, in most essential points, confirmed those of the American observer. During the past year, I have studied the gastric juice, at various times, by means of fistulæ, artificially established in the stomach of the dog, after the method of Blondlot and Bernard. The secretion was almost always obtained by feeding the animal with raw or cooked meat, after a twelve hours' fast, and collecting and filtering the fluid that ran from the fistula during the first half hour afterward. The statements of Dr. Beaumont with regard to the intermittent character of the secretion were always found to be correct. The stomach, in the intervals of digestion, contains only a colourless mucus, usually alkaline in reaction, though sometimes neutral. The acid fluid begins to run by drops in about five minutes after the introduction of food, and in ten to fifteen minutes it flows freely—occasionally in a small stream—from the orifice of the fistula. It then runs so fast that four or five ounces can be collected from a medium-sized dog in the course of half an hour. During the first five minutes of its flow it is sometimes colourless, but afterward it

has a faint amber tinge, resembling that of pale urine or the serum of the blood.

A little confusion of ideas seems to have existed in the minds of some observers with regard to the best method of obtaining gastric juice in a state of purity. Several have thought this could be best accomplished by irritating the gastric mucous membrane with substances not liable to be attacked by the secretion; as, for example, glass rods, metallic sounds, &c. Dr. Beaumont appears to have sometimes succeeded in obtaining a tolerably large quantity of fluid by this means. I have myself, however, never been able to obtain a sufficient quantity of the secretion in the dog by any other means than the introduction of food. The stomach will often refuse to secrete a single drop of acid liquor, though irritated almost constantly for a quarter of an hour by a metallic catheter. Other observers, also, subsequent to Dr. Beaumont, are all agreed that only a very small quantity can usually be collected by such means; and yet they continue to recommend and employ the method, under an idea that the gastric juice so obtained will be "purer" than that procured in any other way. A moment's consideration, however, will show that the fluid which is secreted under the influence of so artificial and unnatural a stimulus cannot be at all relied on as representing the real gastric juice; and that the only proper method of obtaining this is to present to the stomach its natural stimulus, viz. the food which its secretion is intended to digest. The mistake, however, has been frequently made, and will undoubtedly account for some incorrect statements with regard to the specific gravity of the gastric juice, its non-coagulability by heat, &c. Frerichs, for example, in *Wagner's Encyclopædia of Physiology* (Art. *Digestion*), directs that, in order to obtain a "pure" gastric juice, the animals in whom gastric fistulæ have been established "should be fed, while fasting, with *indigestible substances, pebble-stones, pepper-corns, &c.*, and the fluid from the stomach then examined." A singular attempt, certainly, to excite a "normal" secretion by means of substances which are altogether foreign and unnatural to it, and with which it has no physiological relation. The truth is, these authors make a pure assumption when they regard the fluid so collected as more "normal" than that obtained by other means. They do not seem, either to attach sufficient importance to the fact that these irritants can at best produce only a very scanty supply of the secretion. Frerichs himself says he has only been able to obtain in this way from forty-five drops to two drachms and a quarter of "pure" gastric juice. This is not surprising, since he employed the method least adapted to excite its secretion. The secretion of true gastric juice can, in fact, be excited only by food, and by those particular kinds of food which are to be digested by it. Loaf sugar, given to the dog, will be dissolved and disappear altogether from the stomach, as I have repeatedly found, without causing any appreciable amount of secretion. The same thing may be said of starch paste, when given alone. I have even seen tough and indigestible pieces of tendon, introduced through the fistula,

expelled again spontaneously, one after the other, without causing the flow of a single drop of acid fluid; while cooked meat, introduced in the same way, produced immediately a free secretion. It is evident, then, that to procure normal gastric juice, *i. e.* the fluid normally secreted during the process of digestion, we must make use of its natural stimulants; and we cannot hope to obtain it in any other way.

There are only two possible sources of impurity for the gastric juice taken from the stomach after the introduction of ordinary food. One is an admixture of saliva. This, however, is generally regarded as of little importance, and, at any rate, is not to be avoided by any of the methods proposed as substitutes. Saliva is even liable to be swallowed in the intervals of digestion, when no food whatever has been taken. The other source of impurity, and that which has been thought more important, is the danger that the fluid taken from the stomach, even at the commencement of digestion, may have already dissolved some of the elements of the food, and thus present to the experimenter a complicated solution, instead of pure gastric juice. This, however, is not the case. If the animal be fed upon firm pieces of fresh meat, containing little blood—particularly if the fluids have been previously coagulated by a short boiling—a sufficient length of time elapses before solution begins for the experimenter to obtain an abundant supply of the unaltered secretion. The two principal products of digestion, albumen and albuminose, may be readily detected if present; and experiment shows that they do not exist in the fluid obtained by this means. There is, then, really no objection to the employment of this method; which is, indeed, the only reasonable and physiological one.

Gastric juice, obtained as above, has the following properties: Its specific gravity is 1010; its acid reaction is always decided; its viscosity is so slight as to be hardly noticeable; it becomes turbid by boiling, and by an excess of alcohol; it is not troubled by nitric acid, nor by ferrocyanide of potassium.

Its *organic ingredient*, concerning which so much has been said, is undoubtedly the most important of its constituents. Though it has never been separated in a perfectly pure state, its presence is easily demonstrable. It may be the altered mucus of the stomach, as Bernard supposes, or a mixture of various organic matters, as believed by others; but it is certainly a peculiar substance, found nowhere else in the body, and essential to the digestive properties of the gastric juice. There seems no good reason why we should not retain for it the name of *Pepsin*, first given to it by Schwann; always recollecting, for the present, that this name does not indicate its chemical character or origin, but only its physiological destination. This pepsin resembles albumen, as stated by Robin and Verdeil, in being coagulated by heat. Lehmann and Frerichs are certainly in error when they assert the contrary; an error into which they have perhaps been led by regarding as true gastric juice an unnatural fluid, obtained by the unnatural means alluded to above. The coagulum thrown down on boiling fresh gastric juice is not

albumen, as Lehmann intimates, since it is not precipitated by either nitric acid or ferrocyanide of potassium; and after gastric juice has been boiled and filtered it has lost its digestive power, though its acid reaction remains unaltered.

With regard to the *mode of action* of gastric juice, it may be confidently stated that it acts only on the albuminoid substances, and articles of food containing them—as meat, eggs, bread, nutritious vegetables, &c. It is also true that these matters are mostly converted by the action of gastric juice into albuminose, *i. e.* a substance which is not coagulable by heat, or by nitric acid, but which is best precipitated by an excess of alcohol. If the different articles of food be digested in fresh gastric juice in test-tubes, at a temperature of 100° F., it will be found that the digestible ingredients of bread are all taken up under the form of albuminose. If cheese be treated in the same way, its casein is altogether converted into albuminose, and dissolved, while its oleaginous matters are set free, and collect as a yellow layer on the surface of the solution. A very considerable proportion of raw white of egg, however, dissolves in the gastric juice directly as albumen, and remains coagulable by heat. Soft-boiled white of egg and raw meat are principally converted into albuminose, but at the same time a small proportion of albumen is also taken up unchanged.

The gastric juice *actually dissolves the articles of food*. This has been, through some mistake, recently denied by one observer, so far as regards muscular tissue; the fibres being supposed to be simply swollen and partly disintegrated, but not actually dissolved in the stomach. The contrary, however, can readily be seen, by digesting raw meat, cut into small pieces, with fresh gastric juice at 100° F. for eight or nine hours. The filtered fluid, which is perfectly clear, but rather more deeply coloured than at first, has increased in specific gravity from 1010 to 1020, or 1022; and, by the addition of six or eight times its volume of alcohol, becomes excessively turbid and opaque, like thin milk. It is true that all parts of the muscular tissue do not dissolve; and there remains constantly, at the bottom of the test-tube in which the digestion has been performed, a pulverulent, brownish sediment, consisting of the undigested portions of the meat. This is the case, also, with boiled white of egg. While the digestion is going on, it is easy to see that as the general mass of the fragment becomes transparent and gradually liquefies, there remain, scattered through its substance, numerous minute opaque spots which are not acted on; and which, when set free by the solution of the rest, produce an opalescence in the fluid, and collect slowly as a sediment at the bottom. Whether these refractory portions are afterwards dissolved by the intestinal fluids, or whether they are altogether indigestible, and destined to be discharged with the excrements, is at present uncertain.

The *quantity of albuminoid matter dissolved by the gastric juice* has been variously stated. The method which I have adopted to determine this point is the following: it was first ascertained that the fresh lean meat of the bul-

lock's heart loses by desiccation 78 per cent. of its weight; 300 grains of such meat, cut into small pieces, were then digested for ten hours in 3iss of gastric juice at 100° F., the mixture being gently agitated as often as every hour. The meat remaining undissolved was then collected on a previously weighed filter and evaporated to dryness. When perfectly dry, it weighed 55 grains. This represented, allowing for the loss by evaporation, 250 grains of the meat in its natural moist condition; 50 grains of meat were then dissolved by 3iss of gastric juice, or a little over 30 grains per 3j.

From these data, we can form some idea of the large quantity of gastric juice secreted during the process of digestion. One pound of raw meat is only a moderate meal for a medium-sized dog, and yet, to dissolve this quantity (supposing the whole of it to be digested), no less than sixteen pints of gastric juice will be necessary. This quantity, or any approximation to it, would be altogether incredible if we did not recollect that the gastric juice, as soon as it has dissolved its quota of food, is *immediately reabsorbed*, and enters the circulation with the alimentary substances which it holds in solution; so that a very large quantity of the secretion may be poured out during the digestion of a meal at an expense to the blood, at any one time, of only two or three ounces of fluid. The simplest investigation shows that the gastric juice does not accumulate in the stomach in any considerable quantity, to remain there until the solution of all the food has been accomplished, but that it is gradually secreted so long as any food remains undissolved; that portion which has been already digested being disposed of by reabsorption with its solvent fluid. There is, then, during digestion, a constant circulation of gastric juice from the vessels to the stomach, and from the stomach back again to the vessels. Or perhaps it would be more correct to say that it is only the watery portions of the juice, holding sometimes in solution the digested albumen and albuminose, that perform this circulation; while its acid and organic ingredients remain very possibly in the stomach, ready to act on a new quantity of food, as that which has been already digested is withdrawn by absorption. That this is really the case, is proved by the following facts. First, if a dog be killed some hours after taking a meal, there is never more than a very small quantity of fluid found in the stomach, just sufficient to penetrate and smear over the half-digested pieces of meat; and, secondly, in the living animal, gastric juice, drawn from the stomach when digestion of meat has been going on for six hours, contains little or no more organic matter in solution than that extracted fifteen to thirty minutes after the introduction of food. It has evidently been freshly secreted; and, in order to obtain gastric juice saturated with alimentary matter, it must be artificially digested with food in test-tubes, where this constant absorption and renovation cannot take place.

The time necessary for the digestion of food varies apparently in different species of animals. In the dog, one pound of lean uncooked meat is completely digested, and disappears from the stomach, only in from eight to nine hours. From experiments of M. Colin, of the Veterinary School at Alfort,

it appears that the process is equally long in the cat; and also, probably, in all purely carnivorous animals. Dr. Beaumont, on the contrary, found that in Alexis St. Martin, meat and bread were digested in five, four, and three hours; and even, in some instances, in an hour, or an hour and a half. This difference is undoubtedly in part owing to the influence of mastication, which is so very imperfectly performed in the carnivora. It might be supposed that, having constant opportunity of obtaining gastric juice from the dog, and of studying its effects in artificial digestion, we might easily determine the relative digestibility of the different articles of food used by ourselves. This, however, is not the case. The readiness with which a substance dissolves in gastric juice is only one element of its digestibility in the stomach. This depends also on the readiness with which it excites the secretion, and stimulates the peristaltic movements of the organ, the ease with which it disintegrates, the amount of previous mastication, &c. For the human species, then, the digestibility of various kinds of food can be properly determined only by direct observations on the human stomach, like those of Dr. Beaumont; and it will probably be long before we shall have any others so valuable, in this respect, as his.

Amylaceous substances are not digested in the stomach. It is easy to ascertain that cooked starch, artificially digested for ten hours in gastric juice, at 100° F., still exhibits its characteristic reaction with iodine. Bernard also maintains that it is equally unaffected in the stomach. Some writers, however, have thought that the well-known property of saliva in converting starch into sugar, would be exerted even in the stomach during the process of ordinary digestion. Lehmann, for example, states that "sugar may be detected, by the ordinary means, in the stomach of the animal (with gastric fistula) in ten to fifteen minutes after it has swallowed balls of starch, or after they have been introduced through the fistula." I am at a loss to understand the grounds of this assertion, as I have never met with similar results. Either cooked or raw starch, administered mixed with meat or introduced alone through the fistula (the animal will not eat pure starch) is easily recognizable by its reaction with iodine, ten, fifteen, and thirty minutes afterward. In forty-five minutes it is diminished in quantity, and in one hour has almost invariably disappeared; but no sugar is to be detected at any time. Outside the body, too, gastric juice entirely prevents the saccharine conversion of starch by saliva. A thin solution of boiled starch, mixed with an equal quantity of saliva, and kept at 100° F., is completely converted into sugar in ten to fifteen minutes; but if an equal quantity of fresh gastric juice be previously added to the mixture, there is no trace of sugar, even at the end of three hours, and the starch retains its usual properties. It must be acknowledged, therefore, that though saliva converts starch outside the body, when digested alone in test-tubes, yet it does not have this effect, so far as we can ascertain, during the natural process of digestion in the stomach.

Cane sugar is slowly converted by gastric juice, outside the body, into

grape. Ten grains of cane sugar, dissolved in half an ounce of gastric juice, and kept at 100° F., give traces of glucose at the end of two hours, and in three hours the quantity of glucose is considerable. It cannot be shown, however, that the gastric juice exerts this effect on sugar during ordinary digestion. If cane sugar be given to the dog while digestion of meat is going on, it disappears in from two to three hours, without any glucose being detected. There is a singular peculiarity, however, in the action of cane sugar, introduced into the stomach of the dog while empty. If half an ounce of loaf sugar be given to the animal after a twelve hours' fast, when the stomach contains no food or gastric juice, it almost invariably produces an immediate reflux of intestinal fluids (among which the bile is readily recognized by its color), and these promptly convert a part of the sugar which has been swallowed, so that in fifteen to thirty minutes the fluids extracted from the stomach contain an abundance of glucose. This, however, is only temporary. In forty-five minutes to an hour the intestinal fluids cease to be present, and the glucose at the same time disappears. Unaltered cane sugar, however, still remains and continues present for two and a half to three hours, when it gradually disappears without any subsequent production of glucose. During all this time, as has been mentioned above, no gastric juice is secreted; the reaction of the stomach remaining constantly neutral or alkaline.

The above experiments render it probable that even when cane sugar is taken with other articles of food, it is not converted into glucose in the stomach. We cannot, however, be positively sure of this, owing to a circumstance not generally known, viz. that *the presence of gastric juice interferes with Trommer's test for grape sugar*. If a drop of honey be mixed with a drachm or two of gastric juice, on adding sulphate of copper and potass, the solution takes a rich purple tinge instead of its ordinary blue colour, and, on boiling, changes to yellow—but no copper is deposited. A mixture of honey and gastric juice, in equal volumes even, does not reduce the copper unless it be previously diluted with water; in that case the copper is deposited as usual. I have not been able to ascertain upon what ingredient of the gastric juice this singular property depends. It is not its free acid, since, if previously neutralized by potass, it still behaves in the same way. It is not its organic ingredient, since if this be separated by boiling and filtering, it continues to act as before. Even the addition of several times its volume of alcohol does not altogether prevent the interference. A considerable proportion of glucose may still be recognized, notwithstanding the presence of gastric juice, by the change of colour from purple to yellow above mentioned, though there may be no precipitate; but a small proportion does not show itself in any way. One sixteenth of a drop of honey in one drachm of gastric juice produces no change of colour that could be at all relied on as a test; while the same proportion of honey in water reduces the copper promptly and abundantly. We cannot, therefore, detect a moderate quantity of glucose in gastric juice by this means.

It is universally agreed that oleaginous substances are not attacked by gastric juice, but are simply set free from their union with other matters. Thus, if adipose tissue be digested artificially, the walls of the fat vesicles, with the intervening areolar tissue, are dissolved, and the oil collects as a distinct layer on the surface. The oily matter of cheese acts in the same way, as already noticed. Boiled yolk of egg is digested in a similar manner. Its albuminoid ingredients are dissolved out, while the fats collect at the top of the solution, as a mass of yellowish semi-solid granules.

ART. IV.—*On Vital Force—its Pulmonic Origin and the General Laws of its Metamorphoses.* By GORDON HAKE, M. D., late Physician to the County Hospital, and President of the Young Men's Institute, Suffolk, England.

I. OFTEN as the attempt has been made to construct a rational theory of life, and frequently as its results have failed to satisfy the cravings of philosophy, this thing is certain—the efforts of physiologists to resolve the greatest of problems cannot cease so long as science itself lasts. The least fruitful works on this subject have served a purpose; they have paved a way for succeeding labourers. Even partial success was impossible at first; but now that the light of science has been admitted into every path of natural inquiry, views in strict conformity with its progress have sprung up, and, at this period, they almost yield a promise that the knot is to be untied. Of the essential nature of life itself, no more is understood now than was known in the earliest times; nor is it a subject on which speculation can be beneficially employed.

Our improved knowledge of the relations that physiological and physical science bear to each other, every day affords new facilities in the pursuit of inquiries into the living world. It has long been supposed that electricity is the active agent of the muscular and nervous functions, and experiments of the most varied kind have tended to confirm such an idea; but the hypothesis could not, until recently, have been said to have stood its ground. If at one time apparently established beyond all doubt, it has been set aside at another by arguments of the highest order. Not to pause, however, on the threshold of the present inquiry, by entering upon the history of this branch of science, it may be stated at once that Du Bois-Reymond, above all others, has finally put an end to doubt on the subject. Until the question of the electric origin of vital force had thus been settled, it would have been in vain to have proceeded with the discussion, contained in these pages, of the principles that such a question involves. The inquiry, thus as it were set free, becomes a wide one; and, to be conducted successfully, it must be extended not only to the relations of physical and vital phenomena, but likewise to the healthy and disordered states of the animal economy. The science of health affords a

clue to the phenomena of disease, and no surer proof of the accuracy of our notions can be adduced than that afforded us by testing the functions of health, so to speak, by their pathological reagents; or, in other words, by asking to what extent the properties of the one are adequate to explain those of the other. In clinical practice, such tests are found frequently to fail. This is especially true in reference to the dominant theory of animal temperature, a theory which fails signally to respond to the phenomena of heat that crowd disease, as well, indeed, as to the facts observable in the healthy condition. Now, since it is attempted in the following pages to show that vital force is a phenomenon dependent on respiration, and that animal heat is immediately consequent on the former and not on the latter, I deem it advisable, as a preliminary, to place the phenomena of animal temperature in such new point of view as the facts appear to warrant. For in the sequel it will be shown that, to acquire a correct notion of vital phenomena, it is above all necessary to know precisely what changes take place in the lungs during respiration, and in what order of sequence the temperature of the body is evolved, whether mediately or immediately, as the result of the pulmonary function.

The slow oxidation of carbon and hydrogen—elements rejected from the circulation—with the oxygen of the atmosphere, affords the only satisfactory account of whence animal warmth is derived. But beyond the discovery of this, its primary source, I do not hesitate to state that the subject of the temperature peculiar to warm-blooded animals has not been argued logically; the law through which ordinary substances are heated has been applied to animal bodies to explain their warmth, while that through which cooling takes place has been overlooked.

II. The well-defined laws of caloric teach us that a body like that of animals, if simply heated to a temperature of 100° F., and placed in a medium, like the air, of a lower temperature, would begin and continue to part with its heat to the latter, until the two reached the same thermal standard. For example, when the thermometer stood at zero, the human body, if mechanically heated, as is supposed, would sink rapidly in warmth until its temperature reached the same degree as that of the air, and like any other substance it would, under such circumstances, become frozen. The existence of a persistent source of heat to the blood, found in the slow oxidation of combustible matters within the range of a respiratory system, would not prevent the occurrence of this fatal accident to the system from cooling; the physical law would operate unless counteracted by the physiological; by a process calculated to preserve the thermo-genetic results of oxidation from the depressing influence of a lower temperature, such as almost uniformly exists in the atmosphere we live in.

Even if the respiratory process were adequate to sustain so constant a struggle for warmth against the effects of a lower external temperature, it is evident that a waste of heat would be constant, a large proportion of it would undergo radiation, and thus be lost to the economy.

III. But it should be apparent to all, who have viewed the subject reflectively, that no such cooling process does go on, except in disease or during sleep; and that the living body, in sustaining its high temperature in the midst of cold, contrary to the ordinary laws of thermotics, possesses a capacity for thus preserving its heat due to certain structures; not to the rate at which heat is generated in the circulation on the one hand, nor to the non-conducting property of the atmosphere on the other.

IV. The living body, however, under peculiar conditions, is equally liable with inorganic bodies to undergo the cooling process. This happens when it is placed in a medium of which the temperature is lower than its own, at the same time that it suffers under certain deviations from the standard of mature health, which may be thus arranged: 1. The state of infancy and old age; 2, injuries and affections of the nervous centres; 3, other forms of disease, to be specified.

V. In young animals, especially those whose nervous system is least perfectly developed, and in whose relation with the parent there is an established dependence for warmth, as in birds, the temperature of the body falls rapidly on exposure to cold; a law to the operation of which is to be traced the greater mortality of infants during the first months of existence. At a later period of life (and even in middle age, when the nervous system has been enfeebled by disease or by long-continued exertion), there is a return to the same physical law, the power to resist the influence of cold decreases.

In injuries and affections of the nervous system the physical law is observed, equally, to predominate over the physiological; depression of animal heat being found in constant relation with lesions affecting the nervous centres. Respiration, the primary source of animal heat, is found to fail in producing warmth, though the carbon of the lungs may be oxidized, whether by natural or artificial means.

Again, in those whose nervous system has been enfeebled by improper food, by prolonged muscular or mental exertion, by watching, or other cause of exhaustion, the cooling influence of the atmosphere is forcibly felt, and further, during sleep and hybernation, when a diminution of nervous force exists, a similar result obtains.

In other forms of disease besides those alluded to already, the temperature of the living body is observed to obey the physical law. In cholera, which may be adduced as a forcible example, this is strictly true; the warmth of the body in this affection has been known to sink as low as 67° F. The state of the system in this disease is that of collapse, a condition not dissimilar to that of shock from nervous lesions, and from loss of blood. The body is cold; the change effected in the circulating fluid by respiration goes on, but the temperature of the breath, tongue, and surface is equally depressed. The consequence of such collapse or nervous exhaustion is, that the living body is unable to maintain its natural warmth, though the carbon and hydrogen of the lungs is oxidized in the usual manner, and the product evolved.

VI. It appears, then, that the nervous system, if not solely, is specially necessary to the sustentation of a due warmth in animal bodies, and that it establishes in some manner a physiological law that regulates the heat of the system, and protects it from the physical influence of a lower temperature without.

It is the purpose of this paper to discover the law in question.

VII. It has been asserted above that beyond the discovery of its source in the chemical changes going on in the lungs, the temperature of animals has been unexplained, the law through which ordinary substances are heated having been applied to account for its distribution through the medium of the blood, without taking into consideration that the cooling process would in such case rapidly follow. For the general opinion of physiologists is, that heat resulting from the combustion of carbon and hydrogen in the lungs is communicated to, and diffused through the system at large, by means of the arterial current. But we know that if there be collapse, whether from loss of blood, or lesion of the nervous centres, or whatever other cause, the chemical actions, though still advancing in the lungs, are of no effect; caloric is not evolved. How is it that this condition of the system should be attended with such a result? Neither the blood nor the nervous structure can be supposed to possess any peculiar capacity for heat, for they cool with the same rapidity as other constituents of the body when the influence of the physical law is brought to bear upon them.

VIII. The theory of a temperature derivable from a mechanical diffusion throughout the body of heat generated in the lungs, is untenable as a whole, it being fraught with numerous errors of detail. In the first place, although no proof exists of the unequal conductivity of tissues, much less of parts of tissues, it is ascertained that the temperature of these is unequal and variable. The left ventricle of the heart is a degree warmer than the right, and a voluntary muscle is one or two degrees warmer during contraction than it is when in a quiescent state. The condition of the rectum, as respects temperature, is four degrees lower than the left, and three degrees lower than the right ventricle—that of the mouth is relatively the same. The hands are one degree and a half warmer than the cheeks, five degrees warmer than the groins and axillæ, three and a half warmer than the feet; and, lastly, arterial blood exceeds venous in temperature by one degree on Fahrenheit's scale.

All the tissues of the animal body equally are imperfect conductors of heat; it cannot be shown that any portion of a tissue, such as the skin, is a better or worse conductor than another, the skin of the hands, for example, than the skin of the feet. Therefore, the physical law which governs the distribution of caloric in common substances, from particle to particle, equally, throughout the entire volume, gives no explanation of the phenomena above cited as peculiar to living bodies.

These considerations render it necessary to reject the aid of the physical

law in explanation of the phenomena, and to inquire into their bearings in relation to a physiological law, hereafter to be explained.

IX. The influence of the nervous system in the generation of animal heat, suspected from the time of Haller, has not been explained. The physiological law which regulates the evolution of such heat is not discoverable in the nervous system alone; it is to be sought in its relations with the circulation of the blood. A lesion of the nervous system, and loss of blood, are equally opposed to the production of heat; collapse of animal warmth consequent on either, is due to the altered relation of the nervous and circulating systems to each other, to the suspension of their free contact, and thence of certain results of such, which will be duly described.

While the chemical sources of heat are on the one hand cut off on the occurrence of loss of blood, the power of converting these to their final purpose is destroyed on the other when the nervous system has suffered injury. The law of Chosat, based on a series of experiments, and on the earlier views of Brodie, may be here cited. It is, that the depression of animal heat is constantly in relation with lesions of the nervous system, whether they implicate the cerebro-spinal axis and its branches, or the system of the great sympathetic.

X. It remains to be asked, then, in reference to the well-known variations of animal temperature, to what extent it is possible to identify these with coincident conditions of the nervous function. If a correspondence can be traced, the relation of heat to nervous force becomes a less difficult problem to resolve.

The voluntary exercise of the muscles, which affords a good example of a nervous function, is accompanied by the evolution of heat. On this point, MM. Becquerel and Breschet performed well-known experiments with the thermo-electric multiplier; the arrangement was such as to indicate the temperature of the muscle in which the needle connected with the apparatus was inserted.

The needle being introduced into the biceps muscle while the arm was extended, it was found that on bending the latter, so as to contract the muscle, the deviation of the magnetic bar (which was proposed to indicate whatever changes in the temperature took place) was increased suddenly; and on the arm being bent anew several times in succession, a final deviation was observed equal altogether to nearly two degrees F.

This result is a parallel instance to that which occurs during involuntary muscular action; the temperature of the heart being higher than any other part of the body—and on the occurrence of rigor the depressed temperature of the skin being finally raised to fever heat, by the involuntary muscular contractions which attend it.

Thus, the temperature of a part rises at the same moment that nervous or its equivalent muscular force is expended.

An eminent authority (Dr. Edwards) holds the opinion that the increased

temperature of muscles, at the moment of their contraction, is due to an afflux of blood to them; but it is a fact, which I have witnessed myself, that, during the shortening of contractile fibres, the blood is expelled from their vessels, and their colour is lost.

XI. Increase of temperature accompanies the afflux of blood to a part; not when this is occasioned by mere mechanical impediment to its circulation—for then the blood, to whatever extent it accumulates, is of the same degree of heat as when moving in a natural current—but only when such remora is caused by the exercise of nervous functions. As examples of this may be instanced blushing, the nervous action attendant on which is emotion; and inflammation, of which the nervous accompaniment is pain.

XII. It is apparent, then, that nervous, as well as its equivalent muscular force, contributes to the operation of that physiological law which governs the distribution of animal heat, and that the integrity of the circulation is essential to its activity. The blood passes freely to the nervous structures, its course through the capillaries gives it contact with the cerebro-spinal axis, and, while a sixth part of that fluid, perhaps, is constantly traversing the brain, the whole returns to the lungs.

Thus, while in the lesser circulation those calorific phenomena commence by means of which the temperature of the body is sustained, it is due to the relation of the greater circulation and the nervous system to each other that the evolution of heat is finally effected.

XIII. The tendency of the present argument being to demonstrate that the nervous structures, though they do not take the initiative in producing animal heat, inasmuch as they do not supply the materials whence it originates—constitute the sole organization through which it is, during health, evolved and distributed, it has to be determined what is the *modus operandi* of the nervous influence thus exercised—of the physiological law?

If heat, intimately allied to the chemical action of the atmosphere on the lesser circulation, is appreciable to the system only through the instrumentality of the nervous centres, it cannot but be allowed, by those who have followed the recent developments of physical science in relation to physiology, that electricity is the form in which those centres contain the antecedent of such heat. The metamorphosis of the one into the other is of common occurrence, and easily illustrated by the experiment of the voltaic arc; but the difficulty of the subject does not consist in the phenomenal changes, so much as in the order of their succession; and for this reason I propose to trace the connection between the *oxidation of carbon in the lungs* and the presence of electrical currents in *the nervous system*.

XIV. It is an admitted principle that electricity is chemical force acting at a distance; this is easily proved by experiments; and there can be little doubt, if the question be duly considered, that the electricity of the nervous system represents the chemical force of which the lesser circulation is the seat, and the oxidation of carbon is the main source. But there is an undiscovered link

in the chain of phenomena; in the physical world this link is supplied by the electrode, but in the animal it has failed of being detected.

Still, throughout the vegetable and animal kingdoms, electrical phenomena are universal; organic conductors, therefore, exist in living bodies; these are constituted of the cells. The nervous and muscular fibres present well-known examples of conductors, and may be cited as having been viewed as such by the great experimenters on the subject of animal electricity.

The earliest and safest induction, therefore, that can be made in the research—that of the discovery of the electrodes connecting the chemical and electrical forces of the economy—is derivable from the study of cell-life itself, and from a comparison of the functions of the fixed cells of organs and the free cells of blood. In pursuing the subject through this channel, I intend to adhere to physical reasoning; being convinced that, whenever such is lost sight of, the path of truth recedes, and is only again recovered after a fruitless delay.

The cell has been truly characterized as the type of organization. All phenomena pertaining to cell structure are resolvable into the individual and relative actions of these. The result of their individual actions is their vitality; that of their relations is the vitality of some whole, such as a fibre, an organ, or an entire being. The individual life of a cell consists in the exercise of its self-nutrient office; the relative life, is the part it performs in that union of its own species which forms a whole or aggregate life. The act of nutrition in the cell is a chemical process; it consists in the conversion of one material into another; of certain elements of blood, for example, into milk or bile, and of their absorption into the secreting cell; of others, into nerve-cell or muscle-cell. Whether the metamorphosis takes place from blood into the material assimilated by the cell before or during absorption, is of no moment; the result—viz. that the assimilative act is affinitive—is the same, and indicates the exercise of chemical force.

This force, whether manifested within the cell individually, or as the united action of many cells—that is, whether acting at the seat of the changes which occur, or at a distance—is still due to chemical transformations; only, in the one case, it is called chemical, in the other, electrical force.

XV. There is only one type of cell; its forms vary, their figure and development corresponding with the place they occupy in the organization, and the office they have there to perform. Each cell, thus depending for its office on its position, has a determinate energy; the measure of this may be, in one instance, its self-multiplying force; in another, its motor; in a third, its secreting power; but the cell that reproduces itself, as in the rapid growth of the embryo, does not fulfil any secondary office in the economy, while those which perform secreting and motor functions do not reproduce themselves, but, when exhausted, are replaced by a new birth. But there is a variety of cell in the system, whose function has not been determined, the red corpuscle of the blood. All cell-structures in the vegetable and animal kingdoms are referable to an archetype, and are a proportional of such, which may be said

to consist of a cell-wall and nucleus; all contain these two elements in uncertain proportions, or one of them only. In secreting cells, for example, the nucleus is the predominating element; in those devoted to the formation of solid structures, the cell-wall; while in the free cells, or red corpuscles of the blood, examples are found of nucleated cells fully formed, as in the frog, proteus, and siren; and of cells without nuclei, as in the mammalia generally.

On the grounds of analogy, I am led to conclude that the free cells of the blood perform individual and aggregate functions like other cells, and that their office also is determined by their position in the economy.

This position is unique; it is one that exposes the cell in question to the atmosphere, and, therefore, to chemical influences, while the circumstance of its being free, points to some peculiar office that it has to fulfil.

XVI. The free cell of the blood, like any other single floating cell, is a monad, the determinate force of which, from the period of its full growth, is expended on the respiratory office; it is, essentially, a breathing cell. All cells, in a less degree, partake of this character; those approaching nearest to it are certain forms of zoophyte, which inhabit the waters. More than this, when the nature of nutrition is considered, the resemblance of cell-function is made to appear more general; all cells inhabit a medium, whether it be the waters of the globe, or the liquor sanguinis—for both the fixed and free cells occupy the latter in living bodies; and, within this medium, chemical changes, in all instances, transpire. These chemical phenomena occur universally between the cell and the medium it inhabits; between the bile, or the milk-cell, and the blood; between the monas, or the red corpuscle, and their media, together with circumambient atmosphere. The chemical phenomena of nutrition transpire in the former instances; those of nutrition, but especially of respiration, in the latter.

XVII. It is of no moment, as affecting the present argument, whether the office of a cell be that of development, secretion, or respiration; in each of these relations alike the result is chemical, a result which assimilates the functions of cells, and proves, which was to be demonstrated, that the free cell of the blood, like the fixed ones of the system, is a centre of chemical changes, and, as such, an electrophorus, like them.

The chemical changes which characterize the nutrient office in algæ and confervæ, in bile-cells, or nerve-cells, do not result in animal heat, but in another result of chemical action, electricity; a force which, when manifested thus in organic media, is denominated vital.

On the same ground, the result of chemical changes as they occur in the free cells, the oxidation of hydro-carbon, is not calorific, but electric.

XVIII. It has already been shown how inadequate in itself the oxidation of hydro-carbon in the lungs is to maintain the temperature of animal bodies; that though such should proceed with its wonted energy, the system cools rapidly when an injury occurs to the nervous centres. It was necessary, therefore, to inquire by what law the force generated at the lungs was con-

served, so as to be ultimately manifested in the form of caloric in the system at large; and the result of this inquiry, as far as it has proceeded, tends to show that such force, having a chemical origin, is rendered electric through the medium of the red corpuscles, or free cells of the blood. Such a conclusion, as I have shown, is in strict analogy with the physiology of cell-life; by pursuing the subject further, it may be seen that certain structures and elements belong to those red corpuscles which must, necessarily, convert caloric, should such be evolved, into its equivalent of electric force; a fact of deep interest in the study of cell-life at large.

XIX. The free cells of the blood, or its red corpuscles, are not homogeneous in structure, nor simple in their composition; in the first place, they are built up of hæmatin and globulin, substances of different densities, the surfaces of which are opposed; and secondly, they contain free metals, iron and potassium with sodium, besides salts of others, magnesia and lime. Now, either of these conditions is sufficient to convert chemical into electric force, without its passing through the intermediate form of caloric.

The experiments of Seebeck and others on thermo-electricity have demonstrated that all substances are capable, more or less, of converting heat into electric force. A common illustration is derived from two bars, one of bismuth, the other of antimony; these are soldered together at one of their extremities, to which point of junction a spirit-lamp is applied, while wires are attached to the opposite ends, which, by this means, may be connected with a galvanometer. On the connection being completed, the evolution of electricity is manifested by the deflection of the needle.

Any other metals will serve for the experiment, or two different surfaces of the same metal.

But instead of caloric, chemical action may take the initiative in the generation of electric force, without sensible heat being manifested. In the galvanic battery, the chemical action of the acid upon the metals causes heat to be evolved, but when the circuit is completed, heat ceases to be developed, and electricity is produced in its stead. Through the medium, therefore, of the blood-corpuscles, in which the requisite conditions are embodied, the chemical action of the atmosphere, or oxidation of hydro-carbon within the lungs, is convertible into electric force without passing through the intermediate form of heat.

XX. The free cells of blood are always in contact, from the heart to the capillary vessels, and form, therefore, continuous chains; thus, like the fixed cells of the nervous and muscular systems, they become conductors.

These conductors are the electrodes which connect the lesser circulation, or centre of chemical action with the great nervous centres, through the medium of the greater circulation.

Electricity is chemical force acting at a distance, or, in other words, manifested through a new medium, the electrodes, at the same time that the exact equivalent of force, from which it is derived, is preserved.

Thus, then, the entire equivalent of chemical force, without being converted into heat at this stage, is conveyed to the remotest parts of the system, secure from the possibility of loss through the cooling effects of exposure to an atmosphere of lower temperature than the living body.

XXI. These premises established, it is to be perceived that, except in disease, a constant supply of electricity is derived from the action of the atmosphere on the hydro-carbon of the blood; this is metamorphosed into heat at every point of the system, by a process as simple as that which converts electricity into heat, on the electrodes of the battery being brought into contact with each other.

It will be shown, however, in the succeeding paragraphs, that the electricity, thus derived, is changed into vital force on reaching the cerebro-spinal centres; and in its passage from these to the muscles, the method of its manifestation is yet further modified; that, in fact, it goes through its circuit of metamorphoses in traversing the different organs of the body along the courses of nervous fibres still retaining the same general characteristics, and that, finally, it is converted into caloric on the completion of a vital act, such as that of sensation, muscular motion, secretion, &c.

Many experimental inquirers, from Faraday to Du Bois Reymond, have insisted on the identity of electrical and vital force; none of them, however, have doubted the received opinion, which I shall prove to be fallacious, that the great nervous centres generate their own vital force. They have not, indeed, even started the question as to whether the white or the gray substance of these centres is instrumental in generating the force under discussion.

XXII. The nerve-structure is adapted, on dynamic principles, to subserve its telegraphic purpose of transmitting impressions; though not without the intervention of a force. To put the question in this form may appear startling at first sight, but, by examining it in the manner I propose to do, it will be found to contain materials for an analysis, the results of which are novel. Of the nerve itself, a simple membranous tube containing the nervous substance, it need only be premised that its semifluid medulla, so easily affected by agents that even water disturbs the arrangement of its molecules, appears to be suited to its telegraphic purposes in a special manner. This may be in some measure shown on common dynamic principles, the medulla, from its semifluid consistence, offering a medium of great mobility for the transference of an impression from one end of its track to the other. To effect the necessary change in its molecules, what would appear important in the process is, that they should be susceptible of a perfectly equable disturbance throughout their course; in a sensational nerve from the extremity to the point at which it makes contact with a central cineritious structure, and in the opposite direction in a motor. The medulla is the organ of these polar changes, which could not, however, be effected without the intervention of a vital force, any

more than the molecular equilibrium of a bar of magnetic iron could be disturbed without an inward force to take up the external impression.

XXIII. The vital force residing in nerves, and cumulative around their molecules, is not apparently inherent in their structure, but is derived by them from some central element of the nervous system. It does not strike any inquirer as even probable that nerves generate their own force; disease of the nervous centres confirming a directly opposite view by the paralysis of the branches. Upon this fact rests the solution of the whole problem; for, if the nervous fibre is restricted to the reception and transmission of currents, so also is the nervous fibre of the medullary portion of the spinal cord and encephalon. In these nervous centres the white matter has the same structure as nerves; both have a tubular envelop, a soft and pulpy consistence. The tubes are not bound together in the brain and cord by areolar tissue, as in the nerves, nor are they arranged in the same order; there is no other difference between them. Of such tubes, without intermixture of gray substance, a large portion of the central nervous masses is formed; indeed, the entire medullary structure of these organs. Two sets of the white fibres in question are traceable as continuous with the afferent and efferent nerves, so as to form one structure with them; and the large remaining series is commissural; they must, therefore, having the same structure and relations as nerves, perform analogous functions, and derive their vital force from the same source.

XXIV. The gray substance, then, is the only element among nervous masses that remains to be considered as the generator of a vital force. If the nervous centres include a structure to which this force is due, it must necessarily be the cineritious; since the medullary structures are tubular alike in the nerves, the cord, and the encephalon; are conductors of nervous currents, but not the seat of their generation. Is, then, the vital force originated in the cineritious substance?

XXV. Pursuing the same line of investigation, that of considering function in reference to structure, it is of importance to state in this place what are the characteristic features of the gray substance, and what its relations. The gray and white matter of the nervous centres, including ganglia, have no structure in common, though blended one with the other. The former contains none of the fibres of which the latter consists, but is composed of cells with nucleolated nucleoli and granular contents; of caudate nerve vesicles, and pigment particles. But for their contiguity and intimate union, the gray and white matter, therefore, might be viewed as distinct organs, so dissimilar must be their functions; indeed, they resemble each other less in structure, and probably in function, than the gray matter resembles the red corpuscles of the blood.

But the nerve-tubes are everywhere imbedded in the gray substance; the phenomena, therefore, which attend its functions are in strict relation with them, though a total dissimilarity of action must characterize the uses of the two.

XXVI. But it is necessary to proceed further in the analysis before attempting to settle the question; whether the gray substance of the nervous centres is capable of generating a vital force. In the first place, since all forces, whether vital or physical, are equivalents of each other—that is to say, since a certain sum of chemical can be metamorphosed only into a definite amount of electrical force—it is a matter of rigid import to ascertain whether an organ supposed to originate a force by means of its own structural changes—that is, through the electrolysis of its materials—is physically capable of the result attributed to it. With this view, I have endeavoured, approximately, to arrive at the relative proportions of the gray and white substances of the encephalon; and, by a dissection, performed with as much accuracy as possible, on a brain weighing 20,888 grains, I obtained the following result:—

Weight of the encephalon	20,888 grains.	
“ “ gray substance	13,000 “	
“ “ white substance	7,888 “	7,888

XXVII. The chemical analysis of the brain also contains facts illustrative of the preliminary inquiry, and these must be studied before the induction can be fairly made. While these show a less composite structure in the cortical than in the medullary substance (the ^{fact} ~~fact~~ being in a great degree absent in the former), the composition of the granular matter appears, in some respects, to resemble that of the colouring matter of the blood. But the fact of the highest import to the present question is, that water is the principal constituent of cerebral substance, the proportion it bears to the solid materials being from 70 to 88 per cent. This very expressive fact is not in the slightest degree inconsistent with the functions attributable to the medullary structures, as conductors of a vital force, but it would have a different bearing in relation to the cineritious substance viewed as the generator of such a principle. It cannot be supposed, for a moment, that the electrolysis of water within the encephalon yields electric currents which become vital under new media of manifestation; such currents, therefore, if taking their source at all in the brain, must originate in the solid residuum of the cineritious substance, and this would amount to one-fifth part of it, as a mean, or 2,640 grains.

Such an amount of solid matter, were it even possible for the whole of it to undergo electrolysis daily, would be inadequate to the purposes required. The generation of a vital force within so limited a sphere would leave no space for the true functions of the gray matter, such as the exercise of will, and other mental phenomena, for it would necessitate a ceaseless and rapid renewal of structure. In a word, were the generation of vital force to be demanded of so limited and delicate an organization, its tissue must undergo destruction in one day.

Let us for a moment reflect on the vast amount of muscular, nervous, and mental force that is expended daily in health; how much thought, speech, emotion, and bodily labour may be endured; and then ask the question whether

the electrolysis, not of between two and three thousand grains of solid matter, for such would involve the total destruction of the gray substance, but a portion of this, affords an adequate explanation of the phenomena.

XXVIII. It cannot but be perceived that the nervous system is absolutely wanting in structures suited to the production of its own force; and that, therefore, it does not present an exception, as was supposed, to the law that no organ generates its own nervous currents. Indeed, it is difficult to conceive that nature would so deviate from her economy as to use the materials of her most perfect organisms as electrolytes, when the detritus of the system, composed of the same materials, was at her command, and equally available for her purpose. A hundred grains of carbon are every hour brought to the lungs for oxidation, besides the sulphur, phosphorus, and hydrogen, which unite in different parts of the system with oxygen absorbed during breathing; and these materials, beyond all reasonable doubt, afford the only source of the vital forces, whether nervous, electric, or calorific, all of which are mutually convertible into each other, and have a common origin.

After careful consideration of the facts supplied by analogy, respecting cell-function, I am led to conclude that the red corpuscles of the blood are electrophorous, and that they are the probable media of conveyance for the electric force, arising immediately out of the oxidizing process, from the lungs and other parts of the system to the nervous centres.

XXIX. It being admitted, then, that the oxidation of carbon and other bodies is effected under circumstances that give rise to electric, and not, as was hitherto taught, to calorific currents, and that these are finally convertible into their equivalent of heat, it may be here observed that the theory which enables us to express such a result, contains within it the means of measuring these equivalents, by the calculation of the amount of oxygen absorbed, and of thus reducing the amount of vital force itself to a physical standard.

XXX. It is allowed, by all great authorities on the subject, that the currents observed in nerves and muscles, on being manifested through the medium of the nervous centres, and which have an electro-magnetic action on the galvanometer, are identical with those produced artificially by means of the battery, and which have a similar action. This discovery, so long in gaining due ground, though urged by such names as Faraday, Brodie, and Wilson Philip, is finally confirmed by Matteucci and Du Bois-Reymond. There can now be no hesitation in assenting to the proposition that electric force, generated in the lungs in the manner already explained, becomes vital force on penetrating the nervous centres. And it appears to me even probable that the seat of this metamorphosis is their cineritious substance; for it is there that the huge volume of blood given to the brain is chiefly distributed, while the peculiar cells before described, irregularly arranged, are the better adapted to insulate and render static the vital force; in fact, forming a structure totally different from that of the medullary portion of the encephalon, by the instrumentality of which the conduction of will and thought proceeds actively.

XXXI. The electric currents of the red corpuscles, then, on reaching the capillaries of the vesicular substance of nervous centres, are metamorphosed, according to this view, into vital, but no sooner are they manifested as such through their new medium than a fresh transformation occurs; once more in motion, they attain a novel development. If they escape the nerve in a sensational form, or pass from it through muscle, the result is the same—the vital force becomes again electric before finally being metamorphosed into animal heat. These changes follow an order of succession, proved to occur by experiment and explicable by aid of physical laws. The electro-motive state of a nerve and muscle is one of vital force; when it acts, the force which is evolved is electric, as shown by Du Bois-Reymond; finally, nervous and muscular action, thus circumstanced, is followed by the evolution of heat—a fact observable in emotion and many other nervous phenomena on the one hand, and made sensible to the thermo-electric multiplier on the other. It is unnecessary here to detail the well-known process of a metamorphosis of electricity into heat; it is daily witnessed at the electrodes of the galvanic battery, when the arc is formed and the current is impeded.

XXXII. Regarding the laws of their metamorphoses in this light, it is interesting to observe how intelligibly a well-known series of phenomena, believed in some manner to have a mutual dependence, may be explained. The theory of combustion, for which I propose to substitute that of oxidation simply, was inadequate to account for the whole vital heat of the system; and, as chemists do not recognize the same presence of heat during the analytic process as is found to accompany the synthetic, they were unable to put down the destructive changes, which are perpetually advancing in the animal economy, to the account of its temperature.

The process of waste involves a large amount of decomposition; that of secretion is likewise preceded by a destruction of one organic form before another, or the proper compound, is formed; as recognizable, for example, in the production of an element of bile from the red corpuscles of the blood, its fibrin, and fat. But how is it possible to determine the equivalent of heat from these analyses, and of electricity from these syntheses of the animal body? By rejecting the theory of combustion (which really occurs only during disease), and adopting that of oxidation, the matter assumes a much more simple aspect; instead of having to formulate the calorific results of the assimilative and other synthetic processes, and the electric results of the various decompositions, it is necessary only to view every organic change as attended by the evolution of its equivalent of electric force.

XXXIII. The theory thus propounded of a vital force having its origin in the chemical changes of the system, sufficient in extent to sustain it constantly, and acting through the intermediate link of electricity, confirms the opinion entertained by physiologists that this force is manifested only at the moment of contact between the blood and nervous centres. The origin assigned it, however, in this essay, though differing from that attributed to it by other

writers, cannot fail to be admitted as correct, if the facts be well considered. Of the forces pervading living matter, there can be but one main-spring; in the mean time, the nervous centres bear no resemblance to organs employed in the generation of force; their electrolysis, when it occurs, is in connection with their vegetative function. For the rest, their utmost integrity is conserved jealously, in order that impressions may be retained, and reflections performed, at the same time that the material through which so great a modification of force transpires may, without further change than is necessary to assimilation, remain true to its purpose of effecting the highest metamorphosis in nature.

That any process besides that of waste, after the repeated exercise of polar functions, should be carried on in the brain or other nervous centres, will appear impossible to those who are able duly to estimate the nature of force; to such it would appear equally probable that caloric itself should be generated without a physical equivalent. Such argument admitted, we have only to weigh the phosphates daily ejected from the system, to adopt the conclusion that the nervous centres do not yield the physical resources through which their own currents are supplied.

XXXIV. The retina presents as good a means as can be selected for examining into the working of this new theory. An image reflected upon this organ is impressed only on the surface, it does not penetrate the strata of nerve-cells which intervene between this surface and the cerebrum, unless certain conditions are present, the chief of which to be mentioned, for the present purpose, is a circulation of blood through the brain. Let this be arrested, the image may be perfect, but it is not impressed on the nervous tract, it is invisible; let the circulation be restored, and the image is seen. This experiment demonstrates to reason that the optic tract does not possess within its own structure the means of bringing its molecules or cells into the same polar state as that of the retina when an image is reflected on the latter. The optic tract, then, is in the same condition as soft iron; this, encircled by the wires of a galvanic battery, becomes magnetic, and remains so as long as the electric current circulates around it. While in this state, the whole of the metal is in harmony with its poles, and acts through them; arrest the circulation of electric currents, and the unity of action among its molecules is at an end. The circulation of the blood is to the optic tract, what the circulation of electric currents is to the soft iron.

The same argument precisely applies to the brain, and to all the nervous centres into which capillary vessels enter. The cerebro-spinal axis, the ganglia of the sympathetic, are temporary magnets; arrest the current of their circulation, and their functions are paralyzed, for the obvious reason that the electricity conveyed to them by means of the electrophori, or free cells of the blood, is cut off, instead of, as heretofore, being metamorphosed into vital force, on traversing the nervous structures.

The individual functions of all nerve-cells, like those of others throughout the economy, are simply vegetative, self-nutrient; they are responsive to each other's impressions, not through their own vital force—which is solely that of affinity for the elements of nutrition—but through the polarizing force of that vital power which the circulation brings to bear on them as a whole, and in the manner explained.

XXXV. The vital force of the brain is analogous to the physical force of the magnet; it is not identical with it, inasmuch as cerebral and ferruginous substances, acting as their media of manifestation, are totally different in structure; yet the identity is so far complete that the same electric currents are competent to produce vital force in the brain, and magnetic force on soft iron.

The brain is an organic magnet of the highest order of its class; its properties are subject to modifications from the action of new forces upon it, in the same manner, and probably through the same laws, as the properties of an ordinary magnet may be modified by a new force, such as a current of electricity, which causes a magnetic bar to be deflected.

Vital force thus bears a stricter resemblance to magnetism than to electricity, its direct source.

XXXVI. Cerebro-vital force, when united in action with other forces within the same organic medium—namely, with light, heat, and motion (for the phenomena of sound, and the contact of bodies acting on the sense of touch, are resolvable into the latter force)—a new result obtains; there are observed phenomena of sense and intelligence.

The phenomenon of mind, viewed as the ultimate result of a composition of forces, has a certain physical resemblance to these, in as far as the perception of nature is similar, in most respects, to nature; except that the former, unlike the latter, is a consciousness.

In tracing results to their antecedents, it is not possible, in the simplest class of changes, to understand a cause; we only accept the evidence it affords. Oxygen and hydrogen produce water; but we perceive the essential nature of the change as imperfectly as we see what transpires when the most complex forces in nature become compounded, and are attended with consciousness.

It is, however, a step in advance if we can show that consciousness is a phenomenon, like all others, the result of antecedents.

The brain depends on a free current of vital force for the manifestation of its properties; if such force be withdrawn, the action of the extrinsic forces is enfeebled, in the same manner as the deflection of a magnet would be slight, however free the current of electricity in the direction of its poles, if such magnet had lost a portion of its power.

The brain is an induced, or temporary organic magnet, but the supply of its vital force is constant as long as the circulation of the blood, whence it is derived, is healthy. There are states of disease in which this supply is cut

off; I would instance anæmia. In this affection, the power of attention is singularly defective; there is forgetfulness; the action of the faculties is slow, and any attempt to hurry them in their work is productive of wildness; a weak state of mind characterizes this affection.

But whether the supply of vital force be cut off at the brain or at the lungs, the result differs but slightly; it signifies but little whether chemical force be checked in its evolution during the respiratory act, or whether its equivalent in vital force be withheld at the brain owing to disease of the free cells of the blood. In organic asthma, as in ænemia, there is irritation of mind, sometimes a clouded state of the faculties; the strength is prostrated in both cases, however powerful the muscles. The memory, in these affections, is characterized, not by inability to remember, but by want of vital strength to exercise itself. Though the emotions are less vivid, and the faculties show diminished energy, in none of these is lost a consciousness of their latent power—the static force inherent in the vegetative functions of the encephalon.

XXXVII. Any approach, however slight, that can be made towards resolving the grandest of all problems—the nature and sources of the mind—confers a lasting benefit on science. The materials through which the argument in the present paper has been effected, and the method of thought that has been followed, have suggested new views which may not be unworthy of perusal.

Every ascending step in nature is won from the rude mass by those truly wonderful combinations of matter which are due to the affinities. To limit these to the chemical range, would be to take too narrow a view; there are others, besides, which unite element with element; to them, in a more composite form, is due the attraction of world for world, and even the whole harmony of nature.

We see what these affinities will effect among the inorganic elements; but how much more they perform when they give rise to structure—to the living kingdoms! It is in these that we see the result of a composition of forces the most opposite in their origin, the furthest apart.

The forces, or many of them, have the power of modifying each other, and thus of producing a result unlooked for, and wholly different in character to the properties of those agencies which were engaged in its production.

Light modifies the union of elements, both crude and organic; it gives rise to peculiarities due solely to the addition of that force, yet not in the slightest degree evident in the force itself.

Heat, that is the temperature of 100° F., is a necessary condition to the performance of living functions; to the composition and decomposition of organic elements; it enters into the process of union and separation.

Electricity, under the aspect of vital force, gives a character of its own to vital changes; without it, they could not go forward, or, if they advanced, it would be towards the production of abnormal compounds.

It may be assumed generally, that the correlative forces—those which are

mutually convertible into each other—are capable of entering into combination, and of giving rise to a composition of forces, provided that chemical force, inorganically or organically derived, be present. The forces thus mutually related are, electricity, light, heat, and chemical action, to which may be added motion and magnetism.

The forces thus instanced are in themselves compound, and severally express the activity of other bodies. Chemical force is the composite of minor affinities; so, also, is electricity, light, and heat. Either and all of these may be generated by combustion.

Now what is remarkable in reference to these forces is that, though they are correlative, identical in their essence, and different only in their medium of manifestation, they are capable of combining, as will be shown presently, and of giving rise to a new result.

But though electricity, light, and heat will not unite together directly, they are capable of doing so through a medium—through materials capable of being excited to chemical action. Instances of such are witnessed universally; not only in the laboratory, but in the living kingdoms.

XXXVIII. But, to apply this great principle to the present object, let us examine its bearings on the nervous system—on the seat of mind.

We have seen what the retina is in itself; a mere organic mirror, a congeries of organic cells whose constitution is chemical, the office of each being to maintain itself in perfect integrity by means of nutrition, to thus repair waste as constantly as such accrues from the exercise of function.

Regarding the organic cell of the retina as an organic chemical basis—a medium for the manifestation of vital force—with no other function than appertains to it as such, let us inquire into the result of certain physical forces being added. To develop vision, neither electricity, heat, nor light, alone, nor any two of these forces, is sufficient; light is necessary to the living function of the retina itself; electricity to that of the tract between it and the sensorium; heat, or the temperature of blood, to both. The physical forces thus brought to bear, in the manner described, on an organic basis, give rise to a result totally new—vision; a vital force.

XXXIX. The composition of forces like this, accruing in organic media, occasions results proportionally important. The collective experience of all the senses—that is, actual vision, hearing, &c.—becomes so many vital forces, subject to the laws of composition and decomposition, in reference to the organs of intellect, emotion, consciousness, and volition, which, in their turn, become the chemical basis, or static medium, in relation to these vital forces, for the manifestation of mental force.

Without pursuing this deeply-interesting subject into too much detail, I would thus suggest it as probable that intellectual phenomena, like those of sight, are due to a composition of forces—to the last and highest of such as outward nature and the organism combined can contribute.

XL. Before concluding this argument in reference to the mind, I would venture to make a further suggestion, as to whether emotion and sensation may not have an origin the very opposite of intellectual phenomena. The latter are due, as a basis, to the white fibres; they are reflections along these to the seats of the faculties. Not so the former, they have their seat in the gray substance, and the glow accompanying them indicates a metamorphosis of force. The senses and intellect convey intelligence; when such has traversed the white substance, in which all dynamic actions of the mind occur, they reach the gray, or seats of emotional phenomena. Is not the glow which here occurs, and which frequently diffuses itself in the form of heat over the nervous system, a decomposition of the vital forces constituting the intellectual, and a metamorphosis of one of their penultimate elements—electricity—into animal heat?

This view of a composition and decomposition of forces—which are the telegraphic agencies of every complex operation in nature, as giving rise not only to the lower forms of organic function, but finally to the phenomena of mind itself—may be worthy of consideration to those whose study of physical and physiological science as one, have led them into a similar field of investigation.

XLI. The voltaic arc, by the completion of which the electric currents, instead of passing off silently at the electrodes, are impeded, and by the change of length in their waves, converted into heat, affords a parallel of what sometimes takes place in the animal economy, especially when inflammation is present. The stasis of the free cells of the blood during the inflammatory process not only prevents their polarization from taking place, but hinders the cells in adjacent vessels from moving forward. They oscillate instead of advancing, and, being cut off from the general current, their electricity is impeded, and consequently metamorphosed into heat.

The inflammatory process has been studied with much research, but there are points connected with it impossible to be understood before a rational explanation of animal heat had been arrived at.

Let us review the great local symptoms of inflammation, pain, swelling, redness, heat. A remora of the free cells of the blood within the capillaries, explains the second and third of these; they arise from obstruction, the causes of which are foreign to the general argument; the first and last of these symptoms, however, stand in a peculiar relation to each other, a relation similar to that of vital force and animal temperature at large.

In every system, the force destined to control organic functions, undergoes a metamorphosis; chemical in the lungs, it becomes electrical in the blood-cells, nervous in the cerebro-spinal axis and the ganglia of the sympathetic, endo-muscular in the muscles, and calorific, when, as nervous or endo-muscular, its vital act is completed.

It has been shown how disease, or deficiency of the blood-cells, gives rise

to deficient vital force in the nervous centres, and how impediment to the respiratory process conduces to the same result by preventing the oxidation of carbon within the lungs, and thus curtailing the source of electricity in the system.

When vital force is thus reduced in its amount, from whatever cause, febrile states of the system are apt to arise; these are easy of explanation on the views now brought forward.

The coexisting rigor and flush, accompanied by creeping sensations of chill and heat, are indicative of a low state of vital force, and of its imperfect and irregular conversion into caloric. The balance of function is destroyed between the nervous and muscular systems; vital force is expended within the latter by its involuntary movements, or rigors; but these convert it, according to the law already described, into caloric. When shivering has thus proceeded for a certain time, the metamorphosis of vital force into caloric has sufficed to cause reaction; a hot stage. By the time that this stage subsides into that of normal temperature, the function of the lungs, circulation, and nervous system, for the performance of which such temperature is required, are resumed, and the balance is restored, at least for a time. Unless the vital force is irreparably exhausted, as in cases of cholera, the cold stage thus gradually develops the hot; the closure of the pores prevents loss of heat by evaporation, while the trembling of the muscles gradually generates it.

A low degree of vital force from disease, as well as from other causes, such as incomplete development of the nervous system in the young animal, the physical condition of age, &c., thus infallibly occasions a corresponding low degree of heat in the animal body; and from the obvious cause that one is the antecedent of the other in disease, as well as health.

Any impediment to the free action of electric currents occasions heat, both in disease and health. The distension of the vessels in inflammation, impeding their currents by pressure upon the nervous branches, causes pain, and this must be an additional source of caloric; for whenever a vital act is completed, whether sensational or motor, the result is an evolution of heat. A paroxysm of neuralgia leaves the seat of pain deeply flushed; the same effect is frequently an accompaniment of emotion, in which example the blush of the cheek is the counterpart of the same remora throughout the gray nervous substance.

In inflammation, as in health, the impeded nervous current (pain, emotion) precedes the remora (stasis of free cells—permanent, transient).

In secreting organs, the vessels are so distributed as to produce remora of a normal kind; all the capillary vessels partake more or less of this distribution, which is no doubt in some manner connected with the maintenance of a certain temperature, the existence of which is necessary to the carrying on of the vital functions generally.

Inflammation of the lungs is attended with a degree of prostration which accompanies that of no other organ, the encephalon excepted.

When the lungs are inflamed, a phenomenon occurs, which is supposed by physiologists to take place in health; animal heat is generated in them, as in any other part suffering under the inflammatory process.

In inflammation of the brain, there is an apparent increase of energy; but this is merely the expenditure of vital force already existing, and its conversion into heat. Many months may elapse before the nervous power, thus lost under encephalitis, is recovered.

Thus, disease of the lungs, the blood, and the brain, the three principal links in the chain through which physical force is metamorphosed into vital, is productive of prostration to an extent far exceeding that which arises from the disorders of other parts of the system.

XLII. According to the views expressed in this paper, all the forces that act upon the organic being have an extrinsic origin. While light comes from the remotest regions of space, and other forces are derived from surrounding media, both solid and aeriform, the vital force which polarizes these, and thus harmonizes the entire nervous system to their influence, is also derived from without, at least in part, it being due to the union of the atmosphere with the blood.

It is to be hoped, for the honour of physiology, that the doctrine hitherto held, of the nervous centres generating their own vital force, and of the lungs being a seat of combustion, will be abandoned. The one would lead to mental imbecility in the space of a day, even if it were possible for an equivalent of vital power to be derived from the solution of nervous structure; while the other would conduce instantly to inflammation.

How much more consonant is it with the well-known economy of nature to assume that she would not submit her most delicate structure to the destructive process of generating electric force, by means of its own organic changes, when the contact of the atmosphere with the debris of the system, under chemical laws, is adequate to the purpose demanded: a plan so simple as to excite wonder, and, at the same moment, so expressive of nature's resources, who, out of the waste of the organization—the carbon of the lungs—resuscitates the system, maintaining its continuous vitality by the aid of that breath of life from without, of which the supply is inexhaustible.

RACINE, *Wisconsin*, 1854.

ART. V.—*On the Climate and Salubrity of Fort Moultrie and Sullivan's Island, Charleston Harbour, S. C., with Incidental Remarks on the Yellow Fever of the City of Charleston.* By JOHN B. PORTER, M. D., Surgeon U. S. A. (Continued from p. 75.)

Etiology.—In this inquiry, it is proposed to include both Charleston and Sullivan's Island; for a comparison of the two places is desirable, in order to show that the type of disease on the island is assimilated to that of the city, which must be the case if the same causes are in action under similar circumstances. The generation of yellow fever appears not to depend on the presence of one particular cause, but on the presence or combination of several; and the following seem to have been the principal ones in 1852, as in previous years, viz:—

1. *Intemperance, or the excessive use of Alcoholic Liquors.*—We have sufficiently alluded to the pernicious effects of the free use of spirituous liquors on the system in hot climates. In all cities there is quite too much drunkenness, and Charleston is no exception to the remark, as the numerous licensed groggeries and their customers show, as well as each September election for municipal officers. Dr. Thomas Y. Simons, port physician of Charleston, uses the following language in relation to this election in 1852:—

“Early in September, the fever became epidemic, among the Irish and Germans particularly. It was during the excited elections for Mayor and Aldermen of Council, in which elections these two classes of our citizens are always brought actively into play, exposed to the influence of every species of exciting cause used under such circumstances, and who constitute infinitely the largest proportion of those who become victims of the fever.* * * So satisfied were the public authorities of the great influence of this cause, that they petitioned the Legislature to change the time of election of Mayor and Aldermen, which has been granted.”

On Sullivan's Island, the chief source of revenue is from the sale of liquor-licences; and there were not less than thirteen licensed grogshops in 1852, in a population of perhaps two thousand in summer, and as many hundreds in winter, besides illicit venders, all of whom dispensed the vilest compounds to their unfortunate customers.

2. *Fatigue and Exposure.*—Dr. Carpenter says:—

“The fatigue resulting from excessive muscular exertion is commonly accounted one of the predisposing causes of zymotic disease; and this, too, is usually supposed to operate merely in occasioning a general depression of the vital powers. All muscular exertion, it is now universally admitted, involves as its condition a disintegration of muscular tissue, the components of which normally undergo oxidation, so as to be partly eliminated by the respiratory process under the form of carbonic acid and water, and partly by the kidneys under that of urea, &c. Now, if the disintegration of muscle by exercise take place faster than the matter thus set free to decompose can be oxidated and eliminated, it must remain in the blood for a time in that very state of readiness to change which renders it peculiarly fermentable; and thus its presence

in the circulating current will give to the blood the same susceptibility to the action of zymotic poisons which it will derive from any of the causes."

The number of strangers at hard labour in Charleston during the hot weather of summer is large, and it is this class which usually suffers at the commencement of all epidemics. "There were a great number of emigrants from Ireland," says Dr. T. Y. Simons in 1852, "who arrived here during the epidemic, and soon became the victims of disease. Destitute of means, they were compelled to do laborious work, such as excavating the earth, or paving the streets, &c., and having miserable accommodations."

On Sullivan's Island, the condition, habits, fatigue, exposure, &c. of the same class of people is similar. During the summer of 1852 the garrison of Fort Moultrie was small, and for a time the men had to go on guard every alternate day, the other day being devoted to severe fatigue duty, as getting wood and water, draining the fort and parade, which had been deluged by the heavy rains, &c.; so that their fatigue and exposure were very great. The company women (laundresses) also suffered from fatigue and exposure. These two classes of persons, the enlisted men and company women, chiefly suffered from yellow fever.

3. *Imperfect Ventilation.*—On this cause of disease, Dr. Carpenter's remarks are so interesting as to justify our giving a long extract:—

"An accumulation of disintegrating matter in the system may be due, not merely to its excessive production, but to any obstacle which interferes with its due elimination; and this will especially be the case when the respiratory process is imperfectly performed. All physiological and pathological evidence tends to indicate the paramount importance of this process, not merely as regards the direct elimination, through the lungs, of a large amount of matter which is undergoing change; but also as furnishing the conditions by which the matters properly to be excreted by the kidneys are brought into the normal condition for being thus eliminated. For any prolonged deficiency of respiration necessarily lowers the general oxidating process throughout the body; and thus it happens that an undue amount of carbonaceous matter is thrown upon the kidneys for excretion, and that the highly azotized compounds are not so completely brought, as they should be, into the condition of urea. Now, that over-crowding, and consequently deficient aeration of the blood, is one of the most frequent causes of the severity of epidemics, is a fact so universally recognized by all who have attended to the subject, that we need scarcely do more than advert to it. * * * The evil results of an insufficient supply of air are not exerted merely through the imperfect oxidation and elimination of the substances which are undergoing decomposition within the system; for the same cause will operate to confine the putrescent effluvia that are given off *as such* from the lungs and skin, which will produce the effect upon the individuals habitually exposed to them, as if these were generated from some external source. It was ascertained by the experiments of Collard de Martigny, that the fluid exhaled from the lungs is by no means pure water, but contains as much as three parts in 1,000 of organic matter. If this fluid be kept in a closed vessel, and be exposed to an elevated temperature, a very putrid odour is exhaled from it; and from the recent experiments of Mr. R. A. Smith, it appears that its putrescence depends on the decomposition of an albuminoid substance. There is every reason to believe that the fluid exhaled from the skin is charged with a very similar substance; its presence being indicated by the foul odour of garments that have been too long worn. And thus imperfect ventilation becomes the means not only of preventing the due elimination of decomposing matters from the body, but actually of reintroducing its poisonous

products into the blood, by the very process which was designed for the purification of the vital fluid."

Bad or imperfect ventilation, then, may be one of the main causes of an epidemic; and the city of Charleston is peculiarly obnoxious to this cause of disease, it being sufficient in this place to merely refer to the close and ill-ventilated tenements in all parts of the city—the narrow streets, and the dirty lanes and alleys, inhabited by gregarious foreigners and families, where almost every morbid cause is in full operation.

We will quote from the *Charleston Evening News* of April 29, 1853, in confirmation of the foregoing statement in regard to ventilation:—

"We have narrow lanes into which are crowded a larger number within given spaces than should occupy them. We lay out streets, indifferent, not only to the comfort, but the health of those who are to dwell in them, at all seasons, during the prevalence of heat that enervates and predisposes to disease, as well as the cold that excludes it. We extend our municipal limits, anxious to enlarge our taxable resources without providing those places of general resort and recreation, public squares, which have been aptly called the lungs of cities. In our engrossing anxiety not to leave land unimproved, even where land is in excess, we are providing the sources of disease and death for posterity."

"Here, then, are the latent causes of contagious and malignant disorders in our cities. They are built up without regard to due ventilation. When the existing streets and thoroughfares do not admit of full improvement, from a defective plan of original construction, in opening new streets the opportunity is lost of having them of proper width, and making spaces for squares and parks that will allow of a free circulation of air. This branch of municipal legislation should receive more attention than it has heretofore met with in this locality."

On Sullivan's Island this cause of disease is not so powerful as in Charleston. The dwellings are further apart on the island than in the city, and there is a better circulation of air; but still, there is imperfect ventilation in the crowded old houses and kitchens, the floors of which are on the ground, and their timbers decayed. These decaying houses are the most liable to be crowded; and one can be pointed out which, in 1852, contained three Irish families, a free negro or two, and in the house and yard there were cats, dogs, pigs, cows, chickens, &c. The whole house was not comfortable, or decent, for one small family. In the fort the ventilation is, in general, good; best in the officers' quarters, next in the men's barrack rooms, and worst in the servants' rooms, which are on the ground-floor. The barrack rooms are on the same floor with the officers' quarters, in the second story, and are much more airy, though more crowded, than the servants' rooms. Every man had a bunk for himself. The companies being small, the rooms were not so much crowded as might be supposed; but, when compared with the officers' quarters, there was imperfect ventilation. The men, for this reason, have a practice of sleeping on the piazza in warm climates; a practice which is unhealthy, and, in the fever season, dangerous.

4. *High Solar Heat.*—This acts in two ways; on the human system, and by rapidly promoting animal and vegetable putrefaction. It is always necessary for the production of yellow fever, and always prevails in the har-

hour of Charleston. From the tables of Dr. J. L. Dawson, City Register, it appears that the mean summer heat in Charleston is great, and the Meteorological Registers of Fort Moultrie show that it is uniformly so on Sullivan's Island.

Abstract of the Detached Thermometer at Fort Moultrie for Five Years.
Average mean temperature.

MONTH.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily mean.	Highest degree.	Lowest degree.	Range.	Hottest daily mean.	Coldest daily mean.	Range.
1849.											
May	69.48	74.13	75.25	71.71	72.37	84	54	30	79	61	18
June	76.70	80.40	81.86	78.33	79.13	92	74	18	86	74	12
July	76.51	79.87	81.29	78.58	78.87	88	68	20	85	69	16
August	78.35	82.80	83.58	80.58	80.80	91	73	18	85	75	10
September	72.16	75.83	78.16	75.03	75.16	86	64	22	81	69	12
1850.											
May	66.38	73.61	76.83	70.22	71.61	84	56	28	78.0	61.5	16.5
June	71.00	79.66	81.46	75.23	76.23	93	55	38	83.0	63.0	20.0
July	79.03	85.70	88.19	82.12	83.61	98	74	24	90.0	78.5	11.5
August	79.00	85.00	87.29	81.93	83.14	94	73	21	87.5	77.5	10.0
September	73.10	78.90	82.76	77.50	77.93	91	60	31	84.0	68.0	16.0
1851.											
May	67.64	76.38	77.77	72.00	72.70	91	53	38	83.5	58.5	25.0
June	74.56	80.80	82.53	77.90	78.55	98	60	38	84.5	65.5	19.0
July	78.32	83.32	88.67	81.03	83.50	100	70	30	90.5	76.0	14.5
August	78.29	83.16	86.03	81.19	82.16	92	66	26	86.5	74.0	12.5
September	68.06	76.00	79.46	73.76	73.76	91	52	39	83.5	62.0	21.5
1852.											
May	67.74	77.22	78.12	72.12	72.93	93	49	44	81.5	57.0	24.5
June	71.13	78.50	80.93	75.90	76.03	99	63	36	86.5	63.0	18.5
July	76.29	83.41	85.35	80.54	80.82	92	72	20	85.5	76.5	9.5
August	74.38	81.87	84.03	78.87	79.20	95	66	29	85.5	68.0	17.5
September	70.50	77.83	79.00	75.70	74.75	92	61	31	81.0	67.5	13.5
1853.											
May	68.32	75.96	76.77	72.54	72.54	84	56	28	78.0	63.0	15.0
June	74.10	82.73	82.80	77.93	78.48	96	66	30	84.5	72.5	12.0
July	79.29	84.87	86.41	80.74	82.85	91	72	19	86.5	76.0	10.5
August	75.87	83.51	84.29	79.74	80.08	95	66	29	85.0	71.0	14.0
September	72.36	79.06	81.30	75.50	76.83	95	57	38	84.5	64.0	20.5

These tables show a uniformly high summer temperature, but not in the same degree, for the unhealthy summer of 1852 was not so hot as the healthy one of 1853; 1849 was the coolest, and 1850 and 1851 were the warmest. But it is well known that high heat alone, though indispensable, does not produce yellow fever. It is a powerful predisponent, and develops other active agents.

The summer temperature of Sullivan's Island is high; and when the sea-breeze dies away, as is often the case at ebb tide, or there is a west wind, the heat is oppressive. There is not a hotter street in the United States than Middle Street. The surface of the island is composed of sand; but little

grass grows to absorb or soften the sun's rays; there are few trees, and none for shade; the houses are retired from the street, so as to afford no protection from the sun; and the direct and reflected heat is often intolerable. Persons whose pursuits allow them to remain within doors during the day, are pretty well off; but the active man of business will find that Charleston is more tolerable than Sullivan's Island, from sunrise to 11 o'clock A. M., and from 3 P. M. to sunset, the shade of the houses affording great protection from the sun. The great advantage of an island residence is in the nights, which are not so uncomfortable as in the city.

5. *High Dew-Point.*—With a high altitude of the thermometer in the low country of the South, humidity and a high dew-point are always associated. This is the case in Charleston Harbour. Dr. Ramsay says: "The hygrometer in Charleston shows an almost constant humidity in the air." Dr. William Hume, of Charleston, says: "We live in a climate with a dew-point above 70°; the baneful sirocco frequently prevails during the day, while the night is *murky*, likening our city to another place, which Shakspeare speaks of, on the high authority of one of his witches." The meteorological observations of Dr. J. L. Dawson, City Register, for two years, and those at Fort Moultrie for three years, in relation to the dew-point, are given below.

Mean.

MONTH.	FORT MOULTRIE.			CHARLESTON.	
	1849.	1852.	1853.	1852.	1853.
May	70.19	70.51	66.50	64.35	64.70
June	75.36	72.94	71.40	66.63	70.43
July	76.27	78.70	77.33	73.16	75.64
August	77.69	75.77	75.32	70.70	74.45
September	71.70	71.74	71.85	67.66	70.70

Humidity (a high dew-point) plays an important part in the causation of febrile diseases. Speaking of the various causes which have been considered as producing yellow fever in Charleston, Dr. Thomas Y. Simons, Port Physician (*Charleston Medical Journal* for May, 1853), says:—

"Altitude of temperature is indispensably necessary. Moisture is undoubtedly a very important agent, and the dew-point indicates a moist atmosphere during its prevalence. Hence the importance of thorough draining and paving to prevent much evaporation."

There can be no question but a high dew-point is a powerful agent in the causation of fevers; we may go further, and affirm that yellow fever never prevails in a place, endemically or epidemically, unless there is a high dew-point. But we have no idea that this is the sole cause of the disease, for in Charleston Harbour it is always high in summer, and the fever occurs only occasionally, there having been long intervals of health, as from 1839 to 1849. Indeed, heat, humidity, and a high dew-point are always present in summer, as is evident to the most common observer, in the rapidity

with which butcher's meat takes on the putrefactive process; yet we do not meet with the yellow fever annually. It is the concurrence of heat, moisture, and a high dew-point which produces the weather termed *close, sultry, or muggy*, and which also produces the change of complexion, the languor of mind, and the debility of the muscular system; and it is the change from this state of things—from high and continued heat, excessive and long-continued humidity, and a continued high dew-point, which renders a visit to the North, or to any dry, elevated section of country, in summer, so agreeable, and so conducive to health, producing a change in the colour of the skin, energy of mind, and strength of the muscular system.

Long-continued heat, humidity, and a high dew-point, are the characteristics of the summer climate of the South, in contradistinction to that of the North. The heat and humidity at the North are often great and the dew-point high, but they are of short duration; and it seems a little singular that people should forget the importance of a high dew-point; hence probably, the notices which appeared in the Charleston newspapers, in the summer of 1853, in praise of their city summer temperature, and comparing it with the northern cities, as Boston and New York, at the same time, and entirely overlooking the effect of a high dew-point. Give us a low dew-point and it matters not what is the altitude of the thermometer. In 1853, the weather was extremely hot in all the northern cities—in New York, for instance—but it was for only a few days in succession, for thunder and hail-storms suddenly put an end to the hot weather, at least for a time. Here is the essential difference between a northern and a southern climate.

The great humidity of Sullivan's Island becomes evident to all by the rapid oxydation of all articles of cutlery, the rusting of keys in one's pocket, the mildew on linen clothing, the injury done to cloth garments, the mould on boots, shoes, harness, and all articles made of leather, &c. Common table salt is an excellent practical hygrometer, and every good housewife knows how frequently it is necessary to submit this essential article of subsistence to a drying process. A familiar article of convenience, matches, affords another example.

In Dr. Simons's report of 1839, there is the following passage:—

“Our meteorological observations give us but little light on this subject; but from my own observations, I have remarked that the dews are heavy and the air chilly at night, and that cloths and leather become mouldy. Brasses can hardly be kept clean, and steel quickly rusts. The continued solar influence in the surrounding water, causes an immense evaporation, which ascends high, and, after the sun declines, begins to descend in the form of dew, in which, I believe, malaria floats.”

High summer heat, humidity, and a high dew-point are always found on Sullivan's Island, in as marked a degree as in Charleston; but the officers' quarters and men's barrack-rooms at the fort are as comfortable and as free from dampness as any rooms on the island, not being in casemates, and in the second story; the lower story, or ground-floor, however, is very damp, and

wholly unfit for residences; and the brick pavement¹ in front is always damp and covered with fungi. This story was but little occupied in 1852, principally by a few servants and the guard, the guard-room being on a level with the ground-floor of the barracks.

In consequence of the combination of heat and moisture, the island is infested with insects, as fleas, sand-flies, house-flies, mosquitos, ants, cock-roaches, &c.

6. *Defective Drainage.*—This cause of disease is common to both Charleston and Sullivan's Island. In the former, many persons are aware of the necessity for introducing a new and better system of drainage. We may refer for the proof of this fact to an editorial article in the *Charleston Mercury*, for October 1, 1852, and the *Charleston Evening News*, for November 9, 1852; and to a paper by Dr. Hume in the *Charleston Medical Journal*, for January, 1850, and January, 1853; and another by Dr. Thos. Y. Simons, in the same Journal for November, 1851.

After the heavy rains in 1852, we saw men baling water out of cellars with buckets, in Church Street, which is several feet above the level of the wharves on East Bay. A physician who resides above the level of Church Street, only a few doors from Meeting Street, which is one of the best streets in Charleston, told me that it took two men eight hours to bale the water from his cellar in September, 1852; and, said he: "After all, the dampness remained." In the summer of 1853, the city authorities authorized and enjoined the employment of the city engineers, to free cellars from water, but "after all, the dampness remained." Water is so constantly found near the surface, that cellars ought not to be allowed; and in truth, a cellar ought never to have been constructed in the city of Charleston.

There is no drainage on Sullivan's Island, except the most trifling surface drainage. The whole island is composed of sand, which is very porous, and water is found just below the surface in the driest season, this surface water being always a mixture of fresh and salt water, which is insalubrious. The tides wash up sand, mud, and slime, particularly on the back beach, which is exposed to the intense heat of the sun at the ebb, and permanently as the spring-tides gradually subside to neap-tide. Both Pringle and Rush refer to this state of things.

Sir John Pringle's description of Brabant suits Sullivan's Island exactly. The water in the wells is always, even in the driest summer, near the surface. There is a "Potter's-field" on the east end of the island, in a part termed the "Myrtles," and here the graves have to be baled out in the driest season just before interment; and, in a wet season, the water runs into the grave faster than it can be baled out, so that a weight has to be attached to the coffin. We now see prominent reasons for the production of malaria (for want of a better term) in the driest summer, as in that of 1850, when

¹ Brick is unfit for floors unless previously dipped in boiling pitch or asphaltum.

the intense heat, humidity, and high dew-point evolved it with rapidity, and there was epidemic fever.

Dr. Ferguson, of the British Army, a physician of great experience, says that vegetation is not necessary to produce malaria, and that this poison may abound where there is no vegetable matter to decay. He says:—

"In August, 1794, after a very hot and dry summer, our army in Holland encamped at Rosendaal and Oosterhont. The soil, in both places, was a level plain of sand, with perfectly dry surface, where no vegetation existed, or could exist, but stunted heath plants. It was universally percolated to within a few inches of the surface, with water which, so far from being putrid, was perfectly potable. Here fevers of the intermittent and remittent type appeared among the troops in great abundance. It is interesting to observe that the soil in Walcheren is precisely similar. Sir Gilbert Blane describes it as consisting of a fine white sand, known in the eastern counties of England by the name of silt, and about a third part of clay. It was after a hot and dry summer, also, that the British army suffered in that island from the endemic fever, to a degree unprecedented in the annals of warfare."

Dr. Watson (Lectures) says:—

"Facts like these seem to prove that the malaria, and the product of vegetable decomposition, are two distinct things. They are often in company with each other, but they have no necessary connection. Whoever, in a malarious country, waits for the evidence of putrefaction, will wait, says Dr. Ferguson, too long. For producing malaria, it appears to be requisite that there should be a surface capable of absorbing moisture, and that this surface should be flooded and soaked with water, and then dried; and the higher the temperature, and the quicker the drying process, the more plentiful and the more virulent (more virulent, probably, because more plentiful) is the poison that is evolved."

* * * * *

"No very certain or extensive observations have yet been made in respect to the kind of soil from which the miasmata are most apt to be extricated. That which is loose, penetrable, porous, and sandy, appears highly favourable to their formation. So are soils which, containing much clay, are very retentive of moisture."

Sir John Pringle says:—

"And here it may be proper to distinguish between the moist and the rainy seasons; for, in marshy grounds, intense and continued heats, even without rain, occasion the greatest moisture, by the exhalation which they raise and support in the atmosphere; whereas frequent showers, during the hot season, cool the air, check the rise of the vapours, dilute and refresh the corrupted water, and precipitate the putrid and noxious effluvia. But if heavy rains fall in the beginning of summer, and are followed by great and uninterrupted heats, then the water collected by the rain, stagnating in the lower grounds and corrupting there, furnishes matter for more exhalation, and thereby renders the season more sickly, and the diseases more fatal."

"It ought also to be remarked, that the sickness never begins till the heats have continued long enough to give time for the putrefaction and evaporation of the water."

Dr. Rush remarks on this passage:—

"This has too generally escaped notice; hence we confound a moist with a rainy season. A wind often brings a moisture with it, which discovers itself in producing more obvious effects upon metals, and papers, and even wood, than a heavy rain. It likewise disposes the miasmata which produce bilious fevers to be more general, and more active in their effects upon the body."

The foregoing extracts from the writings of Sir John Pringle and Dr. Rush are as applicable to Sullivan's Island as if they had been penned for an exact description. The time of the heavy rains has an immense influence on the health of residents, for it may happen that an intensely hot and very dry summer, the sun acting on the previous heavy autumn and winter rains through the porous and sandy soil, may be unhealthy, as in 1850; and on the other hand, a wet summer may be very sickly, like that of 1852, when the excessive rains, combined with the heat, act both as predisposing and exciting causes of fever.

Can Sullivan's Island be drained? The solution of this question is important. If the island cannot be drained, severe epidemics must occur, as they have always occurred, at intervals. Certain lots may be filled with sand, or superficially drained; but more than this is required, especially on the back beach or north side of the island. A thorough system of deep or subsoil drainage over every part of the island is necessary, at least from the hotel (Moultrie House) on the east to the steam ferry on the west; and, as the settlement extends, the drains should also be extended. The faster the island fills up with inhabitants, the more essential are drainage and every part of sanitary police; for the more the island village assimilates to a city, the more necessary will sanitary measures become, and still more will the diseases resemble those of Charleston than hitherto. That the island can be drained in the manner indicated, there is no doubt. General Totten, chief-engineer of the army, assured me in a conversation last spring (1853), that the measure is practicable. In our humble opinion, nothing will more conduce to salubrity, or more surely prevent periodical visitations of epidemic disease, than a well-regulated system of drainage.

7. *Bad Sanitary Police.*—This is an extensive subject, embracing almost every point of city mismanagement. Under this head may be included animal and vegetable decomposition; and, in strictness, bad ventilation and bad drainage also. It is a disagreeable subject, for the residents of a place dislike to be told that their streets, lots, houses, &c. are in bad condition and filthy; nevertheless, *magna est veritas et prævalebit*. Owing to the natural difficulties which existed at the foundation of the city in 1680, Charleston has always been in a bad state of sanitary police, including both bad drainage and bad ventilation; and this was unavoidable at an early period of her history, and for several years after the settlement of the town. The city seems to have arisen without any plan, in those days when "every man did that which was right in his own eyes." The consequence is, that the streets are irregular, narrow, and badly ventilated; and, though favourably situated for commercial purposes, nature has interposed great obstacles to the growth of a healthy commercial city. Dr. Thomas Y. Simons says:—

"From the old maps it is evident that Charleston was intersected by marshes and creeks, which are now filled, and houses built thereon. At present, the city is surrounded by marshes, through which the salt water ebbs and flows."

Dr. Ramsay, in his *History of South Carolina*, states:—

“George Chalmers, in his *Political Annals of the United Colonies*, printed in 1780, pp. 541, 542, observes that ‘Charleston was long unhealthful. From the month of June to October, the courts of justice were commonly shut up. No public business was transacted. Men fled from it as from a pestilence, and orders were given to inquire for situations more friendly to health.’ This statement is corroborated by tradition from the elder citizens, who inform us that in the time of their fathers, the sick were sent from Charleston to expedite their recovery in the more wholesome air of the country; and that the country was preferred, on the score of health, as a summer residence. This is by no means improbable. The situation of Charleston in its natural state, was a slip of land stretching southerly between the two rivers, and projecting into the harbour formed by their junction, and divided into a number of peninsulas by creeks and marshes indenting it on three sides, so as to leave but little unbroken high land in the middle. The first buildings extended along East Bay Street, and had a marsh in their whole front. A considerable creek, named Vanderhorst’s Creek, occupied the foundation of Water Street, and passing beyond Meeting Street, sent out a branch to the northward, nearly to the Presbyterian church. Another creek stretched northwestwardly nearly parallel to East Bay, from the neighbourhood of Macleod’s Lots, through Longitude Lane, and to the north of it. The same kind of low ground ran up Queen Street, then called Dock Street, beyond the French church and through Berresford’s Alley, till it approached Meeting Street. The north end of Union Street¹ was planted with rice about the middle of the eighteenth century. Another very large creek occupied the site of the present central market, and extended westwardly beyond Meeting Street, which diverged southwardly almost to the Independent church, and northwardly spreading extensively and dividing into two branches, running to the northwest and to the northeast so as to cover a large portion of ground. Besides the marsh and those creeks, which nearly environed three sides of the improved part of Charleston, there was another creek a little to the southward of what is now Water Street, which stretched westwardly over Church Street; and another, which ran northwardly up Meeting Street, and then extended across westwardly nearly to King Street. A creek ran from the west near where Peter Smith’s house now stands, and nearly parallel to South Bay, till it approached the last-mentioned creek, and was divided from it by King Street and a slip of land on each side. Six other creeks ran eastwardly from Ashley River, three of which stretched across the peninsula so as to approximate to King Street. There were also ponds and low grounds in different parts of the town. One of these extended on the east side of King Street almost the whole distance between Broad and Tradd streets. This was granted to the French church in 1701, but being useless in its then state, was leased out by them for fifty years. In the course of that period the tenants improved and built upon it. There was also a large body of low grounds at the intersection of Hasell and Meeting Streets. The elder inhabitants often mention a large pond where the Court-House now stands.² It is believed that this, though real, was artificial. It is probable that the entrenchments attached to the western fortifications of Charleston, which extended up and down Meeting Street, from the vicinity of the Independent church to the vicinity of the Presbyterian church, were dug so deep as to cause a constant large collection of water at that middle part of the lines. It was the site of Johnson’s covered half moon, and of a drawbridge, over which was the chief communication be-

¹ Union Street is now State Street. The street on each side of the central market, spoken of by Dr. Ramsay, is now Market Street.

² I have understood from gentlemen now living, that it is not much more than twenty years since ponds of water stood at the junction of Broad and Meeting Streets, in a rainy season, by St. Michael’s Church and vicinity, extending up and down the latter street both above and below Broad Street, which ponds became stagnant, green, and at last dried up by the heat of the sun. This is the condition of several localities, in mid-summer, at the present day.

tween the town and the country. No prudent engineer would erect such works as these on a pond, though, when they were erected on the soft soil of Charleston, they would be very likely to produce one. Whether this was a natural or artificial collection of water, there was enough in other parts of the town to make it unhealthy. Such, with some alteration, was the situation of Charleston for the first seventy years after its settlement."

"To reduce such a quagmire as a great part of Charleston originally was, to a firm, high, and dry state, required time, labour, and expense. Much has been done, but much remains for future enterprise."¹

We have given this long and instructive topographical description from Dr. Ramsay's History, in order to show that there have been abundant local causes of fever, from the foundation of the city to the first part of the present century; and we will proceed to show, if we have not already shown, from other writers, that extensive local causes of disease have existed from Dr. Ramsay's time to the present day.

Dr. Thomas Y. Simons says:—

"Gradually and progressively, all of the ponds and creeks were filled up. Subterraneous drains in the different streets were made, and drains from the yards of the respective premises were made to communicate with the drains in the streets. But it was found that the drains having the grates in the centre of the street, and becoming thus filled up, caused, after heavy rains, a vast quantity of water to be accumulated in the streets, making them at times almost impassable. The offals of the city were required regularly every day, except Sunday, to be removed."²

"The city of Charleston was originally intersected with creeks, and a quantity of marsh in the midst. These creeks and marshes were filled up with wood, and the offals of the city, composed of an immense mass of corruptible matter; a deleterious miasma or vapour is constantly arising from the surface, from this cause, and the accumulated filth which daily occurs."³

What must be the effect of digging and cleansing drains, laying gas-pipes, and making other excavations, particularly about the wharves, throwing up in midsummer, as in 1849 and 1852, this "corruptible matter," decaying wood, city offal, &c.? Would stringent quarantine have excluded the *materies morbi*?

Dr. Ramsay again:—

"The original settlers of Carolina had no thought that in less than a century Oyster-Point would become a place of commercial importance, and the capital of an independent State stretching from the ocean to the mountains. Had they anticipated half of what has already taken place, ten feet alleys, and streets thirty-three feet wide, would have made no part of their projected seat of government. It would then have been nearly as easy to have made the streets one hundred feet wide as any inferior number. In that case they would have admitted three rows of trees, one at each side, and one in the middle of every street. It would have been easy to have made no lots of less size than half an acre, and by law to have prevented their subdivision. In addition to the inconveniences of a low and moist situation, too many people in Charleston, in consequence of its niggardly plan, are crowded on too small a space of ground. Close, compact cities are the destroyers of the human race.⁴ Every family

¹ History of South Carolina, vol. ii. pp. 70-73.

² Charleston Med. Journ. November, 1851, p. 794.

³ On the Yellow Fever of 1824.

⁴ Recent investigations fully confirm the assertion of Dr. Ramsay, particularly the reports of the poor-law commissioners on the sanitary condition of the labouring population of Great Britain.

generates a portion of filth, and when they are near to each other, that becomes too great for the health of the citizens. Numbers are every year sacrificed to the avarice of the proprietors of lots. The evils of a crowded population are increased by high and close fences, which are daily increasing, and still more by building houses in contact with each other and without any interstices between. The daily removal of putrescible substances lessens the evils of an impure air, but is inadequate to the purpose intended. The only effectual remedy is fresh running water. This unites cleanliness with coolness. It removes noxious vapours, cools the atmosphere, and increases its salubrity by extricating fresh and wholesome air from its own substance. The next best practicable mitigator of heat and corrector of foul air in Charleston is trees planted in all the streets which can admit them. They are the coolers given to us by Nature. In addition to their refreshing shade, they imbibe the poisonous materials which vitiate the air. They fan the earth by the vibratory motion of their leaves. Instead of obstructing the free circulation of the air, they increase a light breeze by creating an undercurrent on the surface of the earth, where it is wanted. Cities built with marble, if destitute of trees and vegetation, would only afford a miserable residence to splendidly wretched inhabitants.”¹

The foregoing extract from Dr. Ramsay is long, but every line is full of instruction. The subject of wide streets, trees, bad ventilation, filth, and all the evils of a crowded population, and the fresh running water, are lessons to city legislators in all future time. One might conclude that the historian presaged the erection of the New York Croton Water-Works, and a similar structure for Charleston. The passage describes the past, the present, and the future.

Dr. Shecut, who wrote in 1819, describing the yellow fever of 1817, says of Charleston, at that period :—

“Among the multiplied causes of infection in cities, it must be admitted that the innumerable sinks and drains, if not the primary, are assuredly the secondary causes, and qualify the atmosphere to support for a longer time than it otherwise would, the stamina of the infection, by communicating almost hourly fresh supplies of their effluvia, and that this is the case may be readily perceived, when we take notice that the infection, which is productive of the peculiar disease of the city, does not extend its influence to more than half a mile without its limits. The drains, it must be acknowledged, were sunk through the city, to prevent the accumulation of moisture, and other exciting causes of disease, and as long as they are kept clean, answer a very valuable purpose. That they are otherwise than clean, especially during the sickly season, is not to be attributed to the neglect of our police, who are indefatigable in their exertions, and in the performance of their duties ; but to those thoughtless and lazy individuals, who wink at the duplicity of their servants and others, that are in the filthy habit of secretly depositing the offals of fish, fowl, and the smaller animals, into the drains, and sometimes into their sinks and sewers.

“I am perfectly satisfied that on this subject I am not alone ; many citizens, particularly those that have been observant of the state of the city at different seasons of the year, have frequently, and especially in the latter parts of the evening, on passing certain drains, been almost overwhelmed with the stench that has issued forth from some of the grates ; this circumstance has frequently occurred in particular sections of the city, particularly from Church Street to the bay, with the intersecting alleys ; and I am warranted in saying that, in nine cases out of ten, the prevailing fever has originated within the limits of that section of the city.

“Owners of wharves and lots of low ground in the city, should be prohibited

¹ Hist. of South Carolina, vol. ii. pp. 101-103.

from raising mud to fill up their wharves during any of the summer months; but more especially those persons who are in the habit of filling up low lots with filth and rubbish collected in the yards and streets of the city. It is also a matter of notoriety, that the scavengers' carts convey, daily, to particular low spots in the city, but more generally the skirts thereof, promiscuous heaps of dead animal and vegetable matters, which, being deposited in heaps, become sources of infection, and greatly add to the general exciting cause of disease.

"In directing the attention of the police to the importance of filling up all low lands within the limits of the city, it is obvious that dry and wholesome materials are intended, and not the rakings and scrapings of the streets and yards. I would suggest the immense advantage of encouraging vessels to bring in dry sand and gravel as ballast, which might be purchased for the purpose of filling up all those situations, heretofore the sources of infection, near the bay."

We have, in Dr. Shecut's description of the city police in 1817 and 1819, in both of which years yellow fever prevailed, the condition of the streets, sinks, drains, yards, cellars, and warehouses; the deleterious effects produced by putrid vegetables, so applicable to Market Street in all summers; and the state of the wharves and low lots, filled up with mud and city offal in the summer months, showing their injurious effects in precisely the same way as did the filth and excavations in 1849 and 1852.

Dr. Shecut graphically describes Charleston, at the present day, from 1849 to 1852. The state of the drains, the alleys, the "putrid cabbages" of Market Street, the raising of mud to fill wharves, the filling up of low lots, with filth and rubbish, and the neglect of lots, houses, sinks, and cellars.

In 1825, Dr. Thomas Y. Simons, in relation to the yellow fever of 1824 says: "The city of Charleston was originally intersected with creeks, and a quantity of marsh in the midst of it. These creeks and marshes were filled up with wood, and the offals of the city, composed of an immense mass of corruptible matter; a deleterious miasma or vapour is constantly rising from the surface, from this cause, and the accumulated filth which daily occurs."

In 1839, Dr. Simons says (Report on Yellow Fever): "Our wharves are formed by the palmetto logs, and filled up with wood, stone, and earth. In the docks, a great deal of mud, with decomposed vegetable and other materials are thrown up by the tide, and at low water the exhalations are offensive; independent of this, in some wharves the drains pour out their contents."—P. 18. Again: "Although the marsh miasma cannot be considered as the essential cause, yet it cannot be doubted, where the tide is but partially flowed, intermingling salt water with fresh, and becoming stagnant, a very noxious exhalation, injurious to health, must result. There is, besides, another evil. Small houses are erected on some of these tide-lots, which are but partially filled up, and then oftentimes with heterogeneous materials, is not only a serious nuisance, but must necessarily be deleterious to health. Another circumstance, I may here mention, is worthy of consideration, viz., the disposal of the offal of our city. It is obvious that if the law requires the citizens to remove all the rubbish and offals from their residences, regarding them as nuisances, the concentrating them upon one spot, for the purpose of filling up streets made on our marsh lots, must be infinitely more pernicious and injurious, as well as improper."—P. 21.

The causes of disease enumerated by Dr. Simons seem to be amply sufficient to produce the yellow fever of 1838 and 1839, especially when it is recollected that the great fire occurred in April of the first year, laying bare a large part of the city, and exposing the cellars, sinks, &c., to the direct action of the sun.

In the *Charleston Medical Journal* for January, 1850, immediately after
No. LVI.—Oct. 1854. 24

the epidemic fever of 1849, and, in reference to it, Dr. Hume gives the "Causes of Yellow Fever in Charleston, with an Outline of a Plan for its Prevention," from which some extracts will be taken. Dr. Hume says:—

"Without entering into the details of removing the excess of organic matter and moisture from our soil, it will be obvious that the indications for preventing yellow fever are to reduce the soil to its pristine sterility and aridity, and thus break up the connection between the materials and the agents of putrefaction. How far this is possible, I leave to the intelligence of those in authority. That our present system is ineffectual, no one will deny. That a change is desirable, all will acknowledge. But fatalism, and a fear of unrewarded expenditure, deter us from the attempt to remove from our city those causes of disease which we have introduced. Accumulated filth of a dense population, in a warm latitude, has been a long-acknowledged cause of disease; and if that filth is not obvious to the sight, its existence is easily demonstrated by experiment; and its necessary presence must be acknowledged, on a little reflection. The sandy and porous soil on which our city is located, is well calculated to conceal the organic matter which is daily absorbed and retained until eliminated again, by solar heat, in the unwholesome and poisonous gases and vapours of the putrefactive fermentation. Should the daily contents of the Charleston market be scattered in our streets, and then allowed to putrefy, who would express astonishment at the prevalence of yellow, or any other kind of fever? Who would accuse our merciful Father of having inevitably doomed our city to fatal epidemics? Who would remain under this blind fatalism, and suffer an evil so obviously the result of our own production? Yet this is no exaggerated state of things. The contents of the Charleston markets are not thrown on the surface of the city, and then left to putrefy; but they are thrown into the interior of the earth, after being cooked and digested, mixed with the subterranean water, transmitted to every portion of the city, defiling the wells, and poisoning the atmosphere by the result of its ultimate decomposition. Is this true picture of the condition of our city less revolting, or less capable of producing disease, than the imaginary one of scattering our food on the surface? Yet, as the matter is concealed from our eyes, we do not appreciate the full extent of the evil. What we do not see, we have not conceived; but that a worse state than is generally supposed (if any one ever had any suppositions on so disagreeable a subject) does really exist, we have only to refer to the statistics of Dr. Lining, and we shall find that upwards of thirteen hundred tons of fecal matter are annually deposited by the inhabitants, with ten thousand tons of urine. Now, if we double those quantities, for the production of the lower animals, we shall have a comprehensive idea of the quantity of organic matter which is annually submitted to the putrefactive fermentation, in our clean city of Charleston, with its population of thirty thousand.¹ Shall we omit to add to this immense mass of corruption, the two hundred dead bodies which are annually deposited in our graveyards, for fear that the truth may outstrip the imagination, and incredulity be awakened to deny the whole computation. Be it so; to my mind, it is a far greater wonder that we ever escape its influence, than that we are occasionally subject to its visitations. And I feel infinite gratitude to the Almighty dispenser that we are occasionally saved from that affliction which our ignorance and negligence have so justly merited."—Pp. 29, 30.

The views contained in the foregoing extracts from Dr. Hume command our hearty concurrence. We see the causes of yellow fever in Charleston, particularly in 1849. Very much of the article applies to Sullivan's Island, as filth, moisture, heat, the sandy and porous soil on which privies are situated, &c.

¹ Since 1849, Charleston Neck, containing about 15,000 inhabitants, has been annexed to the city proper, so that Charleston now has a population not far from 45,000.

We pass to the last year in which yellow fever prevailed in Charleston and on Sullivan's Island, and we find, in the *Charleston Medical Journal* for January, 1853, that Dr. Hume again treats of the "Causes of Yellow Fever in Charleston," having special reference to the epidemic of 1852, in which he uses the following language:—

"Cool nights, warm days, and a high dew-point, are the meteorological characteristics of a yellow fever summer in Charleston. Warm nights, warm or cool days, and a low dew-point, are the concomitants of a non-yellow fever summer."—P. 58.

"Influenced by this conclusion, that the wetness of the earth was the cause of the yellow fever, I published an article, recommending a system of under-drainage; and to obviate the then condition of the earth, I advised all citizens to pump the excess of water out of their wells into the city drains, so as to sink the water in the earth to a wholesome depth, such as I observed in the healthy summer of 1826. That nothing was done, I need not say, or that the fever did not abandon our city."—P. 60.

Again, p. 65: "If the result of the examinations of the last eight years shows that the yellow fever only occurs when the nightly condensations are abundant, and the materials of vegetable and animal origin in the earth are fully supplied with water to aid and hasten their decomposition, and, of course, eliminate in greater quantity and greater intensity the secondary products of putrefactive fermentation, which we have called malaria, for want of a better term, and limit it to the surface of the earth, instead of diffusing it into the higher atmospheric regions, beyond the reach of our respiratory organs, or the contact of the skin, it is clear that we have a combination of two meteorological conditions, the concurrence of which is necessary to produce that condition of the air which is inimical to human health. Our examinations also show that either condition, which I have termed malarial (which implies both the presence of the fermenting mass and the condensation after evaporation), or pluvial (which simply implies rain, or sources of water from any other cause occasionally), may exist, and no yellow fever be engendered; this separate harmlessness suggests at once to us the remedy that must be applied to prevent the deadly combination."

We will refer to other authority on the subject of sanitary police.

Charleston Evening News, Oct. 30, 1852: "*Health of the city.*—There is no fact better established in hygienic history, than the danger of exposing, in warm latitudes, a new surface to solar heat. So deleterious is it to health in rural districts, that even the digging of ground for gardens is avoided in the hot season, and wherever mill-dams or other excavations are made, during such seasons, the upturned soil has been found to contain the seeds of death in the form of malaria."

Again. "But not only have we uncovered to the action of heat a large surface of new soil, but added to it material dug out of the earth, where it has lain imprisoned for nearly half a century, and spread it over the depressions that we cannot elevate fast enough for unhealthiness. We have brought out of the foundations of the new custom-house, in a season the most favourable, from heat and moisture, to the development of malaria, the fetid and putrefied masses of earth which contain the elements of death, and scatter them in cart-loads over certain sections of the city.

"In dwelling on the circumstances of the excavations for the foundations of the custom-house, we are not unmindful of those made, in the upper portions of the city, for drains and gas-pipes, as among the causes which have occurred in producing a vitiated condition of the atmosphere. But we believe that the excavations for the custom-house have been of the greatest degree of injurious influence, from the character of the material dug up, and its being spread over a large extent of surface. Can we look for continued health in the city of Charleston, from proceedings which are attended with malady everywhere else? Can

we expect any other result than disease, when the atmosphere is prepared for the reception of morbid matter, from the degree of heat with which it is pervaded, and the quantity of moisture with which it is loaded? In such an atmospherical condition, if there were no other to give fatal activity, prepared as it is to receive malarious material, we are in our midst preparing a laboratory of poison, by unlocking the earth of the decaying substances so long imbedded in it, and spreading them over as large a surface as possible, where the sun and wind do the rest."

* * * * *

"Our streets and lots have been formed of late, to a greater extent than ever before, of this material. Acres on acres of land have been made not only of upturned soil in the western part of the city, but of scrapings of kitchens, the sweepings of yards and gardens, with the blended vegetable and animal matters which have come to the surface from excavations. As then there can be no fact more strongly impressed, as a lesson, on the administration of Health Police, than that disturbance and removal of such matter in summer is injurious to the health of cities, and particularly that excavations where land has been made by filling up with wood, which has undergone decay, is sure to be followed, in a certain state of the atmosphere, by malarious emanations, all such operations should cease at the commencement of the hot season, say the 1st of May, and whatever material remains capable of forming a ferment, should be covered over with some disinfecting substance.

"Do not our numerous fires, followed by unhealthy seasons, afford an instructive analogy? We have scarcely ever had a large fire unattended by such a season, just previous to, or during warm weather, and the atmosphere was prepared to receive the miasm that would thence be formed. The principle of mischief here, is the same as emanations from putrescent materials collected on or below the surface. These are the ingredients in all such cases alike of that material of miasm, which is destructive of health from animal and vegetable decomposition. The new surface exposed in case of large fires, is enough to poison the sources of life, under a due degree of heat and humidity. Let us look back, and the historical connection between our extensive conflagrations, and the appearance of our epidemical yellow fever will soon be evident. Our most destructive seasons have been those of large fires. It is unnecessary to go further back than the year 1838, which if it did not originate, certainly aggravated the fever of that and the subsequent year. With the occurrence of large fires, exposing a fresh surface to high solar heat, with excavations for drains or any other purpose, with the diffusion of the refuse of kitchens and yards to form streets and lots, we may safely connect seasons of unhealthiness, if the state of the atmosphere favours the production of malaria. The agency of heat and moisture is essential to the effect, but without the material supplied by filth, by animal and vegetable decomposition, no mischief will follow. So that we may open our drains, we may excavate acres of soil, and scatter their imbedded morbid matter on all sides, in an excessively dry or excessively moist season, or with a low summer state of the thermometer, without peril. But having done this with impunity for a number of years, under favourable atmospherical conditions, we are too ready to lose sight of the great lesson which the reverse teaches."

These extracts are sufficient. They show that Charleston *ab urbe condita*, had local causes, which, added to the great solar heat, humidity and high dew-point, were adequate to produce malignant fevers; and are accordingly found, in her early history, that such fevers did prevail. We find, too, that the patriarchs of the profession, and others, from the days of Dr. Chalmers, about the middle of the last century, to the present, a period of one hundred years, have often warned the city government and citizens of the dangers arising from the narrow lanes and alleys, the confined houses and lots, crowding, bad ventilation, defective drainage, filth, and every variety of city mis-

management; and generally, the authorities and inhabitants have disregarded these warnings. In 1849 and 1852, yellow fever years, the measures of the city government in relation to sanitary police were so repugnant to the dictates of ordinary prudence and common sense, that they can scarcely be accounted for.

We will survey the city at the present time, commencing with East Bay Street, on the east side of the town, bordering on Cooper River. This and Elliot Street were paved in 1819, being the first streets ever paved in Charleston, before which time it was no uncommon thing to see drays *stalled* or *set* in the middle of the street in a wet season. These streets were sloughs in those days.

Let one pass from the Battery up East Bay Street, and he first comes to Stoll's Alley and Longitude Lane, both running to Church Street, and both filthy, but paved; then to Tradd Street, the foot of which is always crowded and dirty; then to Elliot Street, adjoining which is Bedous Alley, both of which would be an acquisition to the New York "five-points;" and then we come to Broad, the Wall Street of Charleston. Passing north, we come to Unity and Lodge Alleys, both narrow, crowded, and dirty; nor must Queen Street be omitted, which is crowded and always dirty; and then to Amen, Linguard, and Market Streets, all of which are always dirty. Market Street is invariably filthy, winter and summer. The "putrid cabbages and vegetables" mentioned by Dr. Shecut are always in perfection; and it is surprising that malignant fever is not present annually. The street is invariably filthy, from beginning to end, from the wharf to Archdale Street, in all seasons, emitting unpleasant odours even in mid-winter, and in summer the effluvia are intolerable. In October, 1852, when the yellow fever was severe in Charleston, I met a gentleman who inquired if I was not afraid of taking the fever by coming to town? My reply was: "I do not see why I should take it, for I have been among yellow fever during the whole morning in my own hospital, and I cannot see what harm there is in walking your streets for an hour or two in mid-day; but if you would pay me fifty thousand dollars I would not sleep in Market Street to-night." Crossing Market Street, we come to Pinckney and Guignard Streets, with their close houses and filthy yards, two of the dirty streets of Charleston, in the vicinity of Mott's Lane, and the famed Union Wharves and "Hard Alley;" we then pass Hazell and Wentworth Streets, then Bennett's Mills, and the marshes, abounding in rotting wood; and then passing Society Street, at the foot of which there is a marsh lot into which a sewer discharges its contents, among rotting wood and filth of every kind, we at last arrive at Lawrence Street. From Lawrence we pass to Calhoun, formerly Boundary Street, which divided the city proper from the Neck. Between Lawrence and Calhoun Streets is a square, bounded as follows: North by Calhoun Street, east by Cooper River, south by Lawrence Street, and west by East Bay Street; and containing Washington, Vernon, Inspection, Marsh, Concord, and Wharf Streets, all in the most filthy condition, with numberless filthy alleys and courts; and east of all these are wharves, among marshes, rotten wood, and filth of all sorts. The east end of Calhoun Street, toward Cooper River, is in the most filthy condition. East Bay Street ends at Calhoun Street, and crossing the latter we pass through Washington Street to a creek and marshes; then through parts of Chapel and Judith Streets to America Street, and thence to the city lines. The whole of the Neck, every street, in fact, bounded by the city lines on the north, Cooper

River on the east, Calhoun Street on the south, and Meeting Street on the west, is unpaved, undrained, dirty, and in very many parts, filthy.

The greatest part of the city proper, between Meeting Street and Cooper River, from South Bay to Calhoun Street, is dirty, often filthy, and generally insalubrious, and simply naming them will be sufficient; as Church, State, Anson, Elliot, Queen, East Bay, Market, Pinckney, Linguard, Guiguard, and Philadelphia Streets, the last known as "Cow Alley," a little nasty lane, filled with dirty tenements and dirty tenants, fit only to engender malignant fever; and the same remark will apply to Bedous Lane, Mott's Lane, Union Wharves, and "Hard Alley," which leads from East Bay Street down to one of the aforesaid wharves. These streets and alleys were all considered unhealthy in 1849 and 1852, and in all epidemic seasons they are decidedly so. This is the district, "from Church Street to the bay, with the intersecting alleys," of which Dr. Shecut says, that in "nine seasons out of ten, the prevailing fever originated within the limits of that section of the city." On the east side of East Bay Street are the wharves and docks, with mud, the contents of sewers, animal and vegetable substances in a state of putrefaction, and all kinds of rubbish and filth.

We have now gone west as far as Meeting Street, which is, all things considered, the best street in Charleston; and we will now proceed up the fashionable promenade, King Street, commencing at South Bay.

At the lower part of King Street, from South Bay to Lamboll Street and Smith Lane, there are dirty lots, some of them vacant, which are in bad condition in every point of view; then we come to Lamboll Street, and Smith Lane, both dirty, the last being unpaved, undrained, narrow, crowded, and filthy; and hard by this lane, on the east side of King Street, are two narrow alleys each leading into a little court, or *cul-de-sac*, and each crowded and in the highest degree filthy; and then we come to Weims Court and Princess Alley. All of these courts, lanes and alleys, are close, badly ventilated, crowded with foreigners and negroes, and filthy in the highest degree. None of them are paved, or drained, and the houses are in a state of decay. Crossing Tradd Street, we come to Broad Street, next above which is Queen Street. Queen Street, in its whole length and breadth, from Cooper River to Ashley, is very dirty, badly and only partially paved, badly drained, filled with worthless, decaying wooden buildings, filthy courts, and in epidemic years is considered very unhealthy. Next, north of Queen, is Clifford's Alley. Words will not fairly describe this nursery of pestilence.

Proceedings of Council, February 14, 1854:—

"The Aldermen of Ward No. 4, to whom the petition of certain owners and residents of property in the vicinity of Clifford's Alley has been submitted, praying that said alley be either paved or macadamized, in order to abate the nuisance now existing, and to remedy its almost impassable condition, beg leave to state that, even in the event of their reporting favourable to the above petition, it would by no means remedy the evil complained of. The offensive condition of this alley has, for a series of years, been a continual source of complaint, and the attention of the city authorities has been frequently called to the infectious, filthy state of many of the lots on the north side, located in the very midst of the city, immediately contiguous to one of the popular business streets, of valuable stores on each side to a considerable extent, &c."

Then come Horlbeck Alley and Clifford Street, both very narrow and very dirty; then Beresford and Princess Streets, both very dirty; and then the notorious street before mentioned, Market Street. From Lamboll to Market Street, inclusive, not a decent street, lane, or alley leads from King Street, on either hand, except Broad and Tradd Streets.

We will now proceed from Market, up King to Calhoun Street. First comes Beaufain, which is unpaved, badly drained, and dirty; and the lower portion, near Ashley River, is still worse, being made up of city offal, rice-chaff, and sawdust. Then come Hazell, Wentworth, Society, Liberty, and George Streets, most of which are in a pretty good state, but might be made better, some of them being badly paved and drained. And then we come to two notorious little openings, Burns Lane, and a little higher up, McBride Lane, both of which vie with Clifford Alley and Mott's Lane in everything disgusting. It is impossible to describe the bad ventilation, the want of drainage, and the intolerable filth of the lots, courts, tenements, inhabitants, and everything about them. Finally, we come to Calhoun Street.

This street, the old boundary street, is wide, and the centre of it in the vicinity of King and Meeting Streets, is pleasant, well-ventilated, and salubrious, though not yet paved, or well-drained. But other portions of the street bear a different character, the Cooper River end having been already described as intolerably filthy. The western part of the street, as we descend towards the low grounds on Ashley River, is undrained, and after heavy rains the water stands in the gutters until it gradually sinks into the earth, or is evaporated. This end of the street is very filthy. It is made land—made, as usual, on Ashley River, of city filth, rice-chaff, and sawdust.

Let us continue up King Street. Above Calhoun, on the Neck, are Vanderhorst, Hudson, Warren, John, Radcliff, Ann, Mary, Morris, Reid, Woolfe, Cannon, Spring, Columbus, Line, and Shepard Streets. None of these streets are paved, King Street having a pavement for only a short distance above Calhoun Street. They are wholly undrained, and after heavy rains in summer the water stands in them until it becomes stagnant, green, putrid, and emits unpleasant effluvia. These streets have not a good police, but some are in a much better state than others; and several are really filthy, as John, Ann, Mary, Morris, Reid, Wolf, Columbus, Line, and Shepard Streets. The last named compares very well with Clifford Alley, but being almost out of town, the filthy nuisance is not so injurious, and can be more easily abated. Cannon and Spring Streets may be considered in a terrible state of sanitary police, and insalubrious; though wide, they are low and flat, and wholly unpaved and undrained, the consequence being, that the standing water soon becomes stagnant and putrid in the hot season. In truth, the same remark applies to the whole of them, and to almost every street on Charleston Neck. Another nuisance must not be overlooked. The railroad occupies the position of a street between King and Meeting Streets, from the depot on Hudson Street to the city lines; and I am sure every one will agree, that the whole line of the road within the city is in the highest degree filthy. Standing water, green and putrid, exhales an offensive odour; and the vicinity of the whole track seems to have been made a receptacle for rubbish, decaying wood, and filth of every description. This road-track has always seemed to be capable of evolving more miasma than half a dozen of the offensive streets in the vicinity, bad as many of these are; and it would seem that the only remedy is to rip up the rails, and fill the whole track, not with city filth, sawdust, or rice-chaff; or drain it thoroughly, in connection with the adjacent streets and filthy lots; or finally, to entirely break up the road, transferring the depot beyond the northern boundary of the city. The railroad street is a nuisance which ought to be abated.

One thing more in relation to King Street. Besides not being in the best police generally, there is another evil which is liable to pass unnoticed. There are numerous private narrow alleys on each side of the street, from South

Bay to Calhoun Street, which are often closed by a nicely painted door, where everything appears to be in good order; but, on entering, you find yourself in a *cul-de-sac*, surrounded by little tenements or kitchens, with kitchen slops and filth of various kinds in abundance. These narrow passages are much more numerous than would be expected, and they are liable to be overlooked. Such is the condition of King Street and its purlieus in relation to sanitary police, and it is surprising that malignant disease does not occur in all summers. If such a state of things is found in the most fashionable and popular street of Charleston, what can be expected of the by-streets, lanes, and alleys?

We will now make a brief survey of the west part of the Neck, from King Street to Ashley River. This division of the city is low, flat, wholly unpaved, undrained; the streets are uncared for, the lots are filthy, many of them being occupied for cow pastures, piggeries, &c.; the houses in most of the streets are small wooden buildings, new and old, and altogether, this part of the city is in bad condition. Many of the low lots and streets near Ashley River have been filled with city offal, rice-chaff, and sawdust.

We will now look at the portion of the city bounded on the north by Calhoun Street, on the east by King Street, on the south by South Bay, and on the west by Ashley River. This part of Charleston is generally made up of narrow streets, which are badly ventilated, sometimes unpaved, badly drained, and often (generally) in the most filthy state; the tenements are small and crowded; the lots are small, and in the worst sort of police; and everything seems to be neglected. The whole west side of the city appears to be made land. Calhoun Street, below King, is wet, unpaved, undrained, having old wood buildings, filthy yards, and is filled up in the usual way. From Calhoun Street, and above to Broad, the streets and lots bordering on Ashley River have been filled up—literally made—with city offal, sawdust, and rice-chaff. In passing from Calhoun to Broad Street, near Ashley River, many of the streets tremble under the foot as if one were treading on a bog; and there is a bog, formed of city filth, rice-chaff, and sawdust. From Beaufain to Broad Street, inclusive, the streets and lots are filled, or are filling, with wood, rice-chaff, and city offal; and along this side of the town there are numerous lots with high board fences. Dr. Ramsay noticed the high fences in his day, and says that “the evils of a crowded population are increased by high and close fences, which are daily increasing;” which is doubtless true, for they not only obstruct the free circulation of air, but conceal unutterable filth; and the higher, the more closely put together, and the more neatly whitewashed the fence, the more reason is there for suspicion, and the more surely shall we find a mass of concealed filth within the inclosure. The western part of the town, from above Calhoun Street to below Broad, including (in addition to those before named) Savage, New, Mazyck, Logan, Friend, the west part of Tradd, Legard, Lamboll, and the foot of King, are made streets; and in the southern part of the city, in the vicinity of South Battery, the made land includes, besides Lamboll and King, Smith’s Lane, Winn’s Court, Price’s Alley, many dirty little courts, alleys, and lots, and the south ends of Meeting and Church Streets. Most of these streets, alleys, and lanes, and very many of the lots are filled with refuse wood, rice-chaff, and from the scavengers’ carts; and they are all wet, dirty, and in a bad state every way. Atlantic Street and Lightwood Alley, the precise site of the old Lynch Lane, which was the seat of yellow fever in 1817, are made streets—and so is Water Street. Indeed, whoever consults Dr. Ramsay, will find that much of the eastern, or Cooper River side of the town, has been filled up—as the marshes along East Bay Street, the eastern part of Queen Street,

and Market Street; and rice was planted in State Street about the middle of the last century.

What effect has this filling up of streets and lots? If filled with the right kind of material, as sand and gravel, the salubrity of the city would doubtless be immediately increased; but if every kind of perishable material, animal and vegetable, is to be used, the good effects must be remote and prospective, and the immediate effect is pernicious. Even after a long course of years, when excavations are made in the streets, lots, and along the wharves, where such unwholesome materials have been used for filling, especially when these excavations are made during the hot season, the consequences may be serious, from the abundance of pestilential effluvia given out while in this way exposed to the sun; and in this way were the extensive excavations of 1849 and 1852 so injurious. No prudent city government would attempt such things in midsummer; and the results were, as might have been expected, deplorable.

The filling of lots with refuse animal and vegetable material continues to be permitted by the municipal government.

"After a careful examination, the Board report that all of said lots contain stagnant water; and regarding the same as prejudicial to health, recommend to the City Council to cause said lots to be filled with earth, and drained according to law, within three weeks from notice, under the direction of the City Register."

"N. B. The lot of Mr. Green being very large, the Board would recommend that he be allowed three months to fill, and that he be allowed to use chaff or sawdust, provided the same be covered with three inches of earth." Concurred in.¹

Three inches of earth! As well might they scatter the contents of a pepper-box.

"We would respectfully beg leave to call your attention to the necessity of extending the drain at the foot of Market Street beyond the proposed location of the new fish market."

"We shall soon be ready to remove the earth from the interior of the basin, and it is proposed to use the same in filling in of the open space between the basin and street; to effect which it will be necessary to construct the extension of the drain."²

And the "filling in" with dock-mud is proceeding!

The butcher-pens in the upper part of the city are nuisances, and petitions have been presented against them to the city council.

We have occupied much time in the consideration of a long list of evils, but in connection with bad ventilation and bad drainage, both strictly coming under the head of sanitary police, it is scarcely possible to say too much. The importance of the subject warrants, if anything can warrant prolixity. After having given the opinions of Drs. Chalmers, Ramsay, Shecut, Simons,

¹ Proceedings of Council, April 26, 1853—Report of a Board of Inspection on Lots in Calhoun and Nassau Streets. * * *

² Proceedings of Council, February 14, 1854—Report of Committee of Market Board.

and Hume, and also the opinions of the writers in the *Charleston Evening News*, all in their own language, I trust I shall be excused for saying, what every one of these writers has in fact said, that the city is in a bad state of sanitary police, and that a change of system is imperatively demanded.

On Sullivan's Island there is really no sanitary police. From the 1st of May to the 1st of November, a small allowance is made by the village authorities for the purpose of removing the putrefying carcasses of animals, which float down the river past Charleston, and are carried by the current and tide on to the south side of the island; and these are usually got off in a putrid state. But, from the 1st of November to the 1st of May, people are supposed to have no nostrils, lungs, or stomachs, and the bodies of dead animals are not removed. There are numerous small and crowded houses, in addition to the negro kitchens, which are occupied by Irish and other families, who are more filthy in their habits than the negroes; and for years, the filth of all sorts, kitchen slops, and everything which can be mentioned, has been cast abroad, or allowed to accumulate. The privies are another nuisance. They are literally above ground, and in certain localities, in the heat of summer, the effluvia are very offensive. During the heavy rains in 1852, the water in the vaults, where there were vaults, was on a level with the surface of the earth; and north of Middle Street, towards the back beach, the high tides floated out the contents of several, spreading them over the adjacent soil. The contents of the privies, with other filth, in such a porous soil as this, must undoubtedly affect the water to some extent, and in every respect contribute to insalubrity. Lime, chloride of lime, and other disinfecting agents, should be freely used, which seems not to have been the case. No privy ought to be constructed in such a porous soil as this without a vault of brick or stone work, rendered water-tight by cement.

8. *Bad Water.*—This is a serious matter to the inhabitants of Charleston. For many years there have been great complaints for want of this necessary article of existence. Dr. Ramsay mentions the want of good water at length in his *History of South Carolina*, and proposes two remedies for diminishing the heat of the city: shading the streets by trees, and the “plentiful introduction of water, so as to give a facility for washing and cooling the streets with an artificial shower whenever wanted.” Again:—

“The daily removal of putrescible substances lessens the evils of an impure air, but is inadequate to the purpose intended. The only effectual remedy is fresh running water. This unites cleanliness with coolness.”

Dr. Shecut says:—

“From the situation of Charleston, there were but few springs affording wholesome drinking water; hence, for many years, the citizens were obliged to drink that which was collected in wells sunk eight or ten feet below the surface of the earth, which is a little above the level of the surface of the low tide, and the quantity of water contained in them is usually from four to five feet, which is also nearly on a level with the high tide. These, together with the nature of the soil, occasions the water to be generally brackish; to strangers, extremely

nauseous, inducing nauseous gripings, and even troublesome diarrhœa."—P. 78.

It may be contended that Charleston is in as good sanitary condition as the city of New York, or even much better; but that is not sufficient; for it is to be recollected that a better system of sanitary police must obtain for every month of the year than in any northern city; and to assert that the police of Charleston is as good as that of any town at the North, is saying very little indeed. Our cities are not by any means examples of good sanitary police; on the contrary, scarcely one of them of any size, except Boston and Philadelphia perhaps, have even a decent system of police; but they rarely engender yellow fever, while Charleston, with only the same local causes, may ordinarily engender the disease, and periodically does engender it.

The well-water of Sullivan's Island is brackish and unwholesome, and cisterns to the dwellings are as scarce and as valuable as diamonds from Golconda. Few of the houses have them, insomuch that a cistern is an exception to the general rule. The wells, water, &c., have been sufficiently spoken of under the head of drainage. The wealthy families, who reside on the island only in the summer season, are able to procure pure water in the neighbourhood; but those who need it most, the poor, the miserable, and the degraded, who remain during the whole year, cannot get it. The water at Fort Moultrie, for drinking, culinary purposes, and washing, is cistern water, of which there is abundance during the rainy season; but in a time of drought, the garrison has to be put on short allowance, the brackish water of the wells having to be resorted to for general purposes.

9. *Heavy Rains.*—These were a cause, predisposing and exciting, of the malignant fever of 1852, both in Charleston and Sullivan's Island, producing, in combination with other causes, yellow and severe bilious fevers. The summer of this year was very wet, the quantity of rain in each month being as follows:—

	Charleston.	Sullivan's Island.
May,	4.22	4.17
June,	5.18	8.86
July,	6.93	5.43
August,	4.21	4.15
September,	12.27	11.70
Total quantity,	32.81	34.31

Whole quantity for the year: Charleston, 49.72 inches; Sullivan's Island, 51.26 inches. The quantity of rain which fell in Charleston is taken from the tables of Dr. Dawson, city register. From the records, we see that, both in the city and on the island, about two-thirds of the whole quantity of rain for the year fell in the five hot months, every one of which had more than an average quantity; and that by the first of September the earth was fully saturated, when 11 or 12 inches more were added, most of it in the first twelve

days of the month. The register of Fort Moultrie shows that, during the first twelve days of September, 9.40 inches fell, and from the 7th to the 12th inclusive, six days, there was no less than 7.80 inches, 1.30 inches per day, the earth being previously saturated; and it must be recollected that the epidemic broke out in a decided form on the 7th and 8th of the month, and that the first case of yellow fever occurred on the 11th, showing evidently that these heavy rains were the exciting cause, acting on systems predisposed to disease by heat, great humidity, and a high dew-point, by which malaria is rapidly evolved, and all acting in conjunction with other evident causes of disease. Perhaps the matter was rendered still worse by the dry weather and rapid evaporation which followed, for there was not a drop of rain from the 25th of September to the 9th of October, nor from the 14th of October to the end of the month, the whole of October being very fair, with only 1.20 inches of rain. From the 15th to the 25th of October, inclusive, at every meteorological observation, four times a day, the clearness of the sky was 10, which denotes clear sky, without a cloud; prevailing wind, northerly. During the pouring rains of the first part of September, the whole country was nearly submerged, in consequence of which, various types of fever prevailed; and on Sullivan's Island, bilious and yellow fever were universal, the former being very common indeed, so much so as to merit the term universal, the only healthy spot in the vicinity of Charleston being the barren and circumscribed post of Fort Tumter. Cases of "country fever" occurred at the village of Mount Pleasant. Under the head of drainage, the condition of Charleston and Sullivan's Island, in this year, was sufficiently shown, and more is not required.

We have considered the principal apparent causes of yellow fever in 1852, which appear to have been the same as those of previous epidemics; and they are, 1: intemperance; 2, fatigue and exposure; 3, imperfect ventilation; 4, high solar heat; 5, humidity, and a high dew-point; 6, defective drainage; 7, bad sanitary police, which includes animal and vegetable decomposition; 8, bad water; 9, heavy rains.

These different causes are of more or less importance in the production of disease; for yellow fever may occur (as in 1852) in persons who are not intemperate, and have suffered no exposure or fatigue; and, therefore, these cannot be considered essential causes; but there are others which are essential, without which the disease cannot exist, which are unavoidable, and always present; as high solar heat, humidity, and a high dew-point; and there are important causes which are not unavoidable, and are removable, but, acting with the unavoidable causes, generate malaria or the *materies morbi* with wonderful rapidity; as bad ventilation, defective drainage, and a bad sanitary police generally, and which should be obviated without delay, in order to neutralize the influence of the unavoidable causes. Good drainage will remove another cause of disease, heavy rains, by running the water rapidly from the surface to the rivers and harbour, and thus it will be prevented from becoming stag-

nant and putrid. Yellow fever seasons are not necessarily wet, though a majority of them have been so; Dr. Shecut says, in the proportion of four to three. It is not always easy, however, to determine what quantity of rain constitutes a dry, and what a wet year; for a wet summer and a wet year are very different, in regard to salubrity, as is well explained by Dr. Shecut.

"A fall of 20 to 25 inches in the year, if it happens within the four months which mark the sickly season, will, as it regards the health of the city, constitute a *wet summer*; while, on the other hand, if there be a fall of 30 to 45 inches in the year, provided there does not fall more than $2\frac{1}{2}$ to $3\frac{1}{2}$ inches in each month of the year, the distribution being thus equalized throughout the twelve months of the year, the season is properly a *dry one*.

"Although less rain fell in the year 1802, than in any of the seven preceding years, and though there were only sixty-four days in which an actual fall of rain took place, thirty-six of these days were in the three sickly months of June, July, and August, in which there fell at an average of 7 inches in each month, or 21 inches in thirty-six days of the three months, and which, happening at that season of the year, commonly known as the sickly season, must, as it regards the medical history of the city, be called a *wet year*.—Pp. 63, 64.¹

There is a certain something behind all these evident causes of disease—no one knows what it is, and it may never be known—which is essential to the production of yellow fever; it may be the state of electricity, or it may be a deficiency of ozone in the atmosphere;² but it is certain that, in the city of Charleston, there has always been high-summer heat, great humidity, and a high dew-point; and that, for several years, there has been bad ventilation, bad drainage, and a bad sanitary police, both in wet and dry summers, yet yellow fever has not prevailed in every season in which all these conditions were present. Dr. Hume was so well convinced that every cause of yellow fever was present in Charleston, in 1853, that in the first part of the year he announced the fact in the city newspapers, prophesied the occurrence of the disease, &c.; but, after all, no fever came, the year proving as healthy as almost any one of which we have records.

Whatever may be the ultimate cause of yellow fever, we will give the following extract from Dr. La Roche, on malaria:—

"The digging of canals, the opening of ditches, the cutting down of bluffs, the levelling of lots, the filling up or digging down of streets, the opening of roads, the establishment of brick-yards, &c. have proved highly injurious, not only in this country, but in parallel or hotter latitudes, as also during the hot seasons of cold regions. The results of operations of the kind in the East and West Indies are on record, and, like those in Algeria, an account of which has recently appeared, are of a nature not to be easily forgotten. Near Tlemcen, in Algeria, nearly all the soldiers employed in digging a well were attacked with fever, while all the others in the vicinity escaped. The opening of roads,

¹ Dr. Ramsay gives 39.10 inches as the whole quantity of rain for the year 1802, but during the first three months of that year there were only 2 inches; 2 more in April; May, 3; June, 3.40; July, 12.10; August, 4.95; September, 5.85; 29.30 inches from May to September, inclusive—an average of 5.86. Yellow fever raged in Charleston this year.—*History of South Carolina*, vol. ii. pp. 57, 85–86.

² Dr. Hume supposed that he had "found in the store of Mr. Gatchell, No. — Market Street, between Church and State Streets, and adjacent to Linguard Street, in a part of the city remarkable for the prevalence and fatality of the fever," the real miasm of yellow fever.—*Charleston Medical Journal* for January, 1850, pp. 24, 25, and 26.

and the upturning of the earth for various purposes, are recognized, by the entire body of physicians attached to the French army in that country, to have given rise to the extensive development of fever. The writings of Dr. Drake, so often cited, those of Evans, Blane, Cassan, Macculloch, Usher Parsons, Monfalcon, Caldwell, Baley, Thomas, Anderbach, and many others, abound in similar cases. Dr. Merrill, in an essay on the yellow fever of Natchez, has taken great pains to show, and with every appearance of success, that the epidemic of 1823 arose from the levelling of the streets, and the consequent exposure of the fresh soil to the action of the hot sun."

Almost every line of the above-cited passage is applicable to Charleston. The digging of canals, the opening of ditches, the levelling of lots, the filling up or digging down of streets, the opening of roads, and the upturning of the earth for various purposes, as noted by Dr. La Roche, remind one strongly of the filling up of lots with city offal, the opening of drains, the laying of gas-pipes, the filling in with dock-mud, the excavations for the new custom-house and in other places, and the upturning of the earth in numerous localities, in Charleston, during midsummer in 1849 and 1852; and the "levelling of the streets, and the consequent exposure of the fresh soil to the action of the hot sun," at Natches, in 1823, as shown by Dr. Merrill, bring vividly to recollection the extensive excavations in the summer of those same years of 1849 and 1852, when the filthy and offensive earth was thrown up to the action of the *hot sun*, the abominable material having been actually transferred from one street to fill up another.

A consideration of the causes of yellow fever brings us directly to the subject of contagion.

(*To be continued.*)

ART. VI.—*Extracts from the Records of the Boston Society for Medical Improvement.* By WM. W. MORLAND, M. D., Secretary.

May 8. Dropping of Fluids from Vials; a good Method.—Dr. COALE mentioned that he had found a very simple and ready means of remedying the frequent difficulty of dropping fluids from a vial. Unless the lip projects very much, the fluid is apt to run back on the neck, and remain attached until several drops have accumulated. Greasing the neck of the vial (not the lip) will prevent this, and give a clean, uniform drop.

Encysted Tumour on the Forehead: Disappearance, after puncture of the cyst and irritation of its internal surface.—Dr. COALE had a case of common encysted melicerous tumour, on the forehead of a young lady. Unwilling to make a scar in so conspicuous a place, he passed a needle under the skin, at some little distance from the tumour, and then into the tumour itself, moving it about there and scratching its internal surface. This operation having lessened the tumour very much in size, he repeated it three weeks afterwards, and was gratified to find it in a short time entirely absorbed.

¹ Charleston Med. Journ. for May, 1853, p. 304.

Ophthalmoscope.—Dr. BETHUNE showed an ophthalmoscope, made under his direction by a son of Mr. ALVAN CLARK, of Cambridge, from a description in a recent letter from Paris, published in the *Boston Medical and Surgical Journal*.

It consists of a concave disk of glass or speculum metal, with a focus of eight inches, of about the size of a dollar, perforated by a funnel-shaped hole in the centre, as large as a goose-quill. The brass case serves for a handle. The observer, by throwing the rays of a candle into the pupil of the eye, after dilatation, gets a very good view of its internal appearance through the orifice, from behind.

Caries of Elbow-Joint.—The specimen was shown by Dr. J. MASON WARREN. The patient was a female, 19 years old, of a scrofulous habit. Two years ago, she was under medical treatment for some difficulty in the lungs. Eighteen months since, a swelling appeared near the elbow-joint, which gradually increased to a large size, when it was opened, discharging a great quantity of pus. No dead bone could be detected with the probe. The forearm remained for a time fixed in a bent position on the arm, but suddenly, four months ago, in a single night, became extended, and has hung in that position, entirely immovable and useless, since. Her general health had become much affected by this constant source of irritation and pain. At a consultation held on her case, it was at first decided not to amputate the limb, on account of the affection of the lungs, which was evidently tuberculous; but at a second examination of the case, two months subsequently, it was determined to remove the limb, *especially as the patient herself strongly urged it*. The arm above the swelling was quite small, yet a number of large vessels required ligatures, evidently going to supply the swelling below.

Since the operation, the patient has improved; and now, at the end of two weeks, is able to be up and go down stairs; her cough less, and all the functions are performed naturally.

An examination of the joint disclosed very extensive disease. The cartilages on the head of the bones were entirely destroyed. The fibro-cartilage, on which the radius revolves, had disappeared. The joint was filled with pus, and sinuses extended from it in two or three directions. All the muscles in the neighbourhood had undergone the fatty degeneration.

Blighted Fœtus and Placenta—Suspicion of Syphilitic Taint.—Dr. PUTNAM exhibited a blighted fœtus and placenta. The mother has two children. Since their birth has had three miscarriages; one at the third month, one at the seventh, the fœtus having the development belonging to that period; and this last at the eighth month, though the vitality of the fœtus ceased at the third.

The husband stated that he contracted syphilis subsequently to the birth of the children; that he was immediately put under treatment, and, before any further conjugal intercourse, was considered free from disease. The mother had inflammation of the vagina, with muco-purulent discharge, which readily yielded to treatment. Neither has had any local or constitutional sign of disease whilst these imperfect gestations have been recurring.

Dr. Putnam supposed that, in this and similar cases, abortion might be attributed to the ordinary sympathy of the uterus with any grave disorder; but he also thought it not improbable that the specific poison, once having made its impression on the maternal system, might be operative, after all its appreciable signs had disappeared, and might affect successive products of conception.

Dr. COALE said that, from the experience of others, and from his own, he was always induced to suspect syphilitic taint in morbid alteration of the placenta, like the one just exhibited, where much, if not almost the entire bulk of the organ becomes solidified. Besides other cases, in which cause and effect were too obvious and evident to admit him to doubt for a moment the course to pursue, Dr. C. had one case which he thought an interesting one, on several points. He was called to attend a fine, healthy-looking English lady, who had already a hearty child, some three or four years old. She was anticipating, in full confidence, great pleasure from the prospective increase of her family, and when a stillborn, half-putrid child came into the world, her disappointment was proportionate. Within twelve months, the same promise was held out, to be disappointed in the same way, and this time with great effect upon the spirits and health of the mother. Dr. C.'s suspicions were now aroused as to the probable cause of the state of the placenta and death of the foetus, and he had the trying and unpleasant duty of propounding it to the father. He confessed that, some three years before Dr. Coale's acquaintance with him, he had for a short time been irregular in his habits, had contracted disease, but was not aware of giving it to his wife. Investigation into the case of the wife gave no evidence of chancre, but decided evidence of secondary eruption, which had continued even to the time of examination. Dr. Coale put her at once on the deutiodide of mercury, affected the gums, very slightly, twice, and within another twelve months had the pleasure of delivering her of a fine hearty child.

In another case, however, Dr. Coale had witnessed the same change in the condition of the placenta, and the consequent death of the child, of a healthy primipara, in which no evidence could be found to awaken even a suspicion of syphilitic taint. The mother, in this case, was delivered a year afterwards, without any medical interference, of a fine healthy child.

Dr. Coale noted, in these cases, the very rapid decomposition of the child in utero. In the first, motion was perceptible until within forty-eight hours of delivery, yet the foetus was a putrid mass. In the second and third cases, the child was evidently alive within twenty-four hours, yet the whole body was flaccid, the skull soft and shapeless, and desquamation had proceeded.

Abortion; Diseased Placenta, &c.—Dr. BLAKE remarked that he attended a woman in confinement, April 17; the child came away dead; the whole mass, placenta and unruptured membranes, passed at once. Six weeks previously, having taken exercise to an unusual degree, the patient was attacked by pain and had some flowing, which latter, however, ceased in two or three days, without any resort to remedial measures. The afterbirth was found diseased throughout one-half, or more, of its extent, being converted into a semi-cartilaginous substance. There was no reason to suspect syphilitic taint.

[In cases in which syphilis has had an influence in causing abortion, &c.—as in those related by Drs. Putnam and Coale—Dr. W. TYLER SMITH, of London, has some interesting and important remarks in the *London Lancet* for May, 1854. He concludes that “where the placenta and membranes become so diseased as to cause abortion, the child remaining free from disease, the mother is pretty sure to be affected with the disease; but when the child is born living, and is apparently healthy at the time of birth, the mother may in some cases escape contagion.” “When the ovum is affected by secondary syphilitic disease, we can easily understand that the blood of the foetus should infect the mother through the placenta.” “There can be no doubt, I think, that, in practice, in all cases of repeated abortion, and eruptions in the early

months of infancy, the health of the male parent, before marriage, should be strictly inquired into."—*Op. cit.* p. 425.

At a meeting of the *Harveian Society*, Mr. BORHAM referred to Dr. Smith's paper, proving the "transmutability of secondary syphilis from the father to the child," &c. Mr. B. also exhibited two fetuses, lost at about three weeks from conception, and from the history of which, "as well as from two or three others unrecorded by him, he was induced to believe that many cases of abortion occur, and may be traced to the fetus being vitiated, from having been begot at a time when the father is suffering from that blood disease, secondary syphilis. The mother's healthy uterus would expel it, as it would any other foreign body, from its being stimulated by an unhealthy, vitiated, and unnatural conception."—*Lancet, ut supra cit.* p. 469.—[SECRETARY.]

May 23. Tumour from beneath the Skin covering the Glutei Muscles.—Dr. DURKEE exhibited a tumour sent to him by Dr. HENRY C. PERKINS, of Newburyport. It was removed from a clergyman about forty years of age; began to show itself twenty years ago, just below the skin; was always hard to the touch; never painful; produced no inconvenience except as being in the way while the patient was in a sitting posture. Tumour weighed four ounces and one drachm; consisted of epithelial scales, concrete sebaceous matter, carbonate of lime, and phosphate of lime;¹ no cholesterin. Its structure was laminated, and its shape nearly round. It was so hard and solid that it required to be sawed open for examination. The inner surface of the sac containing the tumour was lined with soft epithelium.

Professor Ackley's Case of Hermaphroditism.—Dr. J. B. S. JACKSON made the following remarks: An account of this very remarkable case has already been published in the *Am. Journ. of Med. Sci.* for July, 1853, by Dr. BLACKMAN, of New York; and in the following number of the same Journal is a report to this Society by the late Dr. BURNETT, of this city. Dr. Blackman's paper is accompanied with a wood-cut; casts of the specimen have also been made, coloured, and mounted for distribution; and a copy is in the Society's cabinet, sent by Prof. Ackley. If there is any question, then, as to the facts, anatomically, it is desirable that it should be made known. Dr. Jackson said that he had had an opportunity to examine the specimen recently, through the kindness of Prof. A.; and he thought that the accounts above referred to were in some important points questionable or incorrect.

The os tincæ is represented in the figure and in the cast as a prominent object. Dr. J. found no trace of it; but the uterus passed insensibly into the vagina. This last was nowhere of the full size, and towards the opening into the bladder, measured, at the smallest part, between four and five lines in circumference upon the inner surface; in the Society's specimen of the cast, something like transverse rugæ are represented, but nothing of the kind was seen. The fimbriated extremities of the Fallopian tubes were distinct enough, though very slightly developed.

The very important fact of the existence of ovaries when the testicles were so well developed, should be well attested. Dr. J. found some thickening of the tissues about where the ovaries should be, but it was ill defined and slight; and it would not have been thought of, except in connection with the present question. Upon one side, an incision was made into this questionable part; but nothing like a Graafian vesicle was seen, nothing but a loose cellular or fibro-

¹ The phosphate of lime was detected by Dr. Bacon.

cellular tissue; a yellow body was exposed, which was thought at first to be a corpus luteum, but it proved to be a lobule of fat. Upon the other side, no incision seems to have been made.

One of the testicles had been cut open, and its structure was quite normal; the other had scarcely been cut into. In regard to size, Dr. J. thinks that they were larger than they have been stated to be by Dr. Burnett; his attention, however, was not particularly directed to this point, as he had forgotten Dr. B.'s statement, and did not have an opportunity to refer to it until after his return. Some allowance may, perhaps, be made for the circumstance of the specimen having been in spirit for a number of years. Neither of the organs was dissected so as to show the epididymis, though there could be no doubt of its existence. An incision was made upon a cord that had the feel of a vas deferens, and it was very nearly exposed, midway, to the extent of about two inches; towards the testicle, it appeared to diverge from that organ, so far as could be seen by holding the parts up to the light; but, as no dissection was made, Dr. J. would not say that it was not the vas deferens, and that it did not arise from the epididymis. Towards the neck of the bladder, this cord could not be traced. Upon the other side, something was felt indistinctly that might have been a vas deferens.

The vesiculæ seminales were not found.

In the situation of the prostate, there was a feeling of resistance, as if the gland might exist; but it had not been demonstrated.

The penis, scrotum, and other parts, appeared to Dr. J. as they have been already described.

Professor Aekley made a longitudinal incision through the uterus, and very kindly gave permission to Dr. J. to examine, by dissection, several points about which there seems to be a question. Dr. J., however, declined the offer, in the hopes that so interesting a specimen would be examined thoroughly by some professed anatomist.

Sudden and complete Cessation of Premature Labour-Pains.—Dr. COALE was called to a young girl in a state of extreme narcotism, as was afterwards ascertained, from an intentional overdose of paregoric. He discovered she was some five months gone in pregnancy. She was relieved, but Dr. C. was called again within forty-eight hours, and found her apparently in violent labour, the pains regular, and so severe as to cause violent straining, clutching at the bedpost, and bearing down; in short, all the symptoms of severe, active labour. He left word to be sent for again; but receiving no message before the next day, he again visited his patient, and found, to his astonishment that, as the night advanced, the pain left her without delivery taking place.

That partial and irregular contraction in the muscular fibre of the uterus near the full term might occur without expulsion of its contents, may not be very strange; but that such violent contraction should take place in a uterus containing a five months' fœtus, and not expel it, Dr. C. thinks worthy of note.

Passage of Air per Vaginam after Delivery, before the Removal of the Placenta.—Reported by Dr. E. W. BLAKE. May 13, Mrs. S. was delivered of a daughter, weighing nine pounds, after a rapid and easy labour. The funis having been tied and the child removed, whilst standing by the bedside, a few moments afterwards, awaiting the throwing off of the placenta, Dr. B.'s attention was attracted by certain peculiar sounds, emanating from the person of the

patient. His first thought was that the desired contraction of the uterus had forced after-birth, coagula, &c. from the vagina, and that their forcible expulsion through this passage had given rise to the *phenomena*. An examination, however, showed that the after-birth still remained, but revealed the real source of the *sounds*. As the hand reached the os externum, the noise was renewed, now affording sensible proof, by the breaking around the fingers of *bubbles of air* forced through the mucus, &c. of the vagina, that their origin was within this passage, and that the *explosions* which continued (audible and evidently annoying to the patient herself) during two or three minutes, followed the uterine contractions. The after-birth was now found to be detached, and as a finger drew down one edge, a sudden and final rush of pent-up (inodorous) air occurred. No other unusual symptoms accompanied this otherwise natural case.

A Precocious Infant.—Dr. BLAKE mentioned the following facts: May 11, attended Mrs. H. in her (fourth) confinement, after a tedious, though not severe labour; the child, a female, unusually vigorous, and weighing, at birth, eleven and a half pounds. On the third day, subsequent to visiting his patient, the nurse asked: “Doctor, do you see our baby laugh?” Incredulous, he crossed the room—but, to his surprise, the infant returned an answering smile to the caresses of the nurse. With a look of intelligence, it turned to the father who stood over it, as he patted its cheeks, and as quickly turned its eyes on Dr. B., when he did the same; and when he stepped aside, its gaze followed him (attracted, he supposes, by his *spectacles*) until, being out of view, it cried aloud; returning to its side, a smile of pleasure again greeted him, and the cry was once more repeated when he ceased to notice the little one.

Here was unmistakable evidence of pleasurable and sad emotions, and showing the distinct recognition of sensible objects, by an infant as yet scarce sixty hours old. That it was no accidental circumstance, the like manifestations, which were daily noticed afterwards, sufficiently prove.

Vagitus Uterinus: the so-termed Sound simulated, in a Case of Operation for Vesico-Vaginal Fistula.—Dr. PARKMAN said that, while performing the operation for the union of vesico-vaginal fistula, he had heard a sound which closely resembled that described as *vagitus uterinus* of the infant. In this case, the sound doubtless arose from motion communicated to the mucus and other secretions in the vagina.

Dr. BIGELOW, Sen., remarked that in foot and breech presentations he had frequently heard a child cry before the head was out of the vagina; the face presenting, so that the mouth was near the external air. He added that, in such presentations, after the body is delivered, if, on examination, the child be perceived to gasp, and, at the same time, kick convulsively, these are signs of danger to its life; if, at this moment, two fingers can be introduced, so as to draw down the jaw, and, by pressing back the perineum, admit air, the child will live; forcible attempts at delivery of the head will not then be necessary, and Dr. B. believes that the head will, in such cases, be naturally delivered.

June 12. *Revaccination; Question as to its Necessity, &c. &c.*—The question was raised by Dr. STORER, and a discussion ensued, in which several members took part; a condensed account of the opinions expressed is subjoined. Most of the gentlemen would revaccinate, *if requested* to do so;

several would certainly do the operation if persons were much exposed; certain members declared themselves *non-revaccinators*.

Dr. STORER observed that two cases had lately occurred in his practice, to which he would refer, with the hope of eliciting the opinions of the members of the Society respecting the subject of *revaccination*; that he might learn the views of the gentlemen present, regarding its propriety, expediency, necessity, and, if necessary, how often?

He felt that he already knew the views of many of his brethren; he wished to hear a free discussion from *all*. As so many cases of serious inconvenience, to use no stronger language, have occurred in this city during the few past years from revaccination, and as we are seldom called upon to revaccinate a patient unless a larger number of cases of smallpox are known to exist in the city than usual, he was in the habit of pursuing the following course. Whenever a person presented himself for revaccination, he inquired if he had recently been, or expected to be exposed to a case of smallpox? If his answer was in the affirmative, he vaccinated; if in the negative, he advised that the operation should not be performed.

Two or three months since, he was called upon to take charge of a case of smallpox. The wife of the patient had been vaccinated, years before, and her arm exhibited two well characterized vaccine cicatrices. She was disinclined to be revaccinated, and eventually suffered severely from varioloid.

Since this case occurred, a boy nine years of age, whom he had vaccinated four years previously, and upon whose arm were two perfect vaccine cicatrices, was attacked with smallpox, and died at the expiration of four days. This latter case naturally awakens the inquiry, how often should vaccination be resorted to? The common notion that it should be performed every seven years, would not answer. In this case the system had not been protected for four years. Ought we to consider this merely an exceptional case? Ought we, as a general rule, to advise revaccination? and if so, how often?

Dr. C. E. WARE reported that he had seen one case in a teacher, who had contracted the disease by visiting a house adjoining that in which a person was sick, and afterwards died, of smallpox. The disease was of so light a character that it did not confine him, and he, supposing that it was merely a cold, visited his pupils as usual. All his family, consisting of five members, all of whom had been vaccinated formerly, but not at that time, had varioloid. None of his pupils contracted the disease.

In four other families, Dr. Ware had recently had a single case of varioloid. Although some of the members of these families were revaccinated, most of them were not; and yet, so far as he had learned, although the period of incubation was long passed, there had been no second case of varioloid. In another family, of many members, although none were revaccinated, there were only two cases of varioloid.

Dr. INCHES desired, as the question of duty, by Dr. Storer, in his case of variolous disease had not been answered, that some advocate of revaccination would state the laws of the durability of protection by vaccination, and consequently the frequency of revaccination necessary to secure protection. This is important, first, because the whole population cannot be revaccinated at each invasion of smallpox, and secondly, because during epidemics of smallpox and erysipelas, severe general and local symptoms occasionally attend the operation of revaccination. Therefore, if revaccinations are to be repeated, it should be at periods insuring protection, and at moments when the above epidemics do not prevail. Dr. I. felt, however, that he did his duty when he tested each vaccination by an immediate revaccination.

In considering the question of the necessity of revaccination, Dr. COALE thought that there were several parts to the proposition, several previous questions to be answered. That the system is capable, after an interval, of a second, or even third impression by vaccine virus, has long since been shown by the result of revaccination in the Prussian army, and in Wurtemberg. The results are herewith given, and we have every reason, in the known high discipline of the Prussian army, to rely perfectly upon these statements. [Dr. C. here quoted the following paragraph from the *Medico-Chirurgical Review*.]

"The Practice of Revaccination.—Every medical man must be aware that the propriety or necessity of this practice has excited much attention of late, although its investigation has been unaccountably neglected by the profession in this country. To us, there seems no one valid objection to urge against it. It has been said, indeed, that such adoption would unsettle the public mind in its faith in vaccination. Nor need this be regretted. The most fatal condition of the public mind, and from which much evil has already sprung, is *apathy*. Let public attention be fairly aroused, the merits of vaccination will then undergo renewed discussion, and its more general adoption must be the result. It is especially to Prussia and Wurtemberg that we are indebted for the experiment of revaccination upon a large scale. In the former country, of 216,289 revaccinations during 1833–7, there were 84,516 successful; and of 44,000 in the latter country, 20,000 succeeded. Frequently, too, cases which failed on a first trial succeeded on a subsequent one. The precise proportion of successful cases has varied from 31 to 45 or 46 per cent.—the period between the ages of 10 and 30 being found that most certain of success. Of course, no one infers that the success of revaccination implies a liability to smallpox in an equal number of cases. The operation, in fact, in the hands of Heim, proved successful also in 32 per cent. of persons who had already had the smallpox—a proportion infinitely greater than that in which the smallpox occurs a second time. But, although we are unable to state the exact proportion of the vaccinated persons, in whom revaccination succeeded at the rate of 34 per cent., who would otherwise have acquired smallpox on exposure, yet experience has shown that this might have been considerable; whereas, among the many thousands who have undergone revaccination in Prussia and Wurtemberg, an example of the occurrence of smallpox has only here and there been observed. Moreover, in the case of an epidemic breaking out, it has been found, in various localities, that immediate revaccination has *arrested its course*—individuals in whom the operation proved successful and those in whom it failed equally resisting the disease."

The interval after which the system becomes impressible for the second time to the vaccine impression seems, from these reports, to be somewhere about seven years. But now, having answered this, there is another question: Is there any relation between the susceptibility to vaccine virus and to variolous virus? In other words, is a person who is susceptible to a second impression of vaccine necessarily susceptible to smallpox? Dr. C. thinks that, reasoning from analogy, we must believe that there is, and that a person who is susceptible to the vaccine impression, is also to that of variola. It is true that we cannot fix this by actual experiment very readily, for the system of the individual is more susceptible at certain times than it is at others. We vaccinate a child repeatedly without making any impression upon it. Two months afterwards, upon another introduction of the virus, it "takes." So with variola, persons at one time exposed to the contagion, are unaffected; at another, a much slighter exposure gives them variola. Dr. C. himself was exposed for six months on board a ship where the disease was prevailing for that length of time, and men died of it in its most aggravated form. He did not, however, become affected until the disease was leaving—his case being the last but one, and that one was a brother officer, who had at the beginning of the

epidemic, six months before, assiduously nursed a case both night and day, without taking the disease himself. These facts should make us chary of deducing from the immunity of a person at any particular time that he is *well* protected; and with the facts exhibited in the Prussian reports, numbering cases by hundreds of thousands, if the numerical system is worth anything, we ought surely to feel that it is our duty to encourage revaccination, and at periods not longer, as a general average, than seven years. Nor should there be any discredit to vaccination or detriment to the glory and greatness of Jenner's discovery. The prophylactic is just as good as he demonstrated it to be, it only has to be administered several times, at long intervals in the lifetime, instead of one dose sufficing.

Dr. DURKEE said that if the reports respecting revaccination in Prussia are reliable, it is quite certain that the vaccine element in that country possesses different qualities from those universally attributed to it in this country. We must either assume that these reports are not entitled to credibility, or else admit that the fame of Jenner rests upon the sand.

Or what is probably nearer the truth, we must suppose that there is some defect in the mode of conducting the business of vaccination among the Prussian troops. If a physician among us were to state that he had vaccinated in a thorough manner five hundred persons within the last five years, and that, of these, one in every four had been successfully vaccinated a second time, should we not look upon the statement as a very strange one, judging of it in the light of our own experience? Should we not suspect some radical error in his method of treatment?

A second attempt at vaccination often results in producing a pustule in the arm of a person who has already had true vaccinia; but this passes through its several stages more rapidly than in the genuine disease, and its aspect from beginning to end is wholly unlike true kine-pox. Nor does it produce constitutional symptoms; or if it does, they are of the most insignificant and equivocal character. The whole affair, in all its features, is a mere counterfeit.

Dr. D. was confident that the experience of the thousands of physicians who have been in practice in the United States twenty or thirty years, would sustain the opinion that one genuine vaccination is sufficient for life. This is the rule. To this there are exceptions, but these exceptions are rare compared with the millions of our population that are protected. There is good reason to believe that the great majority of physicians feel a personal responsibility in all instances of the vaccine disease which they undertake to produce; and if the vaccination is properly conducted, it is not probable that more than one out of a thousand persons, thus carefully treated, ever suffers from varioloid or smallpox. Take the city of Boston: Since the existence of the municipal law which requires that every child shall have a certificate of vaccination from a respectable physician, before it can be admitted into any of our public schools, probably more than 50,000 children have had kine-pox, and it is truth to say that physicians and citizens generally consider these children safe. The law has its origin in this idea of safety, which is the result of many years' experience relative to the prophylactic power conferred upon the system by the vaccine disease. This confidence need not be shaken by any tidings that come to us from the Prussian army or from any other quarter.

Dr. GRAY had repeatedly revaccinated patients at their request, but in no instance had a good vesicle been produced. He was, therefore, led by his own experience to believe the proceeding useless, and to oppose a repetition of the operation, on the ground that it would create an unnecessary alarm in the

community. It remains to be proved by its advocates that a second or even a twentieth vaccination renders an individual more secure than the first. There are instances to show that revaccination, in case it be necessary, should be repeated at very short periods, indefinitely. Two cases of varioloid had occurred in his practice during the past summer, in children under five years of age, one of whom had been properly vaccinated within a year. It was advised by many that revaccination should be practised wherever individuals were liable to exposure. Without denying the wisdom of such advice, he would merely state that, with a single exception, no case under his care of variola or varioloid during the last twenty years could be traced by the patient to any known cause. From the circumstances above mentioned, Dr. G. was led to express the opinion to his patients that one vaccination only was advisable, and since the alternative is a frequently-repeated operation, rendering the community constantly anxious about this formidable disease, he must be thoroughly convinced of his error before he would do differently.

Reference having been made to the distinction between variola and varioloid, Dr. STRONG said that he would consider all those cases which "stop short of the full developments of smallpox, as *varioloid*;" in these, the disease "fades away," as it were. In respect to contagion, and its power over individuals, Dr. S. supposes the amount of exposure will be the criterion of frequency in taking the disease; he himself had varioloid in two weeks from the time of inhaling, accidentally, the breath of a person ill with smallpox.

[In defining variola, authors give the preference to the division into discrete, confluent, and modified smallpox; the latter is the disease "modified" by previous variola in the patient, or by vaccination, and of course answers to the term *varioloid*, correctly speaking. Dr. Watson prefers to say "modified smallpox." *Varioloïde*: *Synonymie*—*Variolè modifiée*; *varicelle pustuleuse ombiliquée*. (Grisolle.)—SEC'Y.]

In regard to the Prussian army statistics, members seemed to differ, widely, in opinion. Dr. STORER was surprised to find that they were brought forward, as worthy of any confidence in fixing a result of any sort—he had supposed that they were, long since, pronounced unreliable.

Dr. PERRY, referring to the researches of the late Dr. FISHER, remarked that Dr. F. had given his opinion in favour of revaccination, and that he had expressed confidence in the Prussian statistics.

Dr. C. E. WARE thought the Prussian experiments were only "half made;" they only prove a susceptibility to revaccination, not to the taking of smallpox or of varioloid. Dr. W. revaccinates, if asked to do so.

In reference to the soreness of the entire arm, frequently following revaccination, Dr. PUTNAM remarked, that it is quite often ascribable to irritation from the clothing over the punctures. If desired, he revaccinates; he mentioned the case of a lady who had had smallpox by inoculation, in her youth; on revaccination, a well-formed vaccine vesicle was produced.

Dr. CABOT related a similar case.

Dr. INCHES inquired of Dr. COALE, whether, in the Prussian army, those who were revaccinated for the third time, would be considered fully protected?

Dr. COALE supposed not, *with certainty*, but they assuredly were protected in great measure.

Dr. SHATTUCK, in an instance of the occurrence of varioloid, had revaccinated certain members of the family; in one person there was an abortive pustule; varioloid was subsequently declared in that individual. No other case in the family.

Dr. PUTNAM asked if any of the members present could offer any valid objection to revaccination?

There was no reply. The only objections previously made were the occasional sore arms, and alarm of the community. If solicited by their patients, they would doubtless not *refuse* to revaccinate.

[“Some have recommended the repetition of vaccination at intervals of a few years. This is a wise precaution, justified by experience, and, if adopted, should be performed a second time at about ten years of age, or from this to the age of puberty.” (*Hooper’s Vade Mecum*, p. 251.) Dr. WATSON (*Pract. Physic*) says: “There seems reason for suspecting that the protecting influence of cowpox diminishes by lapse of time, and at length wears out;” * * revaccination at a distant period reproduces, in a greater or less degree, its primary effects. He mentions the case of a father and son revaccinated; the boy somewhat affected, the father not at all. “It remains to be determined,” he adds, “whether all those who are susceptible of some impression from a second vaccination, are liable to be affected by the contagion of smallpox; and whether a repetition of the operation of engrafting the cowpox, renews, or adds to, their security against smallpox. At any rate, the practice of revaccination is a safe and advisable precaution.—*Op. cit.* p. 739, vol. ii. English edition.

During a session of the *Société de Médecine Pratique* at Paris, March 2, 1854, the following facts and opinions were given, as stated in the *Gazette des Hôpitaux Civils et Militaires*, of April 29, 1854:—

“*Variola; Influence of Vaccination.*—M. DUPERTHUIS had seen, during the winter just preceding the above dates, 57 variolous patients; of these, 35 were from 3 months to 18 years, old; 22 were from 30 to 60 years; between the ages of 18 and 30 years there was not a single case; 30 of the 57 had been vaccinated; these were among the oldest; there were 2 deaths—ill-fed and feeble infants were the subjects; one patient, a man of 32, was quite delirious.”

M. Duperthuis states that he revaccinated several persons who had already been submitted to the operation; he did not obtain any characteristic vesicles (*boutons*).

M. GUERSANT remarked: “That the statistics of M. Duperthuis, although upon a small scale, as to numbers, were important and instructive. Thus, variola had occurred 35 times in patients under 18 years of age, many of whom had never been vaccinated. In this there is nothing surprising, except that, since certain of this number were inoculated, the result would prove that the vaccine disease does not infallibly (*sûrement*) protect the individual up to the said age. On the other hand, as no case of variola was observed between the ages of 18 and 30 years, admitting the undeniable (*irrecusable*) influence of the vaccine inoculation, it would be shown that vaccination may protect from smallpox up to the 30th year; and, finally, there having been 22 cases of variola in persons between 30 and 60 years of age, it would follow that after the 30th year, it is requisite to be revaccinated.”

M. Guersant alluded to the greater exactitude which would have been attained, had M. Duperthuis noted down precisely the number of persons in each series, who were, or not, vaccinated.

M. MASSON said “he could not entertain the opinion generally received, that variola is rare in vaccinated children. He had revaccinated 17 individuals, 5 of whom presented a perfect vaccine vesicle,” (*les caractères d’un bon vaccin*.)

M. THORE remarked “that we must not believe an infant is vaccinated, merely because it has been submitted to the operation of the inoculation of the vaccine virus. From the fact that the eruption is not watched throughout its successive developments, it often happens that abortive pustules (*faux*

boutons) are mistaken for good ones. Moreover, on the other hand, very marked varioloid may be pronounced to be genuine variola, and every one is aware that, so far as contagion is concerned, a great distinction is to be established between the two affections."

M. Masson "could not adopt (*rejeter*) the principle that vaccine protects from variola until the 30th, or even to the 20th year. He had seen a lady, 19 years of age, attacked, after her accouchement, by confluent smallpox, while upon her arm there were large scars of vaccination."

M. Guersant replied "that vaccination has both its partisans and its opposers (*detracteurs*); but the facts given by M. Duperthuis support the opinions of the former, and are in favour of revaccination."

The above discussion has been slightly abbreviated; the facts and opinions given seem of sufficient value to authorize the extracts made.

The opinion of GRISOLLE is given in favour of revaccination. He remarks as follows: "This important question is not yet settled. Nevertheless, very numerous official documents, collected particularly in Denmark, Sweden, Germany, Prussia, and England, and certain of which have been analyzed by two distinguished physicians, MM. Dezeimeris and Hardy, in the 2d volume of the journal *L'Esperience*, prove that variola is very frequent in individuals who have been vaccinated. This is an undeniable fact, and has, indeed, been long known." M. Grisolles quotes M. Serres, as stating that, within a few years, more than one-third of the persons reported diseased with variola have been the vaccinated; and the proportion increases yearly. Again, "variola (*Grisolle*) which is rare within 9 years from vaccination, is very frequent (*sévit surtout*) in persons who have been vaccinated for 10, 15, or 20 years. This authorizes us to believe that the vaccine virus only temporarily protects from variola, and that it is proper to revaccinate. It may be objected that (which is indisputable) after 35 years of age, the susceptibility of contracting smallpox becomes very feeble, indeed nearly null, which proves, according to Serres, that the presumed diminution of the preservative of the vaccine virus is not the sole cause of vaccinated persons being affected with variola." The author insists on the susceptibility of young subjects to the contracting of variola, &c.; from this he would apparently deduce an argument for more frequent repetition of vaccination in youth, and so proportionately to diminish the operations with the lessening necessity. Furthermore, M. Grisolles adds: "Revaccination is generally practised in the countries of northern Europe, and is beginning to be adopted in France. By the aid of revaccination, applied generally, variola has been almost completely extinguished in the Prussian army, and in the kingdom of Wurtemberg."—*Pathologie Interne*, vol. i. p. 105.—
SECRETARY.]

Tænia.—Dr. STORER referred to a case. A middle-aged woman, after suffering from an irregular condition of the bowels for several months, voided three feet of this parasite, and after the administration of an active cathartic, two feet more were passed. Half an ounce of kousso was administered, in infusion, without producing the slightest effect.

Dr. S. referred to this case, particularly, to call the attention of the members of the Society to the unusual number of similar cases which have been reported during the *last* year.

Dr. Storer asked if cases of *tænia* are not comparatively rare in this country? From the infrequent reports of cases, hitherto, such would seem to be the fact.

Dr. ABBOT saw a case, the last week, and advised pumpkin seeds as a remedy.

Dr. PUTNAM mentioned a case in which these seeds were successful.

[Prof. PATERSON (*Philadelphia Medical Examiner*) states that, in 1852, he reported a case of radical cure of tænia by the use of pumpkin seed emulsion, after oil of turpentine and even kousso had signally failed. The fixed oil from the seeds is the anthelmintic principle, and has been employed with the happiest effects in doses of $\frac{3}{4}$ ss. twice in the day, followed by an ounce of castor-oil.—SECRETARY.]

Tubular Pregnancy.—The specimen, with a history of the case, was sent by Dr. T. F. OAKES. The patient was an unmarried woman, twenty-five years of age. Complained of occasional nausea and vomiting, and of pain in the region of the uterus, for a week or ten days before the fatal attack. On the 16th ult. she arose in the morning better than usual. At 11 A.M., while doing some light work in the yard, she was suddenly attacked with violent pain in the abdomen, faintness, and nausea. With some difficulty she was got upon a bed, and Dr. O. saw her half an hour afterwards. She was then so exhausted as to be unable to answer questions. Countenance pale and haggard; eyes sunken, and the whole surface cold and moist; pulseless; nausea excessive, with retching and occasional vomiting of bilious fluid. Complained of great pain in lower part of abdomen, with tenderness on pressure. Stimulents and opium were used, but she died in about four and a half hours from the time of the attack.

About fourteen pints of coagulated blood were found in the peritoneal cavity. The pelvic organs were removed, and after a partial examination, were handed to Dr. Jackson, by whom they were shown to the Society. The uterus is enlarged, measuring four and a quarter inches in length, now that it is cut open, and it is changed in structure as is usual in the gravid state. The decidua is remarkably well-developed throughout the fundus and body, but ends, as usual, at the commencement of the cervix; the only peculiarity noticed being the absence of the characteristic punctated appearance.

The existence of a decidua, in the case of tubular pregnancy, has often been questioned, as is well known. The left Fallopian tube, which contained the ovum, was of about the usual size for some distance from the uterus, and then very suddenly dilated into a thin, firm, membranous cyst, which must have been nearly as large as the fist, there being nothing like a decidua upon its inner surface. The ovum was sufficiently well-developed, and the fœtus was not far from three months old, the spine measuring two and one-eighth inches in length; cord seven and three-fourth inches long. The fimbriated extremity could nowhere be traced, and neither could the seat of perforation in the tube be found. These cases usually terminate, as is well known, in the rupture of the tube and hemorrhage into the peritoneal cavity. Dr. J. remarked that in all of the cases he has seen, excepting one, the hemorrhage had occurred at the sixth or eighth week; the period in the present case being unusually late. The ovary was closely connected with the dilated tube, and contained a well-marked corpus luteum, $1 \times \frac{1}{2}$ inch. It should have been stated that a pretty distinct puncture was found upon the inner surface of the body of the uterus, as if some mechanical attempt had been made to procure abortion.

Upon the other side was a fine specimen of dropsy of the Fallopian tube; a thin, firm, membranous cyst, of a rounded form, nearly, or quite the size of the fist, and filled with clear, yellowish serum. Upon the surface of this cyst, and intimately connected with it, the tube was distinctly seen when inflated, being considerably dilated until it was just about to open into the sac, where it suddenly contracted so as not to admit the smallest probe. The opening, however, was seen upon the inner surface; and the fact of its being free was

proved by inflation. The inner surface of the sac had the appearance of a serous membrane. Fimbriated extremity quite obliterated. Ovary spread out over the sac, and intimately adherent, though distinct.

*Powerful Effects from small Quantities of Opium and certain of its Preparations—Ingestion of five grains of Opium—Excessive Narcotism, &c.—Cases—Remarks—Table of Cases reported to the Society.—*Dr. MORLAND read the following account:—

A gentleman 76 years old, of spare habit and of much muscular activity, though somewhat infirm, and who had been suffering from loss of appetite, costiveness, &c., depending on slight derangement of the biliary organs, took five grains, accurately weighed, of solid opium, in pilular form, it having been mistaken for blue-pill mass; both substances lying together. The pill was swallowed on going to bed, at about 10 o'clock P. M., April 7, 1854, and the stomach was quite empty, nothing but fluids having been taken for the two previous days. It being noticed that Mr. — had not risen at his usual hour, a friend went to his room and found him fast asleep and breathing heavily, with some stertor; not supposing this of any consequence, he was left, and was afterwards visited twice, at about hour intervals, with the same result; it now being considered unusual, the same friend suddenly thought of the possibility of mistake, the evening before, in the medicine taken, and, examining the two substances alluded to, found, to his dismay, that opium had been used instead of the blue mass. A few minutes after this discovery, I came to the house for the purpose of visiting another patient, and was desired to see Mr. — immediately. On entering his chamber, found him lying on his left side, breathing with an occasional snore; he was bathed in perspiration, which, increased by warm coverings, had soaked his night-flannel and shirt, completely—had wetted the sheets and pillow-case, and even the ticking of the bed. He was immediately shaken, and attempts made to rouse him; the skin of the face was of a dark-red colour; he spoke indistinctly, and said that he would get up, that he had had a very good night, and he wished to know why he was shaken, &c.; between these expressions, he would sink off towards sleep again; finding him unable to rise, his linen and flannel were changed, and his body well dried with a towel, and he was then *taken up* and dressed by myself and an assistant, apparently almost insensible. He was next *walked about* and taken out into the air, his legs bending and dragging, and he sinking upon one assistant, if the other for a moment left him. Although it was supposed that the opium was wholly absorbed, having been taken under circumstances so entirely favourable to its complete effect, emetic action was freely induced by means of salt and water, and mustard and water (no more powerful emetics being at hand); emesis seemed more efficient than anything to cause an approach to waking; strong coffee was freely administered, with some effect; the fumes of strong water of ammonia were allowed to enter his nostrils, &c. &c.; continual efforts through the day were required to keep him awake—the eyes closing immediately on ceasing to engage his attention, and heavy sleep with stertorous respiration occurring whenever he was left quiet. At times, the lethargic state seemed to have a sudden access, and scarcely by shaking or by pinching, could he be aroused; often, he did not feel severe pinching.

Taking into consideration the previously constipated state of the bowels, and the additional reason for purgative action, injections of soap and water were given, freely (five in all; Oss at a time); also, hydrarg. chloridi mit. et pulv. jalapæ, aa grs. x; pulv. ipecacuanhæ, grs. v, in one powder; and, after some

hours interval, there being no operation, one and a half drops of croton oil were given, in pill of crumb of bread; in from $\frac{1}{2}$ to $\frac{3}{4}$ of an hour, a very moderate discharge took place. The urine was suppressed throughout the day, although efforts were made by the patient to pass it, occasionally, from a sensation as if of need. At the fecal evacuation, however, it passed freely. About this time there came on a troublesome itching of the arms and chest. Pulse, about 60 through the day; towards evening, 68 to 70; feeble. The dusky-red of the skin of the face, seen in the morning, changed to *extreme* pallor in the forenoon, and so remained; pupils of the eyes not remarkably affected; perhaps a slight contraction. The soporose state left the patient *quite suddenly*, within a few minutes even—efforts to keep him awake being no longer found necessary. The night was restless, with snatches of sleep, only. The effects of the dose were evident for many days; headache, obstinate constipation, unwillingness to move, and weakness, persisted; there was also complete anorexia. Patient is now, April 20, as well as usual.

Although the quantity of opium absolutely requisite to suspend vital action must always vary with circumstances, and often, especially, by the habits of the individual, even the quantity taken in this case might well enough have been fatal, under the existing conditions. CHRISTISON has recorded a case in which four and a half grains, combined with nine grains of camphor, were fatal in nine hours. Under other circumstances, doubtless, five grains would hardly have caused such very marked effects as have been above related. The prolonged effect of so little, comparatively—the excessive resistance to purgative agents and the long-continued *after* influences (not certainly very usual after recovery from the immediate effects), may seem of sufficient importance to warrant a report of the case.

Dr. Christison opportunely draws attention to the importance of knowing, approximatively, the *smallest fatal dose*; for, says he, “in consequence of the dread entertained by many unprofessional persons of this drug, it is currently believed to be much more active than it is in reality, and instances of natural death have been consequently imputed to medicinal doses, taken fortuitously, a short time before (*op. cit.* p. 657). The recording of such powerful effects as are occasionally observed from small quantities even, of opium and its preparations, may also be of service by inducing greater caution in the use thereof, without medical advice and prescription; it may tend to prevent the keeping of such potent remedies in places where mistakes are liable to be made, and where reckless use of the drug is possible.

GRISOLLE states that “he saw narcotism induced in a lady by the ingestion of three centigrammes of opium;” the *centigramme* being about one-sixth of a grain, this would give only *half a grain* for the quantity taken. The same author adds that twenty centigrammes (grs $3\frac{1}{3}$) have sometimes killed *adults*, and that one or two drops of laudanum may cause the death of a newborn infant. (*Path. Int.* vol. i. p. 827.) Laudanum has been said to become stronger by *being long kept*; Dr. Bigelow, Sen., declares this to be incorrect, at least when dropped without shaking;—if the sediment be stirred, a *very slight* increase of power is possible. OREILA (*Dict. de Médecine*, p. 261) says: “I have seen all the symptoms of an extremely severe (*pénible*) narcotism experienced by two young ladies, after the application of opium plaster to the temples.” He does not state the proportion of opium used in preparing the plaster; certainly the amount absorbed must be, comparatively, very small. TAYLOR (*Medical Jurisprudence*) gives a case as occurring in 1838, of a man, aged 45 years, who died from the effects of ten grains of solid opium which he had swallowed. Also, another instance, in which eight grains

proved fatal to a woman, 38 years of age; the quantity, moreover, was taken in two doses. Interval not given.

Many authors refer to the fact of very powerful and alarming effects upon young children by exceedingly small doses. Orfila cites the statement of SUNDELING, "that the smallest doses (*les moindres doses*) act with activity in infants; the works of medical authors abound (*sont remplis*) with facts which support this opinion." (*Dict. de Médecine*, en 30 vols. p. 259.) Members of this Society have, in former years, reported very marked instances. Dr. HALE related the production of narcotism in a child one month old, by one-twentieth of a grain of acetate of morphia. The same gentleman also stated the occurrence of "alarming" narcotism in a child, from taking one drop of laudanum; and said, moreover, that he had witnessed in another child the same symptoms, after the use of "cough mixtures" containing minute quantities of opium, in some form; and again, by "cough lozenges." Dr. H., from observation of many such instances, concluded that opiates have a greater effect upon children, in proportion to the quantity taken, than does any other medicine. (*Vide Records of Society*, 1843.) With a view of presenting the results of several cases of poisoning by preparations of opium, so many as it is possible to collect by search of the volumes of past records, will be grouped in a tabular form below; among them are other examples of strong narcotic action from minute doses, and especially in infantile cases. In consequence of the loss of one volume of the Society's *Records*, any cases therein cannot be cited; one or two are not tabulated, from want of precise data. It is believed that all but two or three are given.

While referring to cases observed in children, one published by Dr. EDWARD SMITH, of London, in the April number of the *Association Medical Journal* (London), may be mentioned. In this instance, one-twentieth of a grain of opium proved fatal to an infant six days old, in eighteen hours after the administration of the dose. Syrup of opium, of the French Pharmacopœia, was the preparation; three-fourths of a grain of crude opium to the ounce of fluid. Artificial respiration was most efficient towards keeping up the action of the heart, which latter was quickened or slackened, according to the use of this means. The usual symptoms of poisoning by opium were induced in about half an hour, coma closing the scene.

In 1838, at a meeting of the Society, Dr. G. C. SHATTUCK, Jr., while referring to the liability of children to be even killed by very small quantities of opium, remarked that, in adults, the habit of drinking ardent spirits may have some effect in diminishing the susceptibility to the action of opium. Dr. S. at that time asked Dr. Bigelow, Sen., what he would consider the minimum fatal dose of opium or laudanum?

Dr. BIGELOW replied that, according to the Edinburgh Dispensatory, it is about four grains of opium, or one hundred to one hundred and twenty drops of laudanum. He added, that he had previously stated to the Society the occurrence of several cases in which an ounce of laudanum had been taken with impunity. (*Records*, 1838.)

An important question, in all cases, is the *quality* of the preparation, and as laudanum is perhaps most commonly used for suicidal purposes, we observe, oftenest, variations in the effects of large and of small doses; idiosyncrasy in individuals is a more rare, but doubtless not an infrequent element in the wide difference of the action of the drug.

A case was some years since reported to the Society by Dr. CHARLES E. WARE, in which $\bar{3}$ ij of laudanum were taken by an adult female, entire recovery ensuing; she was not in the least narcotized, but, on the contrary, was

highly excited; there was spontaneous vomiting. She bought the tincture at two different druggists. It is very possible that an inferior quality of laudanum was dispensed; indeed, with a superior kind, her escape would have been extraordinary.

In the case which forms the basis of this paper, attention may be directed to the profuse *diaphoresis*. This is remarked by Christison in a fatal case examined by him. "The sheets," he says, "were completely soaked, to a considerable distance around the body." This effect of opium has been lately referred to in the Society, when it was administered, in the ordinary doses, frequently repeated, in inflammatory affections; diaphoresis being often obtained very readily, although some have supposed its use would tend, constantly, to constrict the skin.

Subsequent to the reading of the above case and remarks, Dr. PUTNAM mentioned an instance in which five drops of laudanum caused decided and alarming narcotism in an infant seven or eight months old. He also stated that thirty drops of laudanum thrown into the rectum of an adult patient, who had had profuse diarrhoea, caused powerful and even dangerous narcotism. In consequence of such occasional accidents, he is very cautious in the use of opiate injections in cases where there has been violent purgation or diarrhoea.

Dr. PARKMAN said he had known forty drops, given by injection into the rectum, to cause narcotism.

Dr. SHATTUCK, in this connection, reported the following case. The super-vention of narcotism, in this instance, from a small amount of morphia, is the more remarkable, because the existing pain would have been supposed capable of counteracting much of the effect of the medicine:—

W. E. C., ætat. 15 years, entered Hospital May 21, 1854.

Patient reports that, on the 12th inst., being quite warm from exertion, he drank freely of cold iced water, and on the same day ate five cents worth of mixed candy. Soon afterwards, was taken with abdominal pain and bilious vomiting.

At entrance, complains of abdominal pain and tenderness. Lies on right side, with knees slightly drawn up. Abdomen not full; the countenance expressive of languor and discomfort; cheeks of natural colour; skin dry; pulse 92, small, quick, and regular; tongue with dirty yellowish coat. Bilious vomiting. Pain somewhat relieved by sinapism.

May 22. Pain in abdomen continues; expectorated 3ij aerated serum, with small masses of yellow mucus floating in it, and streaks and specks of blood; occasional vomiting. R. Sol. morph. sulph. ʒj, every four hours; blister, 2 × 3, to epigastrium.

23d. Reports better; lying on right side, as yesterday; very languid and sleepy; has not vomited since last night; skin rather cool. No dejection. Large enema of infusion of flaxseed; sol. acaciæ gum., for drink.

24th. Actively delirious through the day, and very restless through day and night; no perception of light or sound; two free dejections from enema; no vomiting; passed urine involuntarily twice in the night. Stupid most of time since 6 A. M., and can now scarcely be aroused; pupils contracted, but no suffusion of eyes; no heat of head; pulse 90, more full than yesterday; respirations from four to seven per minute; skin temperate. Has taken medicine regularly until 3 A. M.; none since; swallows with difficulty. Omit morphia; blister, 2½ × 3 to back of neck.

26th. Lying still on right side; occasionally groaning; some restlessness; skin natural; pulse small, sufficiently strong, about 100; tongue moist, with whitish coat. Has taken food with relish. Abdomen quite tense, hard; resonance dull inferiorly; catheter introduced, and ʒxii pale urine withdrawn.

June 1. Has been doing well; taken light food. Has vesicular and exanthematous eruption on abdomen.

3d. Walked out.

6th. Diarrhœa produced by eating oranges and a plate of baked beans, in market; slept pretty well in the night; abdominal pain; five or six dejections. Hop fomentation.

9th. No tenderness of epigastrium, but soreness complained of in the hypogaster. Vomited yesterday, P. M.; matter ejected containing some bile, which, he says, is generally the case. R. Pil. hydr. gr. iij, repeated in six hours.

17th. Has been gradually improving to present time.

We have here a case of gastritis, from a large quantity of cold water suddenly introduced into the stomach when the patient was heated; pain and vomiting continuing eleven days, and subsiding under the influence of morphia, of which enough was accidentally taken to produce narcotism; but out of this state the patient came, without any other remedy than a blister to the back of the neck. The convalescence was long, the patient being a delicate boy; and though his diet in the Hospital was carefully regulated, he was imprudent elsewhere, and such causes had something to do with the protracted convalescence. The quantity of morphia taken was about a grain, in divided doses, during an interval of forty hours.

Death from Laudanum (3j) in three-quarters of an hour.—The following account of a case, which occurred in 1849, and was then reported, verbally only, to the Society, has been furnished, at the request of the Secretary, by Dr. G. H. LYMAN.—Mrs. —; age 52 years. Intemperate; has been drinking more or less for three weeks. An ounce of laudanum was taken by the patient, with suicidal intent. Dr. L. saw her in fifteen minutes. She said she had taken the laudanum for diarrhœa; was able to walk about with little or no assistance; begged to be allowed to lie down.

Nothing peculiar in pulse, as to force or frequency; pupils natural; external temperature natural; no evidence of sopor. Dr. L. gave two emetics of sulph. zinc.; could get no stomach-pump. No effect produced by emetics.

In thirty-five minutes after taking the laudanum, she began very suddenly to lose her pulse and muscular power; slight spasms were observed; the lips became livid; there was spasmodic dropping of the lower jaw; the extremities were cold, and, in ten minutes, she was unmistakably dead. The moment her pulse began to fail, frictions and sinapisms were resorted to, and preparation was made for the douche and artificial respiration; but she died before they could be instituted.

She had taken a hearty breakfast about three hours before. Her husband says her habit of drinking was induced by taking great quantities of tinctures (that of opium among others), after an attack of traumatic tetanus, ten years previous to her death.

Three quarters of an hour only elapsed from the ingestion of the laudanum to her death. BECK reports a case fatal in two hours. CHRISTISON says, seven to twelve hours is the average duration.

A case is reported in the *Boston Med. and Surg. Journal*, vol. xi. p. 285, fatal in two and a half hours. Christison also remarks that intoxication retards the action of opium.

[A case, fatal in three quarters of an hour, was reported shortly after the above, by Dr. COALE.—Laudanum, the preparation; amount taken, not known.—*Records*, 1849. SECRETARY.]

Cases of Overdose of Various Preparations of Opium reported to the Society since August, 1838. Chief Effects, Results, &c. &c.

Preparation.	Quantity.	How taken.	Subjects.	Results.	Effects observed.	Means used, &c.	Year.	Reporter.
Laudanum	℥j	Suicidal intent	Adult male	Recovery	Spontaneous vomiting; nothing serious.	None recorded.	1838	Dr. Otis.
do.	"2 table-spoonfuls,"	By mistake	Adult female	do.	No ill effects.	None recorded.	1838	" Hayward, Sen.
do.	7 drops		Child, 22 mos.	do.	"Severe" narcotism.	Powerful emetics.	1838	" Fisher.
do.	2 drops	In a mixture, by a drunken mother	Child, 6 to 8 weeks	do.	Stupor; lividity of face through 1 day.	None mentioned.	1838	" O. W. Holmes.
do.	℥j	Suicidal intent	Adult male	Death	No particulars given.	None mentioned.	1841	" Channing.
do.	℥iss		Adult female	Recovery	Coma; slow respiration; insensibility; cold surface.	Powerful emetics; injections; no effect for hours: hyson tea.	1841	" Storer.
do.	1 drop		Infant	do.	"Alarming" narcotism.	No means recorded.	1843	" Hale.
do.	℥ij	Probable suicidal intent	Adult male	Death	Dying when first visited.	No means used — <i>Post-mortem examination</i> . Congestion of stomach and small intestines; dark purple color of heart.	1843	" Conde.
do.	℥ss	Not stated	Adult female	Recovery	Spontaneous vomiting.	Sulphate of zinc ℥j. Free emesis.	1845	" J. B. S. Jackson.
do.	℥j	Not stated	Adult; sex not stated	do.	No particulars given.		1845	" Hale.
do.	5 drops	Injection into rectum	Child, 18 mos.	Death in 6 hrs.	Powerful action; symptoms alarming from first.	Had had cholera infantum.	1845	" J. B. S. Jackson.
do.	℥j	Suicidal intent	Adult female	Death in 4 of an hour	After 35 minutes, failure of pulse and of muscular power; slight spasms; lividity of lips; spasmodic drooping of lower jaw; cold extrineties.	Emetics; sulphate of zinc; no stomach-pump could be procured; no emesis produced by zinc twice given; frictions, stupisus, &c.	1849	" Lyman.
do.	℥j	Suicidal intent	Adult female	Recovery	Thorough narcotism; livid hue; insensible to impressions; no taste.	Ipecacuanha, ℥j—no effect; sulphate of zinc, grs 1x, in 3 doses—no emesis; mustard given with some effect; "walking about;" infusion of tobacco, in large quantity, in coffee; powerful emesis followed, with gradual recovery.	1851	" Strong.
do.	℥ss	Not recorded	Adult male	do.	No particulars given; "effects" of opium mentioned; subsequent mild pneumonia.	Injection of alkaline and acid mixtures, alternately into the stomach—no emesis; sulphate of zinc—free emesis.	1853	" C. E. Ware.
do.	5 drops	Not recorded	Child, 7 to 8 months	do.	Alarming narcotism.	No means recorded.	1854	" Putnam.

Laudanum	30 drops	By Injection in- to rectum	Adult male	Recovery	Dangerous narcotism.	No means recorded.	1854	Dr. Putnam.
do.	40 drops	By Injection in- to rectum	Adult; sex not stated	do.	Narcotism.	No means recorded.	1854	" Parkman.
Opium in substance	30 grains	"Taken in a "freak"	Adult female	do.	Slow respiration; rapid, full, throbbing pulse; coma; strongly contracted pu- pils; effects noticed in about an hour; seen by physician in 3 hours.	Bleeding; emesis; washing out of stomach by stomach-pump; walking patient about; pinch- ing, slapping, &c.; strong coffee.	1845	" J. B. S. Jackson.
do.	2 grains	By mistake	Child, 6 mos.	do.	Slow and difficult respiration; spasms; convulsions.	Washing out of stomach; injecting coffee and brandy into stomach; wetting of head with cold water; stupor; stupors to extremities; termina- tion by violent purging.	1845	" Condo.
do.	3½	Suicidal intent	Adult female	do.	Found on the street, vomiting; great excitation; pulse frequent, full; pupils contracted strongly; complete stupor, even after emetics; pulse subsequent- ly slow; respiration very slow; stupor passed off, and excitement recurred; then stupor, from which patient was easily aroused.	Emetics; 10 grs. of sulphate of zinc with 3ss of ipecacuanha; dose repeated in a short time, with addition of 20 grs. sulphate of zinc and 3ss ipecacuanha; no emesis produced; patient placed in the erect posture, and the stomach washed out, then 1½ pints of strong coffee thrown into it; patient began to rally in 20 minutes after coffee; walking patient about, brushing surface of body, &c.	1845	" J. B. S. Jackson.
do.	4 grs., with 4 grs. ex- tract of bel- ladonna	Accidentally	Child, 9 years	do.	Deep sleep; spontaneous vomiting, from which great exhaustion.	None recorded; the drugs taken after a full meal.	1853	" Coale.
do.	5 grains	By mistake	Adult male	do.	Excessive narcotism.	Emetics; forced exercise; coffee, &c. &c.	1854	" Morland.
Paregoric	"1 tea- spoonful"		Child, 6 weeks	do.	Stupor.	Emetic; no emesis caused.	1845	" Channing.
do.	5j	Suicidal intent	Adult female	do.	Coma; insensibility to impressions.	Stimulants; emesis by feather in throat; walked about; cold affusion.	1854	" Condo.
Acetate of morphia	1-20th of a grain	In cough mix- ture; repet- ed small doses	Child, 1 mo.	do.	Deep sleep; could not be aroused to take the breast.	No means recorded.	1843	" Halo.
Sulphate of morphia	1 gr. in di- vided doses (through 40 hours	Medicinally; in solution	Boy, 15 years	do.	Narcotism.	On omission of morphia, effects passed off.	1851	" Shattuck.

1 Dr. Anderson (*Monthly Journal*, April, 1854) declares belladonna *remedial* in poisoning by opium;—this instance would seem contradictory.

[The *smallest* amount recorded in the above table is *one drop* of tincture of opium; its effect, "alarming narcotism;" patient, an infant; recovery.

The *largest* amount recorded is ninety grains of opium; coma; recovery.

The smallest amount recorded as *fatal*, is five drops, *by injection* into the rectum; patient a child, eighteen months old.

Smallest amount fatal, *when swallowed*, an ounce of laudanum; two instances; adults; in one, death in three quarters of an hour.

Preparation of opium most frequently taken, *the tincture*; 17 times. Opium, in substance, 5 times; paregoric, twice; acetate of morphia, once; sulphate of morphia, once.

Total of cases reported, 26; male adults, 6; female adults, 8; children, 9. One boy of 15 years. Two, sex not stated.

Suicidal intent, 6 times; accidental, 4 times. Administered medicinally, 6 times; taken "*in a freak*," once; intent not recorded, 8 times; in a mixture, carelessly, by a drunken mother, once.

Most efficient means: Emesis; stomach-pump; forced motion; cold affusion; external stimulation; internal stimulants.

Owing to the lack of record, it has not been possible to state the intervals elapsing between the ingestion of the poison and the beginning of its action, and the termination of the same; neither can the time intervening between the swallowing of the drug and the visit of the attending physician, be ascertained, in a sufficient number of cases to make its record here of any avail.
—W. W. M.]

Colloid Tumour of the Neck—Several Operations—Frequent Recurrence—Death.—Dr. PARKMAN, exhibited a specimen, with daguerreotype views, and gave the following history of the case. A single woman, 39 years of age, a school teacher by profession, consulted Dr. P. in March, 1852, for a tumour the size of a goose egg, in the left supra-clavicular region of the neck. This was subcutaneous, smooth, round, without lobulations, elastic and fluctuating, as if a cyst with thick fluid contents, usually unattended with pain, but at the menstrual periods there was a marked increase of sensibility. The diagnosis being uncertain, it was decided to cut down upon the tumour, if a cyst, discharge the contents, and to proceed as might be indicated. This being done, there was discharged a thick tenacious, gelatinous, amber-coloured matter, so remarkably glutinous and adhesive, that it could be drawn into filaments two feet in length, resembling, very much, thick mucus, and filling the interstices of a sponge used to wipe it up in the same manner. This was followed by a more consistent substance, slightly grayish in colour, and with faint traces of structure. The finger passed into the cavity showed this to be circumscribed by an almost imperceptible cyst; and the various muscles, as the edge of the sterno-cleido-mastoideus, and the trapezius, could be readily detected. It seemed as if this substance had been deposited directly in the cellular tissue, occupying the space between these muscles. With a view of exciting inflammation and thus closing the cavity, a seton was passed through it. Such was not the result, however; but the cavity soon became filled with tumours growing from its walls. These were soft, painless on pressure, and easily broken down without hemorrhage or any ill effects consequent upon the manifestations. It seemed as if the tumours were composed of the gelatinous matter already described, deposited in a very fine cellular tissue. Under these circumstances, in May, the whole disease was removed by a clean dissection, exposing the muscles and fascia of the lower portion of the neck. The tumour thus removed presented the characteristics already described. Soft,

friable, glutinous, and held together so slightly by a thin areolar tissue, that its removal was impossible in any way but as an amorphous mass. Cicatrization went on rapidly, and in a very short time the patient was attending to her duties. She continued to enjoy perfect health, until March, 1853, when she called Dr. Parkman's attention to a small tumour outside of the original cicatrix, the size of a hazel-nut. There had been pain in the part previous to its appearance, three weeks since, but now there was no abnormal sensation. This tumour was immediately removed, a simple incision only being required. It was situated in the subcutaneous cellular tissue, and presented exactly the same semifluid, gelatinous, glutinous substance as the large tumour. In April a similar tumour, in May two similar, and in August one similar were removed. The appearance of these was always preceded by pain, which ceased when they were developed. They were all outside of, and without any relation to the cicatrix resulting from the original operation, and could in no case be regarded as the growth of any part of the first tumour, which might accidentally have been left.

The wounds all healed rapidly, the patient being hardly confined to the house. The general health during this time remained perfectly good.

In November, attention was again called to another reappearance of the disease. This time, however, the patient being somewhat discouraged, had allowed considerable progress to be made without mentioning its presence. The disease had reappeared as usual, under sound skin, but with its growth it had insinuated itself beneath the cicatrix of the first operation, and now occupied the whole of the supra-clavicular space. The health remained perfectly good. The complete removal of this mass was again advised, consented to, and performed. This time the dissection was much more extensive, exposing the whole superficial anatomy of the supra-clavicular space. The recovery was, however, rapid. The disease removed presented exactly the same characters as before.

January 1, 1854, attention was again called to another tumour, the size of a hazel-nut, which with similar antecedents to all the others, had appeared below the clavicle, over the coracoid process of the scapula. It resembled exactly all the others, and could as easily have been removed. It was decided, however, to wait one month, and an appointment was made for the first of February. On the 20th of January, however, Dr. Parkman was sent for, and found the patient convinced that there was to be a new development of the disease, from the peculiar pain which she experienced over the lower half of the left sterno-cleido-mastoid muscle. An examination detected, in a space of about three inches square, a certain degree of fulness; so slight, however, that it would not have been observed by one not fortified by past experience in the case. A few days, however, were sufficient to remove all doubts on the subject. Within a week a tumour developed itself of the size of a female closed hand. The impossibility of any further operative influence was now evident.

The further progress of the case may be detailed in a few words:—

This tumour having already shown such a tendency to rapid growth, continued to increase, and finally attained a size fully equal to the patient's head, having for attachment the whole side of the neck, and hanging pendulous over the chest. At several points the skin became ruptured from the distension, and the mass unfolded itself through the openings, and several large sloughs were thrown off. The substance of the tumour as thus shown was soft, friable, and could be torn with the fingers, without much hemorrhage, and with

no pain to the patient, presenting always portions of the soft gelatinous matter with the more concrete texture, which has been described. With its advance, however, the tumour became comparatively more solid and firm, though without any actual change of structure.

With the growth of this tumour, the general health began to give way, and death finally occurred in June, about five months after the rapid development first described, and about three years from the commencement of the disease.

The *autopsy* showed extreme emaciation of the whole body. The tumour, as already described, was cut away from its bed, and it was then seen how the disease, originating in the cellular tissue, had gradually attacked the muscles in the vicinity. But a faint trace of the sterno-cleido-mastoideus muscle could be detected, and all the others were more or less involved. The softness of its texture, identical with that already described, prevented any preservation of its form. There was no other appearance of the disease save a deposit the size of an orange, on the edge of the opposite breast. The viscera of the abdomen and chest were perfectly healthy, save some œdema of the lungs, the symptoms of which had been observed a few days before death.

The points of interest in this case will be perceived to be the limitation of the disease, with but one exception, to a single region of the body; the rapid cicatrization of the wounds; the preservation of the general health until the last attack, and the very rapid growth of the disease—so rapid as to be almost perceptible from day to day.

Subjoined is a note from Dr. SHAW, giving the result of several microscopic examinations, made upon the tumours after their removal, at various times; as also upon portions torn from the surface of the tumour, when *in situ*, as has been described.

“My first examination of the tumour from the region of the clavicle, was in *March*, 1853, about a year after its first removal. The tumour was then made up of cells and free nuclei, with but a trace of fibres, and these very fine and pale. The cells were such as are generally found in well-marked cancer, except that they were somewhat smaller, and many of them, instead of presenting the globular or circular form, were elongated or otherwise distorted, having the appearance, so to speak, as if they had run wild. They did not, however, have the appearance of the fusiform fibro-plastic cell. The cells measured, on an average, 0.017 millimetre, and the nucleus 0.012 millimetre, the mean diameter of the typical cancer-cell being from 0.020 millimetre to 0.025 millimetre, and that of the nucleus from 0.010 millimetre to 0.015 millimetre. Though the cell walls were small, the nuclei were of the average size, generally oval, but sometimes globular, with dark and well-marked contour, and nearly filling the cell. Many of the nucleoli were large, highly refracting, and having the appearance of an oil-globule, whilst others were smaller.

“In *April*, the tumour presented the same appearances under the microscope. Many of the cells were elongated and distorted, as before, and their walls pale; yet the nuclei were very large compared with the size of the cells, and the nucleoli large and highly refractive. As before, the cells were a trifle smaller than usual. Small capillaries were observed, admitting the passage of only one or two blood-globules, and these were compressed and elongated so as to have an oval form.

“*November*, 1853. The cells were perhaps a trifle larger than before; the nuclei still large when compared with the size of the cell, with dark contour, and the nucleoli well-marked and oily-like. The nuclei were not merely larger and more prominent than those of the fibro-plastic cell by measurement, but they nearly filled the globular cells, or were nearly as large as the transverse diameter of the elongated cells. There were as before many free nuclei.

“*February*, 1854. At this examination, the cells would not pass so well for cancer-cells as at the previous observations. They varied from the normal form

more than ever, being less regular in size, and in many the nuclei were indistinct, or wanting. Some were found with large nuclei, but wanting nucleoli, and in many instances the nuclei themselves were small.

"*March, 1854.* Last examination. The cells and nuclei at this time were still further from the typical form of cancer. The cells were quite small, but now generally globular, and very pale; the nuclei nearly filled the cells, but were pale, and below the average dimensions; the nucleoli were mere points, or were absent. The cells were few, the free nuclei predominating. None of the cells were fusiform in this or the previous observations, nor did the cells or nuclei correspond to the type of the fibro-plastic elements.

"If any value is to be placed upon our present knowledge of cancer and fibro-plastic elements, it would seem that this growth should be termed cancer. At the commencement, its minute structure had all the appearance of cancer, except that the cells were somewhat smaller than the average (which is not an uncommon circumstance), and were considerably distorted in form. But the nuclei and nucleoli, during the earlier stages of the disease, were typical. They differed from the fibro-plastic elements both in actual and relative dimensions. The nuclei had the dark contour, and the nucleoli the refractive power which are wanting in the fibro-plastic nuclei and nucleoli. Why the minute structure became changed during the progress of the disease, of course it would be impossible to say. Possibly, from the superficial situation of the tumours, and their frequent removal, the cells had an opportunity of easy, rapid, and unrestrained development, such as they do not possess in the ordinary situations and forms of cancer.

"The peculiar character and history of this case may be some apology for an unsatisfactory microscopic examination; and it cannot be expected that the microscope will determine the nature of doubtful growths, until the minute structure of well-known morbid growths shall be better understood."

Syringe for the Injection of Melted Lard into the Vagina, in Parturition.—Dr. COALE exhibited a syringe which he had contrived, and found very useful for throwing warm lard into the vagina, in cases of dryness of that passage in protracted labours, particularly where the head had been pressing some time on a resisting perineum. It consisted of an elastic nozzle some three and a half inches long, to which is attached an India-rubber tube two feet long, having an ivory socket at the end. An India-rubber bottle with an ivory nozzle fitting accurately into this socket, completes the simple apparatus. The elastic nozzle is introduced, directed by the forefinger. The ivory socket remains outside the bedclothes. The nurse having melted the lard and filled the bottle, introduces its nozzle and makes the injection, during which the elastic nozzle in the vagina can be moved about to distribute the fluid. Dr. C. has found that the relief given in this way to the severe pain of tension, and sense of burning, is more than he would have supposed, whilst the labour was in several instances evidently greatly expedited, both by the mechanical lubrication of the lard, and by its softening the parts, and making them yield more readily.

June 26. Pearl Gaiter-Button removed from the Left Nostril of a Child Eighteen Months Old.—Dr. MORLAND showed this, and stated the difficulty he had experienced in extracting it, and which doubtless was in some measure owing to the smoothness, hardness, and shape of the foreign body. A polished disk of the size of this button is far less easily seized and withdrawn, or pried out, than is a bean or any such object. The eye of the button was broken, and, as was seen after its removal, presented a sharp point, which must have wounded the mucous membrane of the nostrils during the extraction, and caused some bleeding. A bent probe, a scoop, and the ordinary small-sized forceps were

unavailingly employed, the button seeming to glide and escape every effort at seizure. A pair of very delicate curved eye-forceps were finally used with complete success at the first effort. They were inserted *closed*, and employed as a lever, prying the button from behind forwards and outwards. It escaped with a *leap*, as it were, going to the distance of several feet. Dr. M. added, that surgeons recommend the use of a small *curette*, or *scoop*, or a *bent probe* (Druitt, Fergusson), or else that, failing extraction, the foreign body "be pushed back into the throat." Removal by the anterior nares is surely very desirable. As to the *scoop*, in one or two instances, the size usually furnished in the pocket-case has been found too large by Dr. M. Certainly, in infants or young children, who are most frequently our patients in these instances, such is likely to be often the case. In several instances mentioned to him, the operators were obliged to resort to very delicate (though strong) instruments; and in one to those contained in a case of eye instruments.

The child above referred to, introduced the button at about 7 o'clock P.M., June 23, and it could, by account, be readily seen at first; but in the hurried and injudicious efforts made by the nurse and others to remove it, they pushed it completely out of sight, and it could with difficulty be reached by instruments, when the child was brought to Dr. M. at 10 o'clock P. M. The child is small, and the features delicate. The nasal apertures quite small. Button nearly half an inch in width, and one-eighth of an inch thick.

Carcinoma of Liver.—Case reported by Dr. STORER. Margaret M——, widow, aged 50, entered Hospital, June 11, 1854. Was well until January last. At that time, noticed an enlargement in right side of abdomen; appetite became impaired, and she experienced slight bilious vomiting, and more or less drowsiness. For last three months has been unable to work, and for some weeks past has kept in bed all the time. For the last ten days has rapidly failed in flesh and strength; except on one occasion, during this period, has not taken any nourishment except wine-whey. Much emaciation; skin deeply tinged with bile; tongue with a dirty, whitish coat; bowels free. On examination of abdomen, a hard, resisting body is observed in right hypochondrium, extending from the edge of the short ribs behind, to within an inch and a half of umbilicus, and outwards and downwards, to inguinal region. Remainder of abdomen tympanitic throughout, with the exception of a small portion just below epigastrium, which is very tender upon pressure, and slightly emphysematous.

Advised tumour to be bathed night and morning with tinct. iodine, and patient to drink freely of the infusion of wild-cherry bark, and to have wine-whey *ad libitum*.

13th. Complains of great pain in abdominal tumour. An effusion noticed on the right of tumour, occupying a small circumscribed space. Copious bilious dejections.

14th. Much less pain in tumour upon pressure.

15th. Constant disposition to vomit; matter ejected, a dark-coloured, bilious fluid. Complains of great prostration. Skin dry and husky. Pulse 120. R. Mist. calcis carbonatis ℥ii; tinct. opii gtt. x. every two hours until relief.

16th. Vomiting continues; hands cold and damp; pulse 120. Died at 6 P. M.

The following notes by Dr. ABBOT, the Admitting Physician of the Hospital, who made the autopsy, are copied from the Hospital Records:—

Body greatly emaciated, of a bright, golden-yellow colour. Lungs healthy. Some slight, old adhesions between upper and middle lobes of right lung. Heart

healthy. Half an ounce of yellow serum in cavity of pericardium. Before opening abdomen, the tumour, recognized during life, was distinctly felt; also a decided fluctuation quite across abdomen. On opening abdomen, the stomach was found to be enormously dilated, extending across the front of abdominal viscera, and from ensiform cartilage to pubes. It contained a quart or more of greenish-black fluid, somewhat viscid. Previously, as much more escaped from the mouth in removing the body before opening it. Stomach very thin, pale, and suffused by cadaveric changes. A pint and a half of yellowish serum was found in the abdominal cavity. Intestines generally much injected, with here and there portions of slimy lymph on surface.

General contour of liver normal. In the substance of the anterior portion of right lobe a mass of firm carcinomatous disease of the size of a turkey's egg was imbedded, not, however, coming to the surface or affecting the outline thereof. A part of the organ free from disease. Colour generally presenting no unusual appearance. Under surface of that portion where the disease existed, adherent to a mass of carcinomatous disease somewhat larger than two fists, which extended to the left and backwards across vertebral column, to which it was firmly adherent, including the pancreas and gall-bladder, and adherent to the viscera in its vicinity, viz: the stomach and large intestines. The mass was, in general, of great firmness; portions of it, however, broke down easily under the finger. It was supposed that an abscess of three or four inches, in an irregular cavity, was broken into and discharged in securing the specimen. Other organs not examined.

Dr. J. B. S. JACKSON gave the following particulars relative to the *post-mortem* appearances:—

An encephaloid mass, not far from the size of two fists, existed in the region of the gall-bladder, and matted together all the neighbouring parts; the liver itself being of about the usual size, and somewhat, though not very extensively, diseased. Upon cutting through this mass, the gall-bladder, which could not before be found, was exposed, and in its cavity was about a dessert-spoonful of biliary calculi; the parietes were dissected, but it did not appear that the carcinomatous deposit commenced in them.

Female Catheters;—preferable pattern.—Dr. PUTNAM remarked that he was lately requested, by a medical gentleman, to employ the catheter for a member of his family. The instrument used was perforated with holes in the usual way, and gave the patient no uneasiness.

A short time afterwards, the gentleman informed Dr. P. that he had introduced the catheter himself, but that, in attempting to withdraw it, he met with considerable resistance, and, on persisting, the pain, which had already been severe, increased to an amount he had rarely witnessed in surgical operations.

The catheter he had used, had, instead of holes, a slit on each side about one-tenth of an inch in width and half an inch in length.

It was suggested that the mucous membrane might have got sucked into these slits and thereby caused the difficulty; but, lest it should be due to peculiar irritability of the bladder, or to some other cause, it was decided to etherize the patient rather than submit her to a repetition of the pain. This was done once only, and a catheter with the common holes was afterwards used without difficulty.

It happened on the following day that another medical gentleman consulted Dr. P. in regard to a patient in whom a similar difficulty attended the use of the female catheter. This also was made with slits, and on substituting the other, all trouble ceased.

Dr. Putnam almost invariably had used a catheter with slits, in the male subject, and had never met a similar embarrassment.

Remedies in Rheumatism—Antimony and Opium—Lemon-Juice.—Dr. HOMANS referred to his former use of antimony and opium, combined, in rheumatism; when free diaphoresis took place, he had found them efficient at cure.

Dr. H. had given lemon-juice, very freely, to one patient, lately; there had been both gout and rheumatism. Age of patient, 35 years; quantity taken, the contents of one-half a dozen lemons the first day (which was the fourth day of the disease); second day, eight or ten lemons consumed; and, before finishing, the larger part of a box. The attack appeared to be diminished in intensity and shortened in duration, according to the usual average of continuance. There was subsequent excessive derangement of the digestive organs. Dr. H. had never seen so much disorder follow the employment of the lemon-juice—for weeks there was complete anorexia.

July 10. Aneurism of the Aortic Arch compressing the Trachea, without Physical Sign of Aneurism.—Dr. H. J. BIGELOW exhibited the specimen, and presented the following case, drawn up by a relative of the patient:—

The deceased was 37 years of age, of a remarkably vigorous constitution, free from any constitutional ailment, and since his youth has not experienced an illness, of a day, sufficient to detain him from his daily business until his final one. About two years since he complained slightly of an affection in his throat, which he supposed was bronchial, as at times he felt a tickling sensation, and at times also expectorated considerably. He did not then complain of difficulty in breathing. He remained in this general state for perhaps a year and more, sometimes so little troubled that he did not speak of it for weeks, but upon taking cold, he complained of his former difficulty. This bronchial affection, as he supposed it to be, became aggravated at intervals until some time in March of this year, when, as he afterwards stated, he ran about a quarter of a mile through the rain to arrive in season to take an omnibus, and thus save a long journey on foot. From this he dated his first real difficulty in breathing. Shortly after this he discovered a slight whistling noise, which he could make voluntarily, but which was not constant, without such effort. This noise increased rapidly, and he began, in April, to find that too much exercise was an additional aggravation to it. Early in May he was examined by Dr. Bigelow, with reference to the cause of this singular noise, and his obstruction in breathing; these examinations suggested several causes, and among others that of aneurism as most probable.

At this time a very little exertion "put him out of breath;" he was obliged to walk very slowly, and the effort of going up stairs was so great that it required a considerable time to recover his tone. He returned to Boston, and in a few days the exertion of walking became too much for him, and he then kept in the house, gradually restricting himself in his movements. From June 18, he was mostly confined to a sofa, where his occupation was to arrange and rearrange his pillows to gain some comfortable breathing position. For a number of days previous to this, he said that he could sensibly feel the obstruction as the air, in its difficult expirations, glided over it. Until within ten days of his death he insisted on going up and down one flight of stairs, but it was a very slow and tedious exercise. He was carried up the last time, and after being laid upon the bed, he lost his breath and sprang for the open window; failing here, he ran through the house to the window in

the front of the house, where, after two and three-quarter hours of most super-human exertion, he recovered sufficiently to rise from his knees, and was placed upon a couch which had been prepared for him at this window. In this effort his breathing was sometimes gasping, hoarsely and loudly, and sometimes produced quite a shrill whistle. He was never removed from this window—all the doors and windows on this story were kept constantly open to provide air, and his efforts to raise began then to strangle him, so that he was liable to spring up choking, at any moment. The second day after taking his last position, he strangled and was held nearly three hours, seemingly, in the agonies of death, gradually growing weaker and worse, until, having passed through every stage, he fell lifeless into the arms of those who supported him; his pulse had then stopped; there was no perceptible beating of the heart; his eyes, and indeed everything, betokened death. He was then placed upon the couch, and in this situation he remained perhaps three minutes, when suddenly his eyes closed, his lungs became inflated, and in two or three minutes he spoke aloud, in a tone more natural than he had employed for months. He then said that he breathed about as well as ever—felt no obstruction nor any uncomfortable sensation excepting such weakness as made it even impossible to move his arm. He thus breathed freely six or eight hours, when, upon putting the ear to his chest, the same sounds as before became audible. This extreme difficulty came on again, and he lived to pass through seven spasms like the one described, varying in intensity, but diminishing as his strength passed away. During the first spasm his expectoration began to be mingled with blood, and ever after and from that time, he complained of a new kind of obstruction ^{above} the place of the other, which, from his raising, appeared to be caused by clotted blood. All day, July 4 and 5, his strength was so utterly gone that he pushed down the obstructions from the outside with his finger, gradually breathing shorter and shorter until evening, when he expired.

The specimen presented a dilatation of the arch of the aorta, apparently ensuing upon atheromatous degeneration, about three inches in diameter, adherent to the trachea, where it crossed it just above its bifurcation, compressing it, and opening into it by an orifice of the size of a dime, which, however, was most effectually plugged by a slender, thin wall of lymph—the result of inflammation, and which had alone hurried death by hemorrhage.

Dr. Bigelow had repeatedly ausculted the patient, as had also several gentlemen, among whom was Dr. Bowditch, and no abnormal sound whatever, connected with the circulation, could be detected—neither impulse, thrill, nor murmur. The obstructed trachea alone yielded a noisy respiration, a little louder in one lung, its maximum near the supra-sternal fossa.

Dr. B. remarked that similar cases of absence of evidence of thoracic aneurism are not very rare. He thought that thoracic compression of the trachea, gradually increasing, might, in default of evidence of other disease, be attributed to aneurism as its most probable cause.

Urinary Calculus formed upon a Leather Shoestring.—Dr. BROWN, of Bangor, Maine, exhibited the specimen, and reported the case. The patient was a man, æt. 27, and grossly addicted to onanism. Fifteen months ago he passed the string into the urethra after he had gone to bed, and went to sleep without removing it; in the morning it had disappeared. One or two months afterwards, urinary symptoms appeared; and these became so urgent, that he was obliged to give up work last autumn, and for some time past has kept his bed. About a week ago the calculus was removed by Dr. Rich, of Bangor,

assisted by Dr. Brown; the lateral operation of lithotomy was performed, and the patient has done well since.

The calculus is of a regular, oval form, somewhat flattened, and measures nearly two inches in length. The chemical composition, according to the analysis of Mr. J. C. White, student of medicine, is: "Phosphate and carbonate of lime—the latter being slight—with a slight trace of double phosphate of ammonia and magnesia. No uric acid nor urates in the portions examined." One extremity of the calculus having been broken away in the removal, the foreign body has been fully exposed.

Prof. MUSSEY, of Cincinnati, who was present at the meeting, mentioned a similar case in which he had operated, about two years since. The patient was a young man who had introduced (in accordance with medical advice, as he stated) a piece of cord into the urethra, for the purpose of allaying irritation. While introducing it, an unexpected occurrence surprised him, and caused a suspension of the operation, during which time the cord disappeared.

Dr. Mussey saw the patient three months afterwards, and was led from the symptoms to suspect the presence of a calculus in the bladder; at that time, however, it was impossible to detect any stone on examination by the sound. Two months later, a calculus was at once distinctly felt, and the operation of lithotomy was performed. The calculus extracted resembles a petrified lumbricus. The nucleus was the cord introduced, as above mentioned.

Monstrosity. Anterior Parietes of the Abdomen Deficient—Peculiar Form of Spina Bifida.—The specimen was sent to the Society by Dr. R. M. HODGES, and exhibited by Dr. JACKSON. Labour occurred at the seventh month; the woman (Irish) having previously had a large family of children. Pains came on about noon, and she set out to walk a distance of between one and two miles to obtain a permit of entry into the Almshouse. On her way she felt something coming from the vagina, and this subsequently proved to be the intestine of the fœtus. Her case being urgent, she was sent to the Cholera Hospital, as being nearer than the almshouse, and there Dr. H. saw her at half-past three P. M. On examination, the intestines, liver, and the two elbows were found to present; the lower extremities, as it appeared afterwards, being bent entirely backwards; a hand was soon forced down, after which there came one pain and the fœtus was expelled, doubled up in a most remarkable and altogether reversed position. This was at 4 P. M. The child was quite fresh, though stillborn.

The anterior parietes of the abdomen are entirely wanting. The liver is a very prominent object, and somewhat irregularly developed; gall-bladder and ducts traced. Spleen of usual size. A considerable portion of the stomach and intestine had been torn away before Dr. H. saw the woman. The rectum opens freely upon the serous surface, but near to the line of the integument. The urinary bladder consists of two cavities, sufficiently and about equally developed; these communicating freely towards what would be the neck, and the one from which the remains of the urachus arises, communicates almost throughout with the rectum. The right kidney was very prominent, and in the condition of hydronephrosis, being as large or larger than the stomach of the fœtus would have been if distended; presented the usual appearances upon the cut and upon the inner surfaces of a distended kidney, and was filled with fluid. The ureter was much enlarged, and towards the bladder ended in a cul-de-sac. Left kidney sufficiently normal; ureter accidentally cut off. Renal capsules normal. Upon each side of the

opening from the rectum, and at some little distance from it, though still upon the serous surface, was another opening of an oval form, and sufficiently large to admit a small director. The symmetry of these two openings, in regard to position, size, and form, was striking; and on passing in a probe, it was found to play about freely as in a large cavity. One of these cavities was then cut open, and was found to be a pretty well developed vagina. From this last there arose a uterus, having its os tincæ and arbor vitæ well marked, and sending off from its fundus, and towards the right side, a distinct Fallopian tube. The vagina and uterus upon the left side were not cut open, but externally the appearances upon the two sides corresponded precisely. There was upon each side, then, one-half of a uterus, with its tube, and one-half of a vagina, each being nearly or quite as largely developed as the entire organ is in a well-formed fœtus; the two uteri being quite prominent objects, situated far apart, near the margin of the integument, and directed upwards and outwards. The ovaries were not found. A trace of the external labia was seen on each side, and just where the integument ceased. The two pubic bones, as they may be called, were separated, as usual in cases of extroversion of the bladder, and to the extent of nearly or quite one and a half inches. The umbilical cord was inserted, as usual, upon the margin of the integument. Thoracic organs sufficiently well.

Spina bifida also existed. A sac, nearly or quite the size of two fists, hung off from the region of the sacrum, consisting of integument, lined by a serous membrane, and containing, by estimate, ten or twelve ounces of serum; opening from the spinal canal, prominently seen, as soon as the sac was laid open. Dr. J. remarked upon this form of spina bifida as having been observed here in some few other cases, and he thought it had not generally been described. The peculiarities in this case are in the situation over the sacrum, and in the extension over the tumour of a well-developed cutis; whereas in ordinary spina bifida the malformation is higher up, and the integument is deficient to the extent of the malformation. The large size, and the thinness of the sac, with the large collection of fluid, are also observed in the present variety; though in two cases, on the contrary, that have been observed here, the tumour resembled a female breast over the lower part of the child's back, and in one of them, which was dissected, the cavity within was quite small, and there was a large quantity of fat between it and the skin. The fœtus was otherwise well formed externally.

Varicose Arterial Tumour.—Dr. H. J. BIGELOW presented the specimen, a tumour upon the forehead of a middle-aged female. It was of the size of a large hen's-egg, directly over the right eye, projecting upon the forehead, and dipping beneath the orbit; convoluted in its surface; of a brilliant red, pulsating heavily, and filling to its utmost capacity in about three seconds after the blood had been pressed out of it as from a sponge. It was painless, of about two years' duration, gradually increasing, and originally near the hair, from which situation it seemed to have gravitated to its present position. Numerous dilated arteries radiated from its circumference. At the operation, the larger trunks were secured by ligature, but the tumour continued to fill with blood until nearly the whole periphery had been included in successive ligatures. With a view to its radical extirpation, it was then rapidly excised within the ligatures, when the exposed surface bled obstinately, even after the use of the actual cautery. The wound rapidly healed. A wax injection of the tumour was shown, from which the skin had been dissected. In form, it resembled a solid bundle of common earthworms. The convoluted arteries

suddenly expanding and contracting, mutually to inosculate, were inextricably interwoven.

Dr. B. remarked, that a thesis, with plates, had been long ago written upon this rare disease by Breschet. He had himself seen it in two cases in the hand. In the case of a young girl, described by Dr. Townsend, an affected finger had been since amputated, and some of the dilated trunks in the hand and arm tied with great relief. In the other, the case of an old woman, one finger had been converted into a flexible, pulsating mass, the bones having undergone almost complete absorption.

July 24. Membranous Croup in the Infant—Cases, &c.—Dr. COTTING, Associate Member of the Society, showed the trachea of a child who died of *croup* at the age of fifteen months, and gave the following account of the case: Patient was taken on Tuesday P. M., July 18; passed a restless, uncomfortable night. The difficulty of breathing was the chief symptom, but was attributed to a “stuffed cold.” During the next day, the breathing, though difficult, was not so distressing as the night previous. At dark, on Wednesday, the labour of respiration was so great that the parents became alarmed, and sought medical advice. They had not considered it *croup*, although they had had previous experience in the disease. Two years before, an older child (five years) had a severe attack, and, after five days of constant inhalation of steam, threw off the membrane and recovered. According to Dr. C.’s experience, very few cases of membranous *croup* are suspected to be *croup*, or to be of much moment, until the physician gives the startling announcement. Other forms of *croup*, so called, but misnamed, and of very little danger, generally from the onset cause great alarm.

The patient from whom the specimen was taken, died on Thursday, 20th, after an illness of about thirty-six hours. The croupy exudation covered the under side of the epiglottis, was more consistent over the glottis, and formed a complete membrane, which could be readily raised for about five lines below the chordæ vocales. The inner surface of the remainder of the trachea was of a bright red colour, diminishing in intensity from above downwards, but extending, quite visibly, below the bifurcation, and into many of the branches. There was some slight congestion of portions of the left lung, near the smaller bronchi. No other morbid changes noticeable.

The case is interesting on account of the youth of the patient. Some years since Dr. C. performed the operation of tracheotomy on a patient of less than seventeen months. The operation was performed on the following grounds, viz: the child’s danger would not be increased; the parent strongly urged it, having in some way heard that it might afford an additional chance of relief; and lastly and chiefly, that if, as believed, the membrane, or exudation, progressing from above downwards is less liable to descend far, in inverse proportion to the age of the child, an opening made into the trachea low down would possibly go below the membrane, and offer a greater chance of success. The operation was unsuccessful. No *post-mortem* was allowed.

On relating this case to one of great experience in this disease, a doubt was thrown over the diagnosis on account of the youth of the patient, and on that account only. Admitting that no case can be proved to be membranous *croup* unless the membrane can be produced before or after death, and a *post-mortem* in this case having been denied, there was no alternative but to wait till time and chance gave further opportunities of testing the question.

Two cases of recovery in children under one year, and one in a child of twenty-months, have occurred to Dr. C. since the time alluded to. In these,

sufficient fragments of membrane were thrown off to prove the disease. In a family where one child had some months previously died of croup, he was called to a child of five years, in the last stages. While this child lay dead in the house, another, of six and a half years, was taken. During the sickness of this child, some slight symptoms of the disease were supposed to be detected by the mother in a pair of nursing twins. On examination, the throats of both these infants appeared to be covered with the croupy exudation. They seemed to be well; nursed well; and it was only occasionally in their crying and slight cough that any suspicious sounds or symptoms could be detected. If the disease had not been so fearfully present in the house, it would never have been suspected in these children. Some fragments thrown from their throats could not be distinguished from those from the oldest child, or those taken from the deceased. The cases of these two infants had a decided influence on Dr. C.'s willingness to operate in the instance before spoken of.

The specimen presented to the Society, proves unequivocally that true membranous croup has occurred at the early age of fifteen months. It is undoubtedly more frequent in older children; but when the diagnostic symptoms are present, the young age of the patient must not be too much relied upon.

Uterine Polypus.—Dr. J. B. S. JACKSON gave the account of the case and specimen. The tumour is nearly the size of two fists, arises extensively from the cervix, and hangs into the vagina, which is much distended; form rounded; structure indistinctly fibrous, a free incision having been made through the mass; some appearance of vascularity upon the external surface, and at depending part. Where the neck of the tumour passes through the os tinæ, the two are intimately adherent, though the outline of this last is distinctly enough seen. At one point, however, there is an opening through from the vagina into the cavity of the uterus, about large enough for the passage of a small bougie. The uterus itself, through which a free incision has been made from the fundus into the neck, appears to be in every way perfectly healthy. The patient has been under the care of Dr. Samuel Gregg for about fifteen months, and from him the following history was obtained: Her age was 36 years. She had miscarried five times before carrying a child to the full term, and each time with considerable hemorrhage. In January, 1850, she had her second and last child at the full term, preceded by dangerous hemorrhage, as the placenta was over the mouth of the womb. From this last confinement she recovered well, but did not nurse her child. She was afterwards troubled with leucorrhœa. In about four months she began to have attacks of colic and gastralgia, and soon afterwards she was treated for hemorrhage, which continued more or less at intervals until her death. Since Dr. G.'s attendance, she has been perfectly blanched, and mostly confined to her bed. She, however, retained her flesh and appetite, and for a few months last autumn and winter, when the hemorrhage was slight, she gained strength and colour. The tumour was spoken of by the physician who was previously in attendance, only a month or two before Dr. G. saw her; enlarged considerably afterwards; examinations always caused more or less hemorrhage; and it was examined only once with the speculum, at which time it appeared very much as it did after death. It was never painful nor tender to the touch; caused no sense of bearing-down, nor dysuria, and scarcely gave any consciousness of its presence. The catamenia were regular, and entirely without pain, though sometimes attended by hemorrhage; the most severe hemor-

rhages, however, occurred generally a week or two after the flow had ceased, from which circumstance Dr. G. concluded that they proceeded from the tumour. A serious leucorrhœa existed most of the time during Dr. G.'s attendance.

August 14. Additional particulars of a Case formerly reported to the Society as "Ulceration of the Cæcum," (see this *Journal* for July, 1851, p. 65.) — *Death of the Patient recently—Post-Mortem Examination—Correction of the Diagnosis.*—Dr. STORER reported the following facts: In the summer of 1852, a year after the reference to her case to this Society, Miss L. was suddenly seized with severe pains in the back, where swellings in a few days appeared, followed by suppuration. In a short time an opening also appeared in the affected groin, through which pus was freely discharged. Her health became much impaired, and for a period her condition was extremely critical. During the year past, however, she has suffered much less, and has even remarked she "would almost have forgotten," to use her own words, "that she was not well." In April last, her feet began to swell, and about the last week in May, she became very feeble, and from that time has continued to fail. She died a few days since. Although no fecal matter has passed through the aperture in the groin, since the wound in March, 1851, air has continued to be thrown off at longer or shorter intervals, until her death.

The following *post-mortem* appearances, drawn up by Dr. CALVIN ELLIS, who kindly performed the autopsy, are very interesting, as well for the great malformations of the organs, as for the length of time the patient survived—for a portion of this time enjoying tolerable health.

A layer of fat nearly an inch in thickness in abdominal parietes.

The peritoneal cavity contained several ounces of serum. Much fat in the mesentery, meso-colon, and in those parts of the cellular tissue where it is usually found.

Several loops of the small intestine were firmly adherent to each other, to the parietes in the neighbourhood of Poupart's ligament, to the dense cellular tissue filling the pelvis, and to the sigmoid flexure of the colon.

Appendix cœci adherent to the lower half of right kidney, the cæcum lying below; both natural in their appearance. The last three or four inches of the small intestines were much contracted.

At a point about three feet from the pylorus, the small intestine was attached to the parietes, near the anterior superior spinous process of the ilium, and here a free communication existed between the carious surface of the bone and the intestinal canal, the opening in the latter being as large as a ten-cent piece; the mucous membrane immediately surrounding it was perfectly healthy.

The ilium in the neighbourhood of the superior and inferior spinous processes was in a porous, diseased state, breaking down easily under the probe, and was bathed in a dirty, very offensive pus. The exact limits of the disease could not be ascertained, but it communicated with the external openings in the groin.

Liver quite firmly, though rather loosely, attached to the diaphragm, and to some of the surrounding organs, by means of old, delicate, membranous bands. Its surface had a somewhat rough, cicatrized look. The cut surface presented the dark spots noticed in partial venous congestion.

Pelvic organs imbedded in a mass of dense cellular and adipose tissue. A phlebolite in vicinity of uterus.

Spine.—On making an incision over the lumbar vertebræ, the probe struck

what appeared to be denuded, but firm, bone. The muscles, however, were not discoloured as in diseases of the kind, and the opening immediately above this point (the fourth lumbar vertebra) communicated by a longitudinal canal with the diseased portion of the ilium.

Fracture of the Face, Clavicle, and Ribs.—Dr. COTTING, of Roxbury, reported the case. Dr. C. reminded the members that some years since, he related to the Society a case of recovery from a fracture of the entire face from the skull. (See this *Journal* for January, 1850, p. 71.)

A few days since, a boy, aged four and a quarter years, was run over by a carriage containing five persons. The soft parts of the face were lacerated on the right side, from the median line of the nose just above the cartilages, across the cheek to the lower edge of the zygomatic process of the malar bone, and from that point upwards to the outer angle of the orbit, completely denuding the bones of that side. The right superior maxillary bone, together with the malar, was completely broken from its attachments, and driven downwards and backwards for at least half an inch. The force of the blow appeared to have been received immediately below the orbit, a portion of the floor of which was separated from the maxillary bone.

Through the nose and the palate the separation took place at the median line; the nasal process standing off from its articulation, and the teeth and palate projecting into the mouth for the distance before mentioned. The zygomatic arch was broken down, and the articulations of the superior maxillary bone with the cranium, &c. severed.

The fractured portion was wedged in so firmly that, for some time, its reduction seemed almost impracticable. But after removing, with the saw and other instruments, many of the engaged fragments, it was successfully accomplished by the aid of a powerful trepanning lever.

The lower jaw, which was broken through the body on both sides and at the symphysis, was adjusted, and the fragments kept in place by tying the teeth together firmly with saddlers' silk.

The wounds were then closed with interrupted sutures, and cold-water dressings applied.

Over the left mammary process of the temporal bone there was a severe abrasion, where the head came in contact with the ground. The left clavicle was broken near its scapular extremity. There were also seven fractures of the ribs—the second, third, and fourth of the left side, anteriorly, near the junction with the cartilages, the fourth and fifth posteriorly; on the right side, the fourth, anteriorly and posteriorly. There was much effusion of blood into the neck and about the upper portion of the chest.

The boy was a patient of Dr. B. Mann, who rendered efficient assistance during the adjustment of the fractures.

Notwithstanding these severe injuries, the lad recovered his consciousness a few hours after the accident, and on the day following, sat up in bed, recognizing and speaking to some of his mates. His respiration, embarrassed from the first by the fractures of the ribs, grew more and more difficult. It was soon quite evident that his chief danger was from this source. Towards night of the second day he became more distressed. The respiration was partial, irregular, and painful. He remained conscious till about two or three hours before his death, which took place thirty hours after the injury.

The case is interesting in connection with the other, showing how great an injury the face and head may receive, without apparently involving the life of the sufferer.

[*Note.*—The Secretary takes this opportunity to correct an error in the report of the case of hydrophobia related by Dr. Cabot (*Extracts from the Records*, vol. ii. p. 47, *Am. Journ. Med. Sci.* April, 1854, p. 359). Dr. C. E. Ware is stated to have “inquired if there were any well-authenticated instances of recovery from the bite of a rabid animal?” Dr. Ware’s question was: if there were such instances of recovery from *declared and positive hydrophobia*.]

ART. VII.—*Case of Paraplegia, caused by Concussion of the Spine. With Remarks.* By CHARLES A. LEE, M. D.

ACCIDENTS to the spinal cord may be considered, in the light of physiological experiments, throwing much light on its office and functions. Many such have been recorded. The case I am about to detail, has sufficient points of interest, perhaps, to render it worthy of permanent record.

In the spring of 1853, I was called to see Mr. Pope, of South Sodus, N. Y., a patient of Dr. Lewis Graves, from whom I learned the following particulars: Age 57, habits intemperate, health not very good for a short time previous to the injury. On the 28th of August, 1848, he fell from his wagon, striking his head and shoulders on the ground, the whole weight coming upon his head, which was thrown backward and under the body. On raising him up, he was found to be helpless, but wholly conscious. Dr. Graves found him, two hours afterwards, sitting upon the floor, a friend supporting him in an erect position. The arms and lower extremities were found wholly paralyzed, sensation and motion entirely destroyed; the knees were inclined to fall together, and were not easily separated; the hands were flexed upon the wrist; the thumbs and fingers turned in upon the palms of the hands; though he was unable to raise his hands to his head, he had some power to raise the arm and shoulders. On examination, there was found to be a partial dislocation of the sixth cervical vertebra, it being thrown partly to the left of the spinal column. The patient complained of tenderness on pressing over the parts injured. The urine had to be drawn off by a catheter for several days, after which the urine and feces passed involuntarily, though he was conscious when it was about to happen. The quantity of urine secreted was about the same as in health. The bowels were kept open for several weeks by catheters. The pulse was about fifty in a minute, and quite feeble; appetite generally good, and the food digested well for about four years. Since that time, the appetite has been poor, and digestion considerably impaired. Vomiting would often come on soon after eating; the feet and legs became anasarcaous, and abdominal dropsy was also present. This condition existed for several years. There was sloughing of the cellular tissue of the nates and posterior part of the thighs, caused by pressure from sitting on a hard chair. About six months after the injury, he fell into the hands of a Thomsonian, and was treated by lobelia, emetics, and cathartics, during which, inflammation of the scrotum and penis took place, resulting in extensive sloughing of the parts. Dr. Graves, on being again called in, administered stimulants and tonics, under the influence of which the recuperative powers again rallied, and in the course

of two months the parts were healed. Since that time (1849), sores have occasionally appeared on those parts subjected to pressure, as from sitting or lying. The sense of feeling returned in some degree, about ten months after the injury, and gradually improved, while the power of motion was not improved. The treatment consisted chiefly in supporting the general strength by tonics, and suitable diet.

April 9, 1854. Present condition of patient: Sensation has returned, but the power of motion remains the same, being totally lost. The limbs are emaciated; both the flexor and extensor muscles of the hand and forearm are contracted, and have been permanently so since the injury. The surface is cold; pulse feeble, and ninety in a minute. Copious perspiration is excited by the least exertion. The patient is quite weak and feeble; is unable to sit up much; appetite poor; quantity of urine secreted unusually large, and discharged involuntarily; much pain in the back and limbs; is very restless, and sleeps but little. The bowels are moved once in forty-eight hours. Can move the shoulders and body by using great exertion. Several sores on the thighs and hips difficult to heal. Slight cough; tongue thickly furred and flabby; digestion very poor, and the power of the system gradually failing. The mind has remained all the time unimpaired, though occasionally feverish and irritable.

The following letter from Dr. Graves will complete the history of the case:—

“DEAR SIR: I take the liberty of giving you a statement of the case of Mr. P., since my communication of April 9, last. The patient was then feeble, and the powers of life seemed failing pretty rapidly. He continued to fail from that time. Food and drink were ejected from the stomach without much effort, and soon after being swallowed. He complained of no nausea or sickness at the stomach, nor of any distress; still, food and drinks were not retained for about three weeks previous to his death. He was very much emaciated. Sloughing of the thighs had taken place, also the inside of the knees, by the contraction of the muscles, thus bringing them closely and permanently in contact. There was also involuntary twitching of the muscles of the legs, causing considerable motion and pain. The patient was very restless, requiring the attendants to turn and change his position frequently. The urine and feces were discharged involuntarily, as usual; respiration was short; pulseless at the wrist for several days before dissolution. The carotids beat feebly. The surface cold, and covered with a clammy sweat. Mind continued clear and undisturbed until death, which occurred on the 8th of May.

“*Autopsy.*—In the presence of Drs. Pearce, Peck, Mann, Trachout, and Reynolds, I made a *post-mortem* examination of the parts supposed to have been injured on the 8th. I carefully removed the five cervical and first dorsal vertebræ, and then laid them open longitudinally, so as to expose the spinal cord. The spinous process of the fourth, fifth, and sixth vertebræ were slightly depressed; the spinal cord was found to be firm and healthy from the second to the fifth vertebræ; within this last there was much softening and depression, the cord having the appearance of having been *bruised or cut off*. Below this point the coil was softened, and evidently shrunken or atrophied, with evidences of inflammation and engorgement of the bloodvessels. The conclusion, from all the appearances, was, that, at the time of the accident, a partial dislocation of the fourth, fifth, and sixth vertebræ had taken place, with depression of the spinous process, causing a *fracture and entire separation of the cord*, and giving rise to all the subsequent phenomena. I send you the specimen to examine for yourself, after which I would like to hear your views in regard to the case, and particularly with regard to the question how have the lower extremities been supplied with vitality, since the injury?

Truly yours, &c., LEWIS GRAVES, M. D.

— SOUTH SODUS, May 13, 1854.”

Remarks.—On examining the specimen, I find that there is no evidence of there having been any *fracture* of the body of the vertebræ, although the spinous process of the second and third cervical appeared to be *carious*. On opening the thecæ of the cord, which was greatly atrophied, the spinal marrow was found softened and disorganized, while the dura and pia mater were slightly thickened, vascular, and closely adherent to the walls of the vertebral canal. Whether the spinal cord was entirely destroyed within the fifth or sixth vertebra, as stated by Dr. Graves, I could not certainly determine, as it was difficult to ascertain the extent of the degeneration of the nervous into fatty matter, though it was extensive.

Such a condition would be hardly consistent with the existence of sensation, which we have seen was restored, to some extent, during the latter years of life.

The case is an interesting one on many accounts; ramollissement and atrophy of the cord existed through the whole extent of the cervical vertebræ, while the caliber of the vertebral canal was in no point so much diminished as to interfere with the functions of the cord. Was it, originally, a case of concussion only, resulting in congestion and inflammation, and subsequent softening, or was it a case of fracture, the marks of which had disappeared? The latter can hardly be supposed possible; while there are cases on record which go to support the former conjecture. Abercrombie relates a case, where a man received a severe shock, by falling some ten feet and striking his back upon a pile of stones. *He was immediately paralyzed in the lower extremities*, and the urine and feces were passed involuntarily. The power of sensation remained, but at the end of a month, when admitted into the hospital, there remained complete loss of motion, and all the muscles of the affected parts were atrophied, and in a state of great flaccidity. There was deep-seated pain on pressure, in the region of the third, fourth, and fifth dorsal vertebræ; evacuations involuntary, breathing slow, appetite moderate, digestion good, mind quite entire, and pulse and other functions natural. He died about five weeks after the accident; trismus, convulsions, and tetanus having come on forty-eight hours previous to death. *Autopsy* disclosed no injury to any of the bones of the spine. The pia mater of the cord was highly vascular, especially at the upper part of the dorsal region; and there was extensive ramollissement of the body of the cord, chiefly in its anterior columns, and to such a degree, that in some places they were entirely diffuent. This extended to the upper part of the cord, and affected the corpora pyramidalia.

In the case, whose history has been given by Dr. Graves, it is, of course, impossible to determine how long the disorganization of the cord had existed, although there is good reason to believe that it commenced soon after the injury was received. The patient survived for nearly six years, after such a degree of injury, as to prevent, in a great measure, the influence of volition upon the lower extremities, or the transmission of sensation from them to the brain; and the usual phenomena were manifested, which are generally, if not

universally, present under such circumstances, viz: sloughing, slower repair and reproduction of parts, owing to impaired nutrition, involuntary twitchings, and lessened capacity of maintaining itself against the influence of external forces, involuntary twistings, &c. In this case also was strikingly exhibited what we so often find in atrophy and ramollissement of the brain, the breaking up of the nerve-fibres, and the production of abundant granule-cells or masses, and free-floating granules, similar to those commonly found in the granular or fatty degeneration of various cells of both normal and morbid origin, and called by Paget, "liquefactive degeneration."

Since the above was written, Dr. Graves, in reply to some queries addressed to him, has communicated the following particulars:—

"1. The portion of the cord, where it seemed to be bruised or cut, was between the fifth and sixth cervical vertebræ. When first examined, it was very distinctly felt by passing the finger from above downwards on the cord. There was a feeling, at the point mentioned, as though the cord had been cut off, and below this point the cord was found softened and diminished in size.

"2. His urine was ammoniacal and turbid.

"3. His bowels did not move involuntarily at all times. Cathartics acted kindly when given. The discharges were thin and watery.

"4. His mental faculties remained clear to the last, as in health. He had no tetanic spasms. There was pain on pressure over the injured part of the spine.

"5. His speech was slightly affected; voice weak, and had to exert himself to speak loud. There were but slight motions of the limbs, except involuntary twitchings.

"6. He had more power over the upper than the lower extremities.

"7. There was occasional vomiting, and at last both food and drink were rejected as soon as swallowed.

"8. At times he could not sleep, and two or three weeks before his death he became very restless, and slept but little; position had to be often changed.

"9. The sloughing upon the thighs was about four inches in diameter, and on the inside of the knees, about the size of a dollar, caused by their being drawn together; also a large spot on the back, where it came in contact with the chair-back.

"10. Medicines did not affect him as much as when in health; larger doses were required.

"11. There were no prominent contractions of the toes or of the fingers."

The above case requires no extended comments, as a careful examination detects no luxation or fracture—no diminution of the spinal canal; it furnishes an interesting example of lesion of function from violent concussion of the spinal cord; or, it is possible that, from the violent twisting and bending of the cervical vertebræ, the cord may have sustained serious injury, independent of luxation or fracture. It is generally supposed that, in cases of permanent paralysis of the inferior extremities, it must arise from one of the latter; this case, however, goes to prove that it may occur independent of such injury.

If we are to believe, as Dr. Graves states, that the spinal cord was cut and twisted off between the fifth and sixth cervical vertebræ, then we must con-

clude, as maintained by Todd and Bowman, that "the spinal cord shares, in some degree, in the functions of sensation and voluntary motion." These writers remark that "the recent discovery of the *amphioxus lanceolatus*, a small fish found in the Archipelago, makes it highly probable that voluntary motion and sensation may exist where there is a well-developed spinal cord, the anterior extremity of which tapers to a fine point, and is far from exhibiting the ordinary characteristics even of a brain so inferior in organization as that of fishes."¹

Here there was a partial restoration of sensation and motion, with such a physical condition of the cord as would seem entirely inconsistent with the existence of such functions, viz: ramollissement, atrophy, and degeneration; and, according to Dr. Graves, an entire twisting off, or disappearance of the nervous matter of the cord, between the fifth and sixth vertebræ.

ART. VIII.—*Operation for Laceration of the Perineum.* By F. M. ROBERTSON, M. D., Lecturer on Obstetrics in the Charleston Summer Medical Institute. [With a wood-cut.]

It is not my design to enter into a discussion of the manner in which this accident may occur, or to compare the merits of the various operative procedures proposed and advocated by different surgeons and obstetricians for the purpose of relief. In alluding to the slight reference to the accident by various writers on obstetrics, Fahnestock remarks:—

"This silence on so important a subject can only be ascribed to the general impression, that the accident is one of very rare occurrence. It may be, in the practice of judicious practitioners, for Dr. Dewees informed the writer, a few years since, that he had to contend with but one case, happening to himself, in his extensive practice, which he attributed to the patient attention he always bestowed on supporting the perineum during the expulsion of the child through the os externum; yet, such is the nature of things that, in some cases, with the best management, laceration of the perineum is unavoidable."—*Am. Journ. Med. Sciences*, N. S. vol. i. p. 99.

The author just quoted states that only one case of laceration occurred, under his management, in a practice of eighteen years. In a practice extending over a period—from 1829 to 1854—of twenty-five years, the accident has never occurred in any case, of either natural or instrumental labour, under the management of the writer.

During the past summer I was consulted in relation to a lady, aged about twenty-four years, who, I was informed, had suffered from this melancholy accident, while in labour with her first child, about a year since. She was attended by a midwife, and the occurrence was attributed to some mismanage-

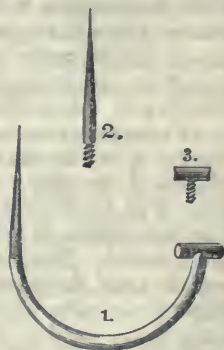
¹ The Physiological Anatomy and Physiology of Man, Am. ed. p. 276.

ment on her part. She came to Charleston in December last, and placed herself under my care for the purpose of being operated on.

Upon examination, I found the laceration to extend from the posterior commissure of the vulva to the anus, implicating, to a considerable extent, the external sphincter; but neither the internal sphincter nor the rectovaginal septum. Nature had so far repaired the injury to the external sphincter, that, in conjunction with the internal, control over the fecal evacuations was preserved. The septum between the lower portion of the rectum and vagina was so thin that, by the advice of a physician whom she consulted, she daily used a cathartic calculated to produce liquid evacuations from the bowels, fearing that the discharge of accumulated hardened feces might complete its rupture. The lower extremity of the posterior column of the vagina projected very much at the deep-seated margin of the laceration. The wound had healed irregularly on each side, and with considerable puckering and induration. The uterus had a constant tendency to prolapsus; the use of a supporter and pad had the effect of enlarging the external opening and increasing the difficulty. Altogether, the condition of the patient was one of great bodily suffering and extreme mental anguish; and she was anxious to undergo any operative procedure that would afford the possibility of relief. An early day was fixed for the operation.

After a detailed examination of the various modes proposed for the relief of this distressing condition, I determined to adopt that recommended by Bushe, and, without discussing the comparative merits of each method, I shall merely quote the following words from the work of this author.¹

“To obviate the inconvenience which I have ascribed to the interrupted suture, I have devised a pin, which is represented in Plate VIII., Fig. 4. [See accompanying figures.] This instrument is as thick as that used for harelip, and consists of three parts. The first, which is made of silver, is from one and a half to two inches long, curved as represented in the plate, terminating at one end in a female screw, and at the other in a transverse shoulder, about a quarter of an inch long. The second is a triangular steel pin, exactly resembling that used for harelip, and screws into the extremity of the first portion. The third is made of silver, and resembles the transverse shoulder of the first portion, with this exception, that a small male screw passes vertically from its centre, so that it may be fixed into the first portion when the second is removed. This instrument is to be used in the following manner: the first and second portions being united, provided the tumefaction has nearly subsided, and granulations are formed”—[he is speaking of recent injuries]—“the patient should be brought to the edge of the bed, her hips elevated, and her knees approximated and carried towards the chin. The parts being now cleansed, the needle ought to be dipped in oil and inserted into the left side of the perineum, a line more than half the breadth of its curve from the edge of the wound, and immediately above the verge of the anus. When it has passed vertically for a distance equal to two-thirds of the depth of its curve, its point should be projected transversely, so as to cross the bottom of the wound, and then carried outward through the other side of the perineum. This stage of the operation will be greatly facilitated, firstly, by pressing out the left labium during the transmission of the needle



¹ A Treatise on the Malformations, Injuries, and Diseases of the Rectum and Anus. Illustrated with plates. By George Bushe, M. D. &c. New York, 1837.

through the left portion of the perineum and the base of the wound; and, secondly, by steadying the right side of the perineum, with the extremity of the thumb placed immediately without the point through which we desire the needle may pass. When the puncture has been completed, the steel pin should be unscrewed and the third portion fixed in its stead. If it is thought advisable to insert a smaller pin higher up, it may be done, and then a thread should be twisted over their extremities, as in harelip. It may be prudent to place a light bolster of lint beneath the twisted ligature. This method of operating was first carried into effect in the almshouse of this city (New York) by my friend, Dr. Stevenson, who, not only on this, but on other occasions, afforded me opportunities of testing chirurgical innovations."—p. 82.

Having ascertained that the pins, as described above, could be constructed by Mr. Steinhardt, an excellent instrument-maker of this city, the 27th of December, 1853, at 12 M., was fixed on for the operation. The patient was directed to be kept upon a low diet for some days; the night previously the bowels to be freely opened by a cathartic, and some hours before the operation the rectum to be cleared by an enema. I regret that I was deprived of the valuable use of chloroform in this case. From the earnest solicitations of the patient and her friends—amounting, I may say, to a positive demand—I was compelled to operate with no other assistants than two ladies; and as my invariable rule, in surgical operations, is never to use chloroform unless its administration be confided to a skilful physician, I was compelled to dispense with its use. To the observance of this precaution, I attribute the fact that no untoward occurrence has ever taken place in my practice from the administration of anæsthetic agents.

The patient was placed across a bed, of such a height as to allow me to sit in a common chair in the proper position, fronting a strong light, with the hips drawn well over the edge of the bed, and the thighs and legs supported by the assistants, as in the operation for lithotomy. I commenced a short distance above the termination of the cicatrix on the left labium, and dissected, with the scalpel and forceps, a thin layer from without inwards a little beyond the mucous membrane, down to the bottom of the fissure, without detaching it. A similar dissection was made on the right side. The two layers were then seized together with the forceps, and removed, by a pair of scissors, from the anus to the projecting lower extremity of the posterior columna of the vagina. This procedure freshened every part of the laceration. Two small patulous processes—remains of the old wound—hanging from each labium on a level with the superior margin of the cicatrices, were also removed. The pins were then introduced as directed by Bushe; one near the anus, the other near the upper termination of dissection. The coaptation of the parts was perfect. No bolster of lint or compress was used. There was no eversion of the edges of the wound, as the crossing of the threads from one pin to the other kept them pressed down, and in perfect apposition. A catheter was introduced into the bladder and confined by tapes, her knees tied together, and, with the thighs flexed upon the pelvis, she was placed on her side, with directions, when she became fatigued, to be turned carefully to the other side, without separating the knees. The bowels were kept in a quiescent state by small doses of tinct. opii, repeated once or twice a day. The parts were daily washed, and a simple pledget of lint, saturated with a solution of the chloride of soda, lightly applied. She was confined to a fluid diet.

On the ninth day the union of the wound was complete, and the pins were removed. There was slight suppuration from one of the orifices of the upper pin, but, under the use of the solution of chloride of soda, this soon ceased.

On the sixteenth day the catheter was dispensed with, and, as the cicatrix appeared firm, an enema was administered. It acted freely without any injury to the newly-formed perineum. Scarcely any traces of the operation could be observed; the linear cicatrix, from the anus to the new commissure of the vulva, resembled the raphé. The four punctures of the pins appeared like small white dots on each side of the linear cicatrix.

On the 18th January, 1854, the patient walked about the room without the slightest inconvenience, and the next day met me in the parlour.

CHARLESTON, S. C., April, 1854.

ART. IX.—*Cases of Ovariectomy.* By ALEXANDER DUNLAP, M. D., of Ripley, Ohio.

CASE I.—Mrs. B., of Bracken County, Kentucky, aged 37 years, dark hair and eyes, bilious temperament, and the mother of five children, in September, 1852, came to consult me in reference to an enlargement of her abdomen, which had been supposed to be ascites. A careful examination, and the history of the case, showed it to be ovarian dropsy. I stated to her that medicine could not cure her, that tapping would only afford temporary relief, and that the extirpation of the tumour was her only chance for a cure; at the same time I told her the dangers of the operation, and that her chances of recovery would only be about three out of five, and left her then to decide that question for herself. She returned home. Her attending physician, still adhering to his former opinion—that she had ascites—tapped her. The enlargement of the abdomen was entirely removed by the operation, but in a month it had regained its former size, and she was again tapped. After this operation a tumour was discovered remaining in the abdomen. She was subsequently tapped twice. The amount of fluid drawn off by tapping I could not ascertain. In March, 1853, she sent for me to perform the operation for the removal of the tumour. I found her very much reduced in flesh; the abdomen enormously distended; fluctuation distinct in every part, with the exception of the right iliac region. She was most of the time confined to her bed, yet free, as I judged, from any organic disease, except that of the ovary. I determined to operate, which I did, after preparing her system, on the 24th of March, 1853, assisted by Dr. J. S. Bradford, of Augusta, Ky., in the presence of a number of medical gentlemen and students. The patient having been placed upon a suitable table, with her shoulders elevated, and her feet resting upon a chair at the end of the table, and brought under the influence of chloroform by Dr. Woodward, of Ripley, O., I commenced by an incision in the median line, through the integument and cellular tissue, from the umbilicus to a point near the symphysis pubis, by one stroke of the knife; then, by a careful dissection at one point of the incision, I opened the peritoneal sac, which I found free from adhesions at that point. By the use of my finger as a director, an opening was readily made into the peritoneum, corresponding with the one in the integument. The opening thus made was nearly twelve inches long. The adhesions, which were but slight, were then broken up by the hand (in which Dr. Bradford assisted me), excepting at the points where paracentesis had been performed, which I separated by the knife. The

tumour was then opened and emptied of its contents without allowing any of the fluid to escape into the cavity of the abdomen. Having raised the sac, together with a fleshy tumour about the size of a child's head at birth, from the cavity of the abdomen, it was supported by Dr. Bradford, while I proceeded to tie the pedicle by passing a needle, armed with a double silken ligature, through it, and tying one around each half. I then separated the pedicle, cut one end of each ligature close to the knot, and brought the other out and left it in the lower end of the incision. The cavity of the abdomen contained about a pint of fluid, and a transparent clot or coagulum floating loose without any attachment, which I suppose would weigh about eight ounces; this was removed, and the fluid carefully sponged from the abdomen. The lips of the wound were then brought together by five interrupted sutures, the intervening spaces supported by adhesive straps; lint was then applied, with a compress and bandage, and the patient placed in bed. The time occupied in the operation and dressing was twenty-eight minutes. On applying cold water to her face, she immediately aroused as from a sweet slumber, perfectly unconscious of what had been done. The pulse was 80, and had undergone no perceptible change since the commencement of the operation.

7 o'clock P. M.—Two hours after the operation, patient complained of pain in the lower part of the abdomen; pulse 88, and increasing in volume. Gave a pill 3 grs. of ext. hyos., and drew off twelve ounces of straw-coloured urine. 2½ o'clock P. M.—Pulse 88; skin natural temperature; drew off six ounces of urine.

The following notes were taken by my office students, E. R. Bell, and J. P. McClanahan, who stayed with and nursed the patient throughout:—

3 o'clock P. M.—Patient complains of pain in the back and bowels. Repeated the pill of ext. hyos. 5 o'clock P. M.—Pulse 84; uneasy sensation at the stomach; pain in the bowels; feet cold. Hot applications to the feet, and repeated the ext. of hyos. 6 o'clock P. M.—Vomited a quantity of fluid of a dark green colour. 7 o'clock P. M.—Vomited again; pulse 100. 9 o'clock P. M.—Patient restless; complains of thirst; skin natural temperature; pulse 98, and compressible. Gave a pill ext. of hyos., and toast-water. Drew off seven ounces of straw-coloured urine.

March 25. 5 o'clock A. M.—Patient has rested well; pulse 84; temperature of skin natural. Drew off sixteen ounces of urine. 8 o'clock A. M.—Vomited a worm. 11 o'clock A. M.—Patient restless; has been vomiting; pulse 84; drew off five ounces of highly coloured urine; gave a teaspoonful of camph. tinc. opii. 4 o'clock P. M.—Has rested well; pulse 84; drew off four ounces of highly coloured urine.

26th. 2 o'clock A. M.—Rested well, until within the last two hours; complains of severe griping pains in the bowels; has been vomiting; gave a teaspoonful of camph. tinc. of opii; she vomited soon after, throwing up a large worm. 10 o'clock A. M.—Complains of distension of the bladder; pulse 84; drew off six ounces of urine. 2 o'clock P. M.—Found her in fine spirits; pulse 84, full and soft; tongue clean; appetite good; has no swelling or tenderness of the abdomen. Dressed the wound; found it uniting by the first intention; removed two of the sutures, and ordered her clothes to be changed. Diet—toast and rice-water. Ordered an enema to be given to-night to move her bowels. 8 o'clock P. M.—Resting well; drew off eight ounces of urine, and gave an enema. At 9 o'clock P. M. repeated enema, which produced a slight operation from the bowels. 10 o'clock, P. M.—Complains of pain in

the bowels; pulse 104, and full; skin warm and dry; some thirst. Gave a pill composed of blue mass, rhei, and ext. of hyos:

27th. 6 o'clock A. M.—Has rested moderately well, and has taken three of the blue mass, rhei, and extract of hyos. pills during the night; pulse 90; no evacuation from the bowels. At 10 o'clock A. M. gave two ounces of Rochelle salts. 2 o'clock P. M.—Repeated the salts; at 8 P. M. gave an enema. 10 o'clock P. M.—Had a free evacuation from the bowels; passed her urine without the use of the catheter; pulse 84.

28th. 6 o'clock A. M.—Rested well all night; pulse 84.

29th. 8 o'clock A. M.—Resting well; pulse 84; tongue clean; appetite good; has had a good night's rest. Dressed the wound; found it united by the first intention; removed the remaining sutures. It is discharging healthy pus from around the ligatures. During the night, the patient turned upon her side, producing no unpleasant feelings.

It is unnecessary to detail her symptoms farther. From this time her recovery was rapid, and uninterrupted by any unpleasant symptoms. On the thirteenth day after the operation she walked across the room with assistance. I received the following note from Dr. Ferree, of Rock Spring, Ky., to whom I am greatly indebted for his kind attendance, with Bell and McClanahan, on my patient during my absence.

April 14, 1853.

I visited Mrs. B. this morning; found her in an improved condition, strong, and well enough to sit up half her time, and to walk into the adjoining room without assistance. I removed the ligatures this morning without the least difficulty or pain. I told her my attendance would no longer be necessary.

J. V. FERREE.

It was the left ovary that was removed. The weight of the tumour was thirty-seven pounds. Not more than four ounces of blood were lost in the operation.

CASE II.—Mrs. F., of Clermont County, Ohio, aged 46 years; menstruation had ceased in her fortieth year. Had received an injury in her left side seventeen years ago, and had felt the effects of it ever since. About three years since, she discovered a small tumour, about the size of her fist, floating loose in the lower part of the abdomen, which had gradually enlarged. At the time she visited me, April, 1853, I found it filling the whole cavity of the abdomen, and greatly distending it. Fluctuation was distinct in every part. After examining the case, and hearing her history of it, I told her the disease was ovarian dropsy, and that her only chance for a cure was an operation for its removal. I stated to her the dangers she would have to undergo, and gave her Dr. Atlee's tables of ovariectomy, from which to make up her mind in reference to an operation. Her general health up to this time had been good, and although she was greatly reduced in flesh, I told her I could see nothing in her case that would deter me from operating, if she wished it. I gave her directions for medicine and diet, to prepare her system in case she determined on an operation. After returning home and consulting her friends, she sent me a note, stating that she wished the operation to be performed on the 17th of May, 1853. Accordingly, on that day, assisted by Dr. J. T. Bradford, of Augusta, Ky., and in the presence of a number of medical gentlemen and students, I performed the operation in the same manner as in the case of Mrs. B. There was but one slight adhesion to the omentum, which I divided by the knife. The patient did not come under

the influence of chloroform well, and had to be held during the operation, which lasted seventeen minutes, including the dressing; but she was entirely unconscious of pain. Pulse, on placing her in bed, 84, and had undergone no change during the operation. Complained of being sick at the stomach, which soon passed off, and which I supposed to be the effect of the chloroform. Six hours after the operation she complained of severe pain in her stomach, which readily yielded to $\frac{1}{4}$ gr. sulph. morph. Her urine had to be drawn off by the catheter for three days. Bowels were moved on the third day by medicine. Catamenia made their appearance the second day, and continued for three days. The sutures were removed, two on the third day and two on the fourth. The wound healed by the first intention; she was able to be on her feet the fourteenth day after the operation. No unpleasant symptoms occurred during the progress of the case. The ligatures came away on the twenty-seventh day after the operation. The tumour consisted of one large sac, to the inner surface of which were attached the small ones, the largest of which would probably contain one-third of a pint. The walls of the sac were thin, and of a very even thickness throughout. The sac and its contents weighed thirty-one pounds. It was the left ovary that was removed. The length of the incision made was ten inches; very little blood was lost during the operation. She has since enjoyed uninterrupted good health, and is now more fleshy than ever before in her life.

In the *Louisville Journal* of September, 1853, may be found a short account of an operation in which I assisted Dr. J. T. Bradford to perform, on the 17th day of June, 1853. Tumour weighed forty-one and a quarter pounds. Patient recovered without an unpleasant symptom. This makes the fifth case of ovariectomy in which I have been concerned, four on my own responsibility, and this last one in connection with Dr. J. T. Bradford. The last four have perfectly recovered.

Mrs. H., of Brown County, Ohio (whose case was reported in the *Western Lancet* of June, 1851), has since given birth to a healthy male child.

The one operated on in 1843 (this case was never reported, being refused publication by a western journal), died on the seventeenth day after the operation from diabetes. The wound had nearly healed by the first intention. No tenderness existed over the abdomen. No *post-mortem* examination was had in the case. The tumour was cystiform, and weighed forty-five pounds. At that time I was opposed to the operation, and only performed it at the urgent solicitation of the patient and her friends. The case, although unsuccessful, satisfied my mind that the operation was a justifiable one, and that, had it been performed earlier, would have terminated favourably. The foregoing cases were all cystiform. The pedicles were small. From the history of the cases, I judged no great amount of peritoneal inflammation had previously existed, giving me ground to believe that the adhesions to the surrounding parts were not extensive or firm. This I consider an important point in the diagnosis to justify an operation. During the past year, Dr. Bradford and I have been applied to for relief in seven other cases of diseased ovary and fibrous tumours of the uterus; in most of these the patients have been willing to submit to an operation, and some of them were even anxious and urgent to have it performed. But we have refused to operate for various reasons—some on account of obscurity of the diagnosis; others from the inflamed state of the peritoneum, and the fibrous character of the tumours.

In answer to the question, Is ovariectomy justifiable? I would unhesitatingly say yes, where the case is clearly made out to be cystiform in its character, the tumour free, or but slightly adhering to surrounding parts, and

the general health of the patient such as would justify any other major operation. The difficulty of diagnosing those diseases is, I consider, the great obstacle to recommending the operation, and not the opening of the peritoneum. But because we are not able to make a clear diagnosis in every case, are we to abandon the whole class of patients to become the prey of quackery? I believe it to be considered on all sides that medication has not, as yet, been able to reach those cases.

Two cases of spontaneous rupture of the sac have come under my notice; in one, the patient was under preparatory treatment for an operation. While in the act of stooping she felt something give way, and immediately experienced the sensation, as she described it, as if boiling water had been poured into her bowels. She died in two hours after the accident occurred. The *post-mortem* examination showed a rupture in a small sac, which would contain about a pint. The whole tumour, which was cystiform in character, weighed thirty-two pounds.

The other case was more fortunate. A tumour had been growing gradually in the lower part of the abdomen until it attained the size of a child's head one year of age. One night she was awakened by a burning sensation in her bowels; on placing her hand upon her abdomen she found that the tumour had disappeared. In the morning, as soon as she arose, she had occasion to evacuate her bowels, when she discharged about a gallon of straw-coloured fluid. In a short time, however, the tumour reappeared. The discharge, the second time, was not so rapid as at first, and at the same time that she had the watery evacuation from the bowels, she also had large discharges of water from the bladder, in which some traces of blood could be discovered. Once or twice in the course of two months she could feel a return of the tumour, but which disappeared upon a discharge of fluid from the bladder and bowels. Since that time, now nearly four years, there has been no return of the tumour; but she has scarcely passed a day without passing more or less wind from her bladder. Within the last six months I have frequently examined the evacuations from the bladder; I found them to possess the same appearance and odour as that passed from her bowels. This, however, is only when she is labouring under diarrhoea. Her life is one of great suffering, and opiates are her only relief.

ART. X.—*Autoplastic Operation for the Removal of the Deformity produced by a Burn.* By A. CLARKSON SMITH, M.D., of Columbia, Lancaster County, Pennsylvania.

IN the autumn of 1853, I was consulted by John Munroe, æt. 20, in reference to a deformity of the neck, produced by a burn he had received a year previously.

On examination, I found the "tissue of the cicatrix" unattached, for the most part, to the superficial fascia, being movable, and easily raised from the parts beneath. The tissue was thick, and composed of dense fibres crossing each other, and interlacing, covered by a delicate cuticle. Small cavities, formed by the crossing of the fibres, existed here and there over its surface, at the bottom of which slight adhesion was found with the fascia beneath.

The cicatrix extended from the inner edge of the mastoid muscle, of the right side, along the course of the lower maxilla to a point midway between the chin and angle of the left side of that bone, involving the integuments of the neck to a point half an inch below the thyroid cartilage, following a line drawn from this point to the acromial third of the clavicle. The parts seemed firm and rather pale, though somewhat sensitive. The appearance of vascularity was not such as to cause any apprehension of much hemorrhage. Altogether, the patient seemed in a condition in which an operation would be most likely to succeed; accordingly, I advised him to submit, stating, at the same time, the risks of a failure. After weighing the matter for a length of time, he concluded to have the operation performed, and, on the 2d of January, placed himself under my care for that purpose.

On the 4th inst., assisted by Dr. B. Rohrer, now of Germantown, Pa., and Dr. Mahon, of this place, I operated. The patient was placed on a table, with his head slightly inclined backwards. A mixture of chloroform and ether was exhibited until completely under its influence, and then, being firmly held by assistants, an incision was made, a line above the upper margin of the cicatrix, from the edge of the mastoid muscle of the right side to the termination of the cicatrix on the left. Getting under the "inodular" tissue, it was carefully dissected from the fascia, until the entire mass was completely removed. There was but little hemorrhage, and no vessel requiring the ligature was cut. A flap, sufficiently large to allow of considerable contraction, was taken from the breast (the head being still inclined backwards), and carefully applied by means of the interrupted suture and adhesive strips. The edges of the wound on the breast are approximated, as nearly as possible, by adhesive strips, and then covered with soft lint steeped in olive oil. The connection between the transplanted integument and its original location was preserved by a strip of integument an inch and a half in breadth. This was so loosely twisted as not to interfere with its circulation. The flap was covered with oiled silk, an opium pill given, and the patient left in charge of a nurse for the night.

January 5. Patient rested quietly during the night; complains of soreness in his extremities, produced, doubtless, by the struggles of yesterday, and a feeling of stiffness in his jaw. Pulse 88. Lymph thrown out abundantly around the edges of the wound. Ordered, pil. opii gr. j, ter in die, and milk diet.

6th. Passed a restless night; complains of a sense of fulness in the epigastrium; much pain in the wound on the breast; tongue coated; pulse 110; discharge of sanguineous fluid from the inferior portion of the flap. Removed some of the adhesive strips which had become loosened, and applied fresh ones; also redressed the breast, and in place of the oiled lint applied a slippery elm poultice. Ordered sennæ, mannæ, mag. sulph. āā ʒij; semen. fœniculi ʒss; aquæ fervent. Oss. M. ft. haust.

7th. Symptoms much improved; pulse 95; little or no pain in the wound on the breast; flap still adhering; cuticle vesicating. To prevent this, coated the entire flap with collodion.

10th. Removed the dressings from the neck and breast. Adhesion has taken place between the flap and surface beneath. The exclusion of the air by means of the collodion checked the vesication. A slight suppuration where the stitches were drawn through induced me to remove them. The parts were still supported by a few adhesive strips and a slight bandage passing around the neck and over the head.

From this time he rapidly recovered, and at the end of three weeks was able

to leave the house. So perfect has been the union of the parts that scarcely a vestige of the line of adhesion remains superiorly.

The connection between the flap and breast was severed at the expiration of three weeks, and the patient discharged.

Remarks.—Of the importance and utility of this class of operations there can be little doubt; and the profession owes much to the ingenuity of Prof. Mutter in establishing it in this country. By this means, unsightly disfigurements can be removed, and deformities, oftentimes so great as to interfere with the ordinary duties of life, remedied. Of course, there are many circumstances to be taken into consideration before making the attempt; and among these the most important are the *situation, extent, and depth* of the inodular tissue, and the condition of the patient. Reasoning on the established principle that perfect rest is necessary for the adhesive process, we should conclude that where this cannot be obtained we should not be justified in making the attempt; and yet, in the above case, the operation was entirely successful in a situation where perfect rest *could not* be obtained. The motion of the thyroid cartilage in swallowing, and constant throbbing of the carotid artery, increased by the irritation in its vicinity, precluded the possibility of obtaining this desideratum.

Where the cicatrix is extensive, it would probably be better to remove a portion at one time, inserting sound skin, and after adhesion has taken place, to finish it at a second operation. Where the fascia and muscles are involved, the operation becomes more formidable, and the chances of success are diminished; but, even in these cases, an attempt to relieve is oftentimes attended with the happiest results.

COLUMBIA, Pa., April, 1854.

ART. XI.—*Cure of Laceration of the Urethra.* By Dr. J. GAUTIER, M. D., of Tuskegee, Alabama.

JAMES HALL, aged 35 years; has light hair, blue eyes, fair complexion; a sound and vigorous constitution; weighs about one hundred and fifty pounds. In May, 1852, he was engaged as a deck-hand on board of a steamboat running from the city of Galveston to Brazoria, and while in the latter port, and engaged in discharging freight (at night), he attempted to cross over the hatch, and in doing so missed his footing, and fell astride of a square bar of iron extending across the hatch, upon which the doors rested. Laceration of the urethra, and great contusion of the perineum resulted. The injury occurred about 10 o'clock at night. I did not see Hall until the next morning, between 8 and 9 o'clock. He was then complaining of excruciating pain, with great distension of the bladder, and an inability to pass urine. By the forcible contraction of the bladder, occasionally a few drops of bloody urine would pass through the penis. On investigation, I at once concluded that there was laceration of the urethra. The only case of the kind I had ever

seen before, was in a little boy, treated in the Pennsylvania Hospital, under the care of Drs. Norris and Fox, in 1850. The experience furnished by the case, together with the valuable instruction given by the doctors above mentioned, rendered the treatment of this case, to my mind, positive and clear.

At once, I attempted to introduce a silver catheter into the bladder, but could only pass it up to the seat of laceration. After working with him an hour or two, and trying every size of gum-elastic catheter, I at last succeeded in introducing one of the latter, of very small size, into his bladder, and drew off by measurement twenty ounces of urine considerably coloured with blood. Having got a catheter beyond the point of laceration, I knew, from the facts of the case seen in the Pennsylvania Hospital, that it was wise to keep the instrument as long as possible in the urethra. On the second day, I had to remove the catheter; but I immediately introduced another of a larger size.

The perineum was kept constantly bathed in cold water, or a solution of sugar of lead. Hall had high fever and costive bowels, and saline purgatives were administered and an antiphlogistic treatment adopted. In a few days his fever subsided, and he was doing well. In six or eight days after his injury, I was able to introduce a large silver catheter into his bladder.

From the time of his injury, I have endeavoured to make it an established rule with Hall that he should never attempt to pass urine while the catheter was out of his bladder. On the morning of the fifteenth day of the injury, professional duty compelled me to be absent, and during my absence, Hall took the catheter from the urethra, and left it out for several hours. When he wanted to make water, he was unable to introduce the instrument. As he was suffering from distension of the bladder, another physician was sent for, and Hall was told to pass water *without* the catheter.

On my return home in the afternoon, I went immediately to see my patient. I found him in a condition vastly more dangerous than he had been upon the morning of his injury. The penis, scrotum, perineum, and cellular tissue about the pubis, were enormously distended from infiltration of urine. He had high fever, and his mind was considerably disturbed. Some of the bystanders supposed him to be dying. With considerable difficulty I again succeeded in introducing a catheter into the bladder. I then freely scarified the scrotum, to allow the infiltrated urine to escape. In a few days, mortification of the scrotum took place, and both testes were left entirely denuded of skin and cellular tissue, hanging only by the cords. The posterior and under part of the penis, and a portion of the perineum, were in a state of sphacelation. About two inches of the urethra was destroyed. From the destruction of the perineum, under part of the penis, and the urethra, at least two inches of the silver catheter could be seen. The bulb of the urethra was not injured, the destruction being anterior and posterior to the bulb. To add still another pang to Hall's misfortunes, one day, when asleep, flies deposited their ova on his testes and perineum. In the course of four or five days, at least fifty large maggots were taken from the perineum. My patient was now troubled with constant hectic fever, and greatly emaciated. In this condition, I had but little hope of his recovery. For ten or twelve days he lay feeble and prostrated almost unto death. I watched his case closely, removed all disturbing causes, and kept the catheter constantly in his bladder, removing it only to be washed. I gave him but little medicine, principally tonics. Rich, nutritious diet was allowed. The case was left as much in the hands of nature as possible.

About the latter part of June, Hall began to show evidences of recovery. His general health was gradually improving. Nature was throwing off the

putrid mass, and beginning to reproduce new tissue. Indeed, the rapidity with which the parts were restored to their normal condition was truly astonishing. In less than six weeks from the commencement of the formation of new tissue, the testes were enveloped in a new scrotum; the penis, urethra, and perineum were entirely restored.

By the latter part of August, Hall was well, and free from deformity. He could retain his water as well as he ever did, and void it *per vias naturales*, and with ease. He has not used a catheter since the 12th of August. When I saw Hall again, early in September, he was riding an unbroken mustang horse; and he assured me he was perfectly well, and had as much strength, and as perfect use of himself, as he had before the fall.

TUSKEGEE, ALA., July 12, 1854.

[*Note by the Editor.*—In connection with the above interesting case, we will refer to some observations which we published in the number of this Journal for Feb. 1837, on accidents of this character, their nature and mode of treatment, illustrated by a large number of cases.]

ART. XII.—*Case of Epilepsy, treated by Ligation of the Common Carotid Artery.* By JOSEPH B. BROWN, Assistant Surgeon U. S. Army.

“June 2, 1848.—Miss Elizabeth W., æt. 22, has been subject to attacks of epilepsy since her 17th year. Some four years before this time, she first felt a strange sensation in her right forearm; it gradually passed above the elbow, and now appears at the intersection of the omohyoideus by the trapezius, and precedes the convulsions as a true *aura*, being described by the patient as a stream of warm fluid issuing from this spot. Menstruation was normally established before the disease became confirmed. The patient is now constantly afflicted with convulsions, which vary in character and intensity; those occurring in the night being of the most violent convulsive character; while those during the day are generally less aggravated. Sometimes a week or two intervenes without any attack; but she states that she has had as many as twenty-four of the fits in the twenty-four hours. Her face bears constant marks of the violent injuries she receives from falls against articles of furniture, when prostrated, as she often is, without warning, while engaged at her usual avocations, or walking across the floor. She is remarkably intelligent, and of excellent physical development.

“Her attending physician, Dr. Z. Pitcher, of Detroit, despairing of affording her any relief from the ordinary means of medical treatment, which have been properly and perseveringly tried, has consulted with me as to the propriety of attempting a cure by a surgical operation. I saw the patient to-day, and finding no sign of disease external to the brain, to which the convulsions could be referred, agreed with Dr. P. as to the propriety of attempting to relieve her by applying a ligature to the common carotid artery of one side. *It was decided not to use chloroform*, as generally hazardous in operations of importance about the head, and for such a one as this, and in such a case, as particularly imprudent.

"5th. Operated to-day, assisted by my brother and Dr. Alfred Brush, Dr. Pitcher and some other medical gentlemen being present. The vessel upon the right side was selected, and tied *above the omohyoideus*, a slight enlargement of the thyroid gland interfering with the artery below. *No chloroform was used*. Pulsation immediately ceased in the temporal and facial arteries of the side, upon tightening the ligature, but no perceptible change in the feelings of the patient was experienced."

The above is an extract from my note-book, kept at the time. The relief afforded by the operation was immediate. The ligature came away on the twentieth day, long before which time pulsation was re-established in the right temporal. Constant service upon a remote frontier has until recently prevented me from hearing the subsequent history of the case. Dr. Pitcher reports (July, 1853) that "the aura has never returned, neither have the grave and convulsive forms of the disease; but within the past year, when exercise is omitted, and any unpleasant mental disturbance takes place at the same time, very slight returns of the *petit mal* occur, but never to occasion loss of consciousness. The health of the patient is now perfect. Her temper, which had been rendered irritable by the disease of her nervous centre, is very much improved, and her appreciation of existence greatly enhanced."

For more than *three years* she was exempt from seizure of any kind.

The history and statistics of ligation of the carotid have been made the subject of a valuable essay by Dr. Norris, of Philadelphia.¹ His tables show that this operation, though several times performed for cases of epilepsy, has never been successful for the cure, but all have recovered from the effects of the operation. He says that "the idea of curing epilepsy by tying the carotid seems to have been founded on false principles." It is certainly difficult to reason upon this matter, when the pathology of epilepsy, the species designated as *centric epilepsy* especially, is so imperfectly understood. Should it depend upon a functional derangement of the relation between the arterial and venous circulation within the head (as probably in the above case), a measure which would so materially diminish the force and volume of the blood sent to the brain as ligation of the carotid, would seem, if not expressly indicated, at least to demand a trial. It is true, the difficulty of diagnosing cases in which this derangement operates must ever exist; but the above case certainly shows that the most aggravated forms of the disease may arise from some such cause, and that mechanically checking the volume and force of the arterial blood poured into the brain may restore the balance and effect a cure. The operation has never proved fatal, though uniformly, till the present case, unsuccessful as a cure; but with the experience of this *one*, productive of so much benefit, restoring the patient from a state of existence too horrible to endure, to the comfort of health, it seems to me that, in proper cases, we should not be justified to abandon it entirely.

Note.—This case has been recently reported by Dr. P. in the *Peninsular Medical Journal*, but the report is evidently drawn up from recollection, as it contains several inaccuracies.

RINGGOLD BARRACKS, TEXAS, Jan. 28, 1854.

¹ Am. Journ. Med. Sciences, July, 1847.

FISKE FUND PRIZE ESSAY.

ART. XIII.—“*Neuralgia; its History, Nature, and Treatment.*” By CHARLES W. PARSONS, M. D., of Providence, R. I.¹

THE writer cannot suppose that a detailed description of the symptoms or varieties of locality of neuralgia is expected in this Essay. It is believed that the objects for which this generous foundation was established, and is now administered, will be best consulted by brevity, and a strictly practical aim in the choice of topics. We shall, therefore, confine ourselves to the following subjects:—

- I. The General Topography of Neuralgia.
- II. Its Causes.
- III. Its “Nature,” or Pathology.
- IV. Its Treatment.

I. By *general topography* we mean an account of the kinds of nerves, or divisions of the nervous system in which neuralgia exists; and also some general statements as to the situations in the course of any nerves, which are its ordinary seats and centres of radiation. We do *not* mean to include a list of all the individual nerves that are sometimes attacked.

Neuralgia is best known as occurring in the cerebro-spinal nerves. Does it ever attack those of the ganglionic or sympathetic system? Experiment has proved that mechanical irritation of these nerves does not elicit signs of pain till after a time, when congestion or vascular excitement is produced, and then acute pain is caused by such irritation.² We might expect then that other irritating causes, such as poisons circulating in the blood, or some derangement of different functions, might produce an analogous sensitiveness in the nerves of the sympathetic system. And such is the fact. The analogy of many painful visceral affections to external neuralgia is as complete as the nature of the case admits; though the sensations cannot be described so definitely, because the nervous fibres have not so direct and continuous a relation

¹ The Trustees of the Fiske Fund, at the annual meeting of the Rhode Island Medical Society, held at Providence, June 7, 1854, announced that the premium of fifty dollars offered by them in 1853, for the best dissertation on *Neuralgia, its history and best mode of treatment*, had been awarded to the author of the dissertation bearing the motto,

“What is writ is writ,
Would it were worthier!”

And upon breaking the seal of the accompanying packet, they learned that the successful competitor was Charles W. Parsons, M. D., of Providence, R. I.

JOSEPH MAURAN, M. D.,
SYLVANUS CLAPP, M. D., } *Trustees.*
ARIEL BALLOU, M. D., }

S. AUG. ARNOLD, *Secretary.*

² Brachet, *Recherches Expérimentales sur les fonctions du Système Nerveux Ganglionnaire.* Paris, 1837.

with the sensorium as the cerebro-spinal nerves have. These affections, gastralgia, nephralgia, &c., are not necessarily or usually connected with any change of structure. In angina pectoris, one of this class, ossification of the coronary arteries, fatty degeneration of the heart, and other organic alterations have been found, but by no means uniformly; they are not, so far as we now know, essential parts of the disease. The attacks of these "visceral neuralgiæ" are generally marked by little constitutional disturbance, in proportion to their severity. They are paroxysmal, and often the fits arise without any obvious exciting cause. Though very obstinate, they sometimes subside suddenly, we know not why, and their long continuance does not always seriously affect the general health, except by the mere amount of suffering. They occasionally alternatè with external neuralgia. If the attacks of those which occupy glandular organs sometimes end in a sudden flow of secretion, the same is common also in the analogous affection of the fifth pair.¹ It is true that there is generally some functional derangement accompanying the pain, and, of course, the distinct character of these internal neuralgiæ, as pure *nerve-pains*, is not so perfectly marked, nor so uncomplicated, as in pains of the superficial nerves.

How far the acquired sensitiveness of ganglionic nerves depends on their connections with the cerebro-spinal system, is a question properly of physiology, which we will waive.

Among cerebro-spinal nerves, the most superficial ones are said to be the most common seats of well-marked neuralgia. But to this there are many exceptions. Nerves which plunge into the thickness of dense organs have their branches closely interwoven with the tissues around, so that the sensorium can only distinguish a diffused pain. Such affections as neuralgia of bone and of the testis (when the spermatic cord is not involved), are called neuralgie, because pain is altogether out of proportion to the slight and inconstant anatomical changes. But in the superficial nerves, pain can be tracked along the nervous trunks, so that a patient can study their anatomy on his own person. Facial, dorso-intercostal, and sciatic neuralgia are believed to be the most common forms.

But certain points appear to be the peculiar and favourite rallying points, or foci, of this disease. This part of its history has been particularly investigated by Valleix,² and his observations are among the most interesting additions recently made to our knowledge of this affection. The "focal points" of external neuralgia are generally in the following situations:—

1. The place of emergence of a nervous trunk, as at the supra-orbital and infra-orbital foramina, the crural arch and ischiatic notch, &c.

2. The points where a nervous branch comes out through the muscles, to ramify in the integuments. For example, where the posterior branch of the second cervical appears behind the mastoid process, or where those of the lower spinal nerves become superficial.

3. The points where the terminal branches of a nerve expand in the integuments, as at the anterior extremity of the intercostal nerves.

4. The points where the nervous trunks become superficial during their course, as where the ulnar nerve passes the inner condyle.

These points are peculiarly liable to suffer in neuralgia; they are, almost

¹ The secretions may be apparently *perverted* during the fits. A patient of Macculloch suffered from neuralgia in different superficial nerves, and at last from nephralgia, accompanied by diabetes mellitus. This "was strictly paroxysmal, or the morbid secretion of sugar commenced with the fit, and entirely disappeared in the interval."

² *Traité des Névralgies*, &c. Paris, 1841.

without exception, sensitive on pressure in the intervals between the attacks, but much more so during the attacks; at these places there is, in nearly all cases, a constant dull pain, or bruised, tense, or numb feeling; and from these points the lancinating pains diffuse themselves, either by spreading along the nerve in a continuous line, or leaping over to other spots. Pressure sometimes merely increases the dull pain in these points, sometimes causes twinges along the nerves. The extent of surface over which this tenderness exists is very small, often less than a square inch. The tenderness may be absent at one examination, and be found on a subsequent one; it would appear sometimes to intermit. These facts explain, wholly or in part, the various opinions of observers as to the effects of pressure. I have many times examined the points above mentioned as focal points, in facial and sciatic neuralgia, and have only once failed to ascertain that the patients had painful sensations there in the intervals of acute paroxysms, and that pain was produced by pressing these spots. Valleix mentions but one exceptional case—a sciatica, which had existed only twenty-four or thirty-six hours, and was very slight.

Neuralgia commonly occurs in the nerves which, in health, are conductors of sensation. Is it confined to these, or do some motor nerves ever become morbidly sensitive in this affection? The inquiry is interesting, because many cases of *tic douloureux* occupy the side of the face, and follow nearly or exactly the course of the branches of the facial nerve or *portio-dura*, a motor nerve. They are often believed to be seated in this nerve. Some are accompanied by spasmodic motions of the cheek, which gave rise to the name *tic*, and these are thought to show that the motor functions of this nerve are implicated. To suppose that the same nerve-fibres are at once sending an impression of pain toward the brain, and also a motor impulse from the brain to the muscles, would be in contradiction to all received notions of physiology. In one case—and perhaps many such have occurred—a competent surgeon considered a neuralgic affection to be seated in this nerve, the pains spreading over the cheek from the stylo-mastoid foramen, where this nerve emerges from the cranium. He divided this nerve near its place of exit, with the effect of paralyzing the muscles of that side of the face, with no mitigation of the pain. As to the inference drawn from muscular spasm accompanying the pain, it has been shown by contemporary observers, that real involuntary spasmodic movements are rare in *tic douloureux*.¹ After all, their existence is explained by the anatomical facts, which will be found, we believe, to account for all these phenomena.

Communicating fibres from the inferior maxillary nerve² join the facial near its point of emergence, behind the jaw, so that the latter contains threads whose origin and endowments are the same as those of the trifacial or fifth nerve. In some dissections, this branch from the fifth nerve has been found as large as the whole *portia dura* of the seventh. It has been shown by experiment that, when the fifth nerve is divided within the cranium, the seventh loses all traces of sensibility.

Nor is this all. The posterior branch of the second cervical nerve sends a division to the back part of the scalp, passing almost exactly over that point already named, where the facial nerve issues from the cranium. In many cases, the seventh pair was believed to be affected because the pains started from near the mastoid process. But in many of them it will be found that

¹ Rowland and Valleix. They occurred in four out of fourteen cases observed by Valleix, and in sixteen out of fifty-five, whose history he collected.

² From the ascending branch of the anterior auricular.

the starting-point is intermediate between the mastoid process and spine, exactly where this cervical branch comes out from the muscles to become superficial. Indeed, the same thing is mentioned by authors who did not interpret it as I would do. Halliday, in his Treatise before me, gives a case of "neuralgia of the facial nerve;" it is stated that the pains "set out from the occiput, a little above the nucha, between that and the mastoid process." There had been a softish swelling in the same spot. It is believed these anatomical circumstances may explain most, if not all the alleged instances of neuralgia of the facial nerve.

II. A consideration of the causes of disease is, in a practical point of view, one of the most important parts of its history. It is too common to pay exclusive attention to the structural changes, the visible effects of morbid agents, and to classify disease on such principles alone—an error which cannot fail to have its influence on treatment.

Of the local causes of neuralgia, the most frequent undoubtedly is exposure to cold and wet. These attacks, in my own observation, are most frequent in the earlier months of spring. A residence in cold, damp situations and climates is a predisposing cause. Bellingeri analyzed the history of forty cases, and found thirty-four of them attributable, as he believed, to exposure of this kind. A cold, dry, and piercing wind, or sitting in a current of air, sitting long in wet clothing, are the exciting causes most often and distinctly recognized. Indeed, any agent which deranges the circulation of the surface for a long time, so that a proper reaction is not established, is capable of inducing attacks.

The forms of neuralgia most often produced in this way are, I believe, facial and sciatic. The branches of the trifacial or fifth nerve are distributed over a large extent, many of them are quite near the surface, and the part they supply, the face, is habitually most exposed to changes of temperature. The sciatic nerve is deeply covered, but its fibrous envelop is of denser structure than the neurilemma of other nerves, more allied to the textures usually invaded by rheumatism; and we find, accordingly, that it is very liable to an affection completely analogous to rheumatism in its nature and causes. Sciatica prevails in cold, wet seasons, in stormy and changeable weather, and in low and damp places; also, according to European observers, in sailors and fishermen. The severest and most intractable cases I have seen were in Irish labourers, who had been employed in wet and underground work, and wore their clothing wet for hours.

Exposure to heat has sometimes produced neuralgic attacks, as in the face of a cook who had to stand over a hot fire a great deal. In connection with this may be mentioned a case I have lately seen in a man whose face is often exposed to the vapours of hydrochloric acid. He says that many of those working in the same business have eruptions on the face, which he has not. His neuralgia has lasted six years, and occupies the terminal branches of the superior maxillary nerve.

Direct mechanical injuries are not among the most common causes. Bellingeri analyzed forty cases, and found but two of them from injury. Halliday relates twenty-seven, and only two of these came from injury. In several cases proceeding from this cause, the disease has continued for months or even years after the accident; and the origin of the symptoms has been shown by their immediate subsidence upon making an incision over the part. One case in a girl of fifteen, began after a severe blow on the right temple, continued rather mild for the first two months, then ceased for two or three months, and came on again in severe neuralgic fits. No immediate cause for this recur-

rence could be ascertained, except that the catamenia had failed to appear. Relief was twice obtained by covering the cicatrix with lunar caustic. This point was so tender at times, that the slightest touch produced acute suffering, which at one time was followed by general convulsions. The catamenia were regularly established about thirteen months after the original injury, and the neuralgia rapidly improved after that time.

Venesection is known to have often produced neuralgia, both in the arm and leg. A puncture of the superior maxillary nerve at the infra-orbitary foramen, made for the purpose of applying galvanism, has caused it. In some cases of neuralgia from puncture, it has been proved that the nerve was pierced or partially divided, and a complete division at the wounded part, or about it, has sometimes given relief. But it is not certain that a nerve is touched in all these cases. The analogy of other causes, such as cutaneous eruptions, leads me to doubt whether any inflammation or great disturbance in the circulation around a number of the most minute terminal fibres may not serve as an excitant, and whether, therefore, we need always suppose the wound to have reached a nervous trunk. Dr. Rowland mentions a person who had neuralgia from leech-bites, applied to a bubo.

Neuralgia seldom begins immediately after the wound, or rather, as soon as the pain caused at the time has subsided. Such a fact is not unknown. One man had a severe laceration on the thigh, leaving some filaments of the sciatic nerve hanging loose. Shooting pains, with spasmodic twitchings, came on in about four hours, and were followed by numbness, &c. All these symptoms were passed in nine days. The disease rarely presented so acute a form. In general, neuralgia following external injuries, begins after cicatrization, or even some months later. With a view to prognosis and treatment, it is very important to distinguish between those cases in which the symptoms come on within a few hours after the injury, and those in which they manifest themselves after the wound has healed. In the former, we have more reason to think a wounded nerve has occasioned the pains, and we may suspect a partial division of the nerve. An operation is much more likely to give relief in these cases. "It is always to be feared," says Mayo, "when the nervous symptoms have been slow in coming (and, therefore, have been present for many days), that they are dependent not more upon the injured point of the nerve than on an irritation involving its whole length." Some of those which immediately follow a wound may be presumed to be inflammatory; as in the above-mentioned case which followed laceration.

It is important to bear in mind that an injury which had not been suspected of having involved any particular nerve, and perhaps had occurred so long before as to have been forgotten, may still cause acute nervous pains. For example, Dr. Rowland mentions a girl, aged sixteen, who had paroxysms of darting pain in the *left* temple and side of the head; and upon inquiry it was found that several years previously she had received a severe cut over the *right* parietal bone, which was long in healing, and this spot had been tender ever since. A large uneven cicatrix was discovered, when the hair had been removed, and a blister over this part relieved the pain for several weeks. The pains may be much more distant from the seat of injury, and even tenderness of the cicatrix may be wanting as a guide to their true source.

Pressure upon nerves is a more common local cause. It may be made by some foreign body, a tumour, an enlarged or displaced organ, a curved spine, stercoral concretions, the passage of the foetal head, the pressure of crutches against the armpit, &c. In a neuralgia of the spermatic cord, pain was excited by flatus passing through the cæcum. Dr. W. E. Coale, of Boston,

witnessed a case of neuralgia, relieved on the appearance of a deciduous tooth, which had remained in the upper jaw till the person reached mature age.

Pressure by bone is a well-recognized cause. Sir Henry Halford may be thought to generalize from his observations a little too broadly; but as he gives facts for his authority, we will not make this topic a bone of contention. He thinks that "the disease (*tic douloureux*) is connected with some preternatural growth of bone, or a deposition of bone in a part of the animal economy, where it is not usually found in a sound and healthy condition, or with a diseased bone." He gives four cases, all of them interesting, and three of them pertinent here. A lady, aged forty, suffered severely from *tic douloureux*; it was observed that the attacks were frequently preceded by uneasiness in one tooth, which exhibited, however, no signs of unsoundness. The tooth being extracted, a large exostosis was observed at its root; and the lady never suffered more than slight attacks afterward, and those very seldom. In another, recovery took place after the completion of an exfoliation from the *antrum maxillare*. In a third, there was exfoliation of the alveolar processes, and relief occurred only after an apoplectic seizure. This may have depended on disease within the cranium, as in the case of Dr. Pemberton, related in the same connection by Halford, but which we may do best to consider under another head.

Tumours may form in the nerves, and become the radiating centres of neuralgic pain. Though not among the most common causes of neuralgia, still, tumours of nerves form an important part of our subject, and we will give a condensed sketch of the leading facts respecting them.

The only known cause of them is a wound or similar irritation of the nerve, at the point where they appear. Hence, they are divided into two varieties, the traumatic and idiopathic neuroma. They are generally ovoidal, their long axis parallel to the nerve, movable, not adherent to the skin. The majority of cases where but a single tumour occurs, are believed to be of traumatic origin; but under some unknown predisposing influence, many tumours, even hundreds, may exist in one person. The symptoms of neuroma, when seated in superficial nerves, correspond very well with those of neuralgia—constant sensations of tingling, &c., with exacerbations of acute, shooting pain, excited by atmospheric changes, disordered digestion, &c., as well as external pressure. The pain is by no means proportioned to the size of the tumour. It is more moderate at first, perhaps no more than a tenderness, with many ill-defined sensations along the course of the nerve. Traumatic neuroma is, of course, most common on nerves near the surface. In cases where a great many exist in the same person, they have been found in the nerves both of the sympathetic and cerebro-spinal systems.

The structure of these tumours is pretty uniform. They are probably never cancerous, and never act like malignant disease. Those coming from injury are generally solid, but either variety may be encysted. The solid tumour begins in the neurilemma, or cellular sheath of the nerve, or else in one of the finer sheaths enveloping particular fibres of the nerve; the nervous threads may generally be traced through it, by careful management; those which are not involved in it are generally pushed one side, or some of them are spread and flattened out on all sides, according to the exact situation where the growth commences. The neurilemma by its compression gives the ovoid shape. The section of a solid neuroma shows it to be fibro-cellular and homogeneous; it cuts smooth. Those in the shape of a cyst contain some kind of liquid. Rokitansky believes they serve as *bursæ mucosæ*.

A bulbous enlargement of this shape and structure forms on the nerves

after amputation, usually without occasioning pain. But some of the worst known instances of neuralgia have followed amputation, so that patients have more than once submitted to reamputation, for the chance of relief. "The adhesion of the end of the nerve to the cicatrix of the stump; its irritation by osseous spiculæ springing from the extremity of the sawn bone; the including of a nervous filament in a ligature; the occurrence of a conical stump, such are a few among the many circumstances which have been stated as the occasional causes of these terribly painful tumours."¹

Carious teeth are believed to be a very frequent cause of tic douloureux. From my own observation, I should not expect much good from extracting teeth, unless the pains occupied the very nerve whose branches pass to a diseased tooth. Brodie says, in his lectures in the *London Medical Gazette*, that he never knew a case where genuine tic douloureux was relieved by the extraction of a carious tooth; and a very experienced dentist informed him that he had arrived at the same result. Still, it cannot be denied that neuralgia, even at a distance from the face, is sometimes caused by carious teeth. Bell met with a case "in a gentleman, who had for some time suffered from a neuralgic affection of the right arm; the paroxysms were subsequently observed to commence with pain in the second molar tooth, and to be excited whenever it was pressed or otherwise irritated. Upon the removal of the tooth, the pain disappeared." A pain running up the left arm to the shoulder, chest, and cardiac region, has been immediately cured by extracting a carious molar.

Neuralgia has sometimes been produced by stretching of nerves, without any laceration or wound, as where a heavy weight has been suddenly brought into such a position as to be supported by one arm. Sir Everard Home reported in the *Philosophical Transactions*, for 1801, the case of a gentleman, who received a violent sprain of the thumb by the weight of his body being thrown upon it, in saving himself when he was nearly thrown off of his horse. The attacks of pain extended up the arm; and the case proved very obstinate. A nerve was divided without obtaining a cure.

Various irritations and eruptions of the skin are sometimes curiously associated with pains in the neighbouring nerves. The congestion, or peculiar state of circulation in the minute vessels, which immediately precedes the appearance of eruptions, is more commonly marked by itching and diffused burning heat, but sometimes assumes a properly neuralgic character, particularly in herpes zoster, also sometimes in eczema and impetigo. These pains may last through the eruptive period, aggravated by everything that heats the surface. The most obstinate pains, I believe, that are connected with herpes, are those which follow its disappearance, and which are noticed by Dr. Bright in his lectures as particularly refractory to treatment. A recent writer, Marriott, mentions that he has observed many cases, which all subsided spontaneously after some weeks or months.

Carbuncle may cause neuralgia, in near or distant nerves, as shown by a case which was long under my own care. The carbuncle was a large and bad one, seated over the left scapula. Its appearance was preceded by severe pleurodynia on the same side; at the height of the suppuration there were paroxysms of pain in the subscapular nerve; and at that period the following symptoms first appeared. Pain and numbness were felt down the left ulnar nerve, particularly at the point where it passes beneath the inner condyle, and from the inner side of the wrist down the little finger, and half the next. The pleuro-

¹ This quotation, as well as most of the account of neuroma, is from an excellent monograph, "On the Pathology, Diagnosis, and Treatment of Neuroma." By Robert W. Smith, M. D., &c. Dublin, 1849.

dynia was very distressing, was always relieved by soothing applications to the carbuncle, and never so severe after that was freely opened. It disappeared long before healing was effected, but the pain in the arm continued. The spot was quite slow in healing, being exposed to continual fretting by the motions of the scapula; and while it continued open, the pains down the arm were constantly increased by anything that irritated the little ulcer, and relieved by soothing applications. After complete cicatrization, for a month or two if not longer, pressure on the scar would cause painful and strange thrilling sensations down the ulnar nerve. A second, comparatively small boil, on the corresponding point on the right side, caused much slighter and very transient suffering of the same kind, at the same points of the right ulnar nerve.

Inflammation of mucous membranes has been known to excite neuralgia in those nerves which supply the inflamed membranes. This has been observed in influenza particularly. I have just had a case under treatment, where the fits of pain shooting over the whole scalp and temple, recur about three o'clock every morning, with a good deal of regularity, and last till late in the forenoon; when they seem to be relieved by a copious discharge from the pituitary membrane. The pains were confined to the region of the frontal sinus—a severe headache, till the influenza had lasted about five days. There are some curious notices in authors of similar pains, caused by larvæ of insects lodged in the frontal sinus.

Neuralgia may have its origin in the nervous centres. It would be obviously out of place to give an account of all the affections of the brain or spinal cord in which it may arise as a symptom. Softening of the brain is more often indicated in its earlier stages by spasmodic contractions of the muscles, particularly rigid spasm, and by prickings or numbness; sometimes these are preceded or accompanied by shooting pains in the limbs. I do not know that these pains ever follow exactly the track of a nerve. Like symptoms are apt to be among the earlier indications of softening of the spinal cord.

Neuralgia is thought to be sometimes a symptom of cerebral congestion. Interesting cases are related by Dr. Copland, in his *Dictionary*. One, in a gentleman aged about fifty, was treated by him as depending on an active determination of blood to the head, and disappeared. In about two years it returned, and was followed by apoplexy and death. A lady about the same age was relieved by cupping on the back of the neck, but the pains soon returned, when carbonate of iron was prescribed by another attendant; after this “she immediately became maniacally delirious, afterwards hemiplegic, and she soon afterwards died.” An autopsy showed intense congestion and signs of previous inflammation.

Similar statements will be found in the writings of Dr. Armstrong. He regarded congestion, or disturbed action in the vessels of the brain, as the most common cause of *tic douloureux* and of periodical headache.

In the *London Journal of Medicine*, August, 1850, is a case of pains which were unaccompanied by any signs of inflammation, and considered and treated as neuralgie; situated in a limited spot near upper and inner margin of right ilium. An unnatural fretfulness was the only proper cerebral symptom. The liver was enlarged, so as to be felt below the ribs, and this was thought to explain the neuralgia. These symptoms began in the autumn, the pain suddenly subsided about the middle of the following April, and two weeks after an attack of apoplexy occurred, which was fatal in five days. Autopsy revealed extensive softening of left side of cerebrum, with effusion of blood. The cessation of pain was probably owing to the progress of the disease in the brain.

Sir Anthony Carlisle mentions that a lady was relieved of infra-orbital neuralgia, by three bleedings within a week of one another. First, sixteen ounces were taken; three days later, twelve ounces; three days later, sixteen ounces again. She was entirely free from pain, and never had any return of it. She died two years after this, of apoplexy. These facts should not be forgotten in estimating the gravity of this disease, and in determining whether to give tonics.

In the case of Dr. Pemberton, there was disease of the bones within the cranium—an unusual thickness of the os frontis above its sinuses, and near its junction with the parietal bones; and in the falx, near the crista galli, there was “a small osseous substance about three-eighths of an inch in length, rather less in breadth, and about a line in thickness.” After amputation, performed for neuralgia in the leg, the posterior surface of the spinal cavity has been found studded with cartilaginous and bony deposits.

It would be interesting to obtain a series of facts, to show how frequently neuralgic pains are connected with a tendency to serious disease in the nervous centres, and how far they may be premonitions of such disease. In persons approaching that age when apoplectic and paralytic affections may be feared, obstinate sciatica, not traceable to any obvious external cause, should always call our attention to the state of the brain. Facial neuralgia, too, has sometimes passed into severe periodical ache in the inside of the cranium, followed by cerebral symptoms. Before leaving this subject, we will quote the suggestive remarks of Copland, from whom we have already given cases in point. After referring to the external physical causes, cold and wet, &c. he says: “When neuralgia cannot be imputed to any of these causes, when it is occasioned by less manifest causes, when there is reason to suspect that organic lesion exists within either the cranium or spine—protracted disease may be expected, and the supervention of another malady, generally resulting from the progressive increase of the primary lesion, and of still more fatal tendency, may be anticipated although at a more or less remote period. In a very large majority of these cases, neuralgia terminates in some related malady, in a convulsive, epileptic, apoplectic, or paralytic seizure. From either of these the patient may recover partially, rarely completely, and be again attacked, but he seldom experiences the neuralgic affection, or at least in the same form or degree of severity. Of the several maladies into which neuralgia passes, palsy, generally in the form of hemiplegia, sometimes in that of paraplegia, when the lower extremities have been the seat of the affection, has been that most frequently brought under my own observation. Next to this, apoplectic, or apoplectic joined with convulsive seizures, have been noticed.” In cases I have witnessed, where neuralgia was followed by palsy, I have not seen any relief ensue, except so far as the sensorium was blunted for the time against the perception of pain.

The following remarks of Simon, one of the ablest recent writers on pathology, are too interesting to be overlooked. He gives no specific facts to confirm them: “It is in these diseases of subordinate centres—chiefly of tertiary centres—that *neuralgia* so commonly arises; for by this term, when it is properly used, we denote the subjective experience of pain in a part where there is no equivalent objective disease; and in most of such cases, the disease is not in the nerve-tubules, but amid the gray nerve matter of the secondary or tertiary centre”—i. e. the cerebellum, optic thalamus, corpus striatum (secondary), or gray matter of the spinal cord, or sensitive ganglia (tertiary).

As this essay is intended to have a practical character, we will not go deeply into the vexed question of spinal irritation as a cause of neuralgia.

Certain cases of this disease in the face, and more particularly in the intercostal nerves and abdomen, are connected with tenderness alongside the spine—often confined to the surface over those intervertebral foramina which transmit nerves to the parts affected by pain. This tenderness is exhibited on pressure, or on applying heat or cold, as for instance by a hot sponge or the cold hand passed down the back; and also after some motions of the limbs or body. Pressure here will sometimes excite the painful or other peculiar sensations in distant parts. There is often a little œdema about the tender spots; and it is said the skin is at times more easily irritated by such applications as tartarized antimony than the surrounding parts. Lastly, many such complaints are relieved, or perhaps permanently cured, by leeching or counter-irritation over the neighbourhood where tenderness exists.

These facts have been ascribed to a state called “spinal irritation,” of which only a very vague idea can be formed. It has been considered a very moderate sort of inflammation—proved to exist by the tenderness on pressure, and by the very prompt good effects of leeching, blistering, &c. As to this theory, it is enough to say that no pressure can, as a glance at the anatomy of the parts will show, be transmitted from our fingers on the outside to the spinal marrow itself. And the effects of treatment are by no means so constant as to make a fair ground for pathological inferences. They show, we think, that there is a state—temporary congestion it may be—of these parts of the spinal cord, which causes or keeps up the neuralgic, and other distant affections. But the relief is both very uncertain, and sometimes so surprisingly prompt as to show that some other condition than inflammation has existed.

The tenderness itself has often a neuralgic character. It is in many cases excited by a slight brush or change of temperature more than by firm pressure. Pinch up a fold of the skin over the intervertebral foramen, and in many of these cases the local sensitiveness will be evinced, and, what is more, the distant neuralgia will be waked up. This result is not constant, it is true, but neither are any of the phenomena of spinal irritation. We have seen (p. 418) that tenderness on pressure usually occurs, in neuralgia, at those points where the nerve issues, either from bony foramina, or through the mass of muscles, to come nearer the surface. All the facts of tenderness, &c. prove, then, is, that the posterior branches of the spinal nerves in question are affected together with the anterior, and that very probably the ganglia contained in the cord itself are the media of communication between them. It should be added, to illustrate the inconstancy of all these facts, that many of these patients have numerous tender points in different regions, a general sensitiveness—that the same spinal tenderness may be found without any neuralgia, or any of the other complaints which modern theorists have traced to the same source—and, conversely, that just such symptoms, apparently owing to some peculiar state—call it irritation if you please—of the spinal marrow, are found without any of the spinal tenderness. A man was in my office yesterday, with quite a variety of visceral neuralgia, which, according to his story, take turns with a chronic eruption on his skin; there was a fine assortment of the remote effects of spinal irritation, but no flinching when I examined his back. The case was no remarkable one.

Let us try to shun hypothesis, and state what is most important in this connection:—

1. Pains in the course of the anterior branches of spinal nerves, and also spasmodic and various nervous affections, sometimes coexist with pain and tenderness in the corresponding posterior branches; and the tenderness is frequently, as we might expect, at that point where the posterior branch issues from beneath the muscles, to become more superficial.

2. In addition to this, there is sometimes disorder in some internal organ, which is probably primary, the spinal cord serving as the medium through which irritant impressions from this organ are conveyed to the nerves affected.

3. Some persons have, with spinal tenderness and neuralgia, a good deal of suffering in bending, stooping, or even holding so hard as to make the ligamentous stays of the vertebræ tense. I have seen such cases proceeding from a strain, as in trying to hold a runaway horse; and some of them appear to be rheumatic, the ligamentous and adjoining white textures being probably involved.¹

4. In determining whether a painful affection, say within the abdomen, be inflammatory or not, we may sometimes infer its neuralgic character from the presence of this neuralgic state of the posterior spinal branches.

Beside functional derangements, particularly of the uterus, many organic lesions may produce spinal irritation—as tubercular deposition in its early stages, and wounds of nerves.

As to neuralgia from disorder of the digestive organs, I can say little from my own observation. Sir Charles Bell has given some five cases of *tic douloureux* successfully treated by croton-oil, and was disposed to infer that they depended on derangement or overloading of these organs, which he easily explained by the connections between the trifacial and sympathetic nerves. Many English physicians of the schools of Hamilton and Abernethy, attach much importance to the treatment by purgatives. It is certain that neuralgic attacks may be excited, in persons subject to them, by indigestible food in the stomach or bowels, and by the passage of cathartic medicines. From the teeth to the rectum, every part of the alimentary canal may make its disturbances felt in this manner. In a case of stricture of the œsophagus, every attempt at swallowing excited acute pain in the little finger. Pain in the lower limbs has been known to come on violently after dinner, and be relieved on vomiting up some ice-cream; also to be immediately removed by swallowing an alkali, by removing fecal accumulations, and by replacing a protruded hemorrhoidal tumour. Many persons of sedentary habits have their *tic douloureux* usually aggravated by allowing the bowels to become confined, and severe attacks relieved by free evacuation. I have known one who avoided sweetmeats of all kinds, from having observed that they excited paroxysms of facial neuralgia. Possibly, this person had carious teeth, which produced the pains.²

The mention of nervous pains in the lower limbs from hemorrhoids naturally reminds us of like cases caused by distension of the bladder and the passage of calculi. I have seen the same after lithotomy. The patient, a man of 70, suffered within less than a week after the operation, from a painful swelling of the thigh and leg, resembling *phlegmasia alba dolens*, which subsided in five or six days. For a few months after the subsidence of this, he was distressed by fits of lancinating pain down the long saphenous nerve.

Rheumatism undoubtedly depends on a material cause circulating in the blood, probably lactic acid evolved in the transformations of the tissues. It invades all the membranous forms of areolar tissue, from the fibrous textures about the joints to the serous membranes. The sheaths of nerves belong to this class of tissues, and may be the seat of rheumatism. Were I to take my own observation as the only guide, I should be surprised that any doubt of this existed among practical observers, particularly as regards sciatica. This and other forms of neuralgia are common among those who are much

¹ See Wood's Practice, vol. ii.; art. *Spinal Irritation*.

² For interesting cases in point, see Edin. Med. and Surg. Journal, April, 1850. (Communication by Dr. Stark.)

exposed to wet and cold and atmospheric changes. Symptoms characteristic of neuralgia not unfrequently supervene in the course of acute rheumatism; toward its close. The analogous affection of temporary paralysis of particular parts, as the hand, without any signs of cerebral affection, has also been observed in the same circumstances. The painful sensations common in persons subject to rheumatism, at times when they are comparatively well, are sometimes indefinite in their seat, but in some cases follow the well-known course of some nerve. Rheumatism attacking the neurilemma, in an acute form, is known by the unusually severe pain, gnawing and piercing sensations, and by its haunting a particular seat, at the same time having marked remissions. I am glad to see that the frequent occurrence of rheumatism in this seat is recognized in the latest excellent work on this subject, that by Fuller of London. The following is the commencement of his chapter, "On Sciatica, and other forms of Neuralgic Rheumatism." "When rheumatism attacks the nerves or their fibrous envelops, it causes pain which follows the course of the nervous trunks, and extends along their several branches. The pain is bounded by such narrow limits, and can be traced so clearly following the track of the larger nerves, that it cannot be mistaken for pain affecting any of the other structures."

Some of the most intelligent clinical observers of our day have pointed out affections of the spinal cord and of the nerves, depending on the gouty diathesis. Dr. Graves has adduced cases tending to show that "gouty inflammation of the nerves and their neurilemma may in process of time extend to the spinal marrow and its investments, and give rise to the derangements of the latter, terminating in ramollissement and structural degeneration." In the *Edinburgh Medical and Surgical Journal*, Jan. 1854, Dr. Begbie gives numerous illustrations of the many forms of gout, among which, besides neuralgia, he mentions three cases of neuralgia in the lower limbs. One, a case of sciatica, was treated with colchicum and neutral salts, and complete relief was quickly obtained on two occasions. Another case, also a sciatica, was of a more asthenic character, the gouty diathesis "more suspected than proved or acknowledged," and yielded to quinia combined with colchicum.

For myself, I have only suspected this connection of neuralgia with gout; in some cases from finding no other ascertainable cause of neuralgic pains; from the hereditary tendency to gout or gravel in a patient's family, and from the good effects of colchicum. The neuralgiæ I have seen under these circumstances were facial (accompanied by inflammation of the articulation of the lower jaw on the same side), and visceral pains somewhat resembling colic.

Syphilis has been pronounced a cause of neuralgia, on account of the occasional cures by mercury. There are few operations of medicine more reliable than the good effects of iodide of potassium in syphilitic headache; and in some cases which were relieved by this medicine, I have been unable, on careful examination, to detect even any commencement of puffy swelling. Were they neuralgic, or are we to assume that there was some brooding inflammation of periosteum or bone? From their lancinating character, and their radiating from a well-known centre of neuralgic pains, near the parietal protuberance; I am disposed to consider them of this nature, in a case lately under my treatment.

The metallic poisons, lead and mercury, by their slow absorption into the blood, produce affections of a neuralgic stamp. The "lead-arthralgia" of Tanquerel is a disease remittent, subject to severe paroxysms, the pain being little dependent on motion of the part, and not increased, but oftener dimi-

nished by pressure. The scalpel shows no correspondent anatomical lesion in the limb, or the nervous centres; lead has been found in the muscles of the limb. The diagnostic marks between this and "common neuralgia," named by Tanquerel, do not characterize a distinct disease. His observations include 1,217 cases of lead colic, 755 of "arthralgia," 127 of paralysis, 72 of encephalopathy. Lead-colic is probably a compound of muscular paralysis and neuralgia, having its seat in the bowels; so that by far the larger number of the cases, where lead poisons the nervous system, partake of a neuralgic character.

Malaria is a well-ascertained cause. Neuralgia prevails in marshy districts and damp climates, and particularly in some of the haunts of fever and ague. A condition resembling the cold stage of intermittents has been noticed at the beginning of the neuralgic paroxysm, marked by a similar character of the pulse, and a sensation of cold, either local or general, followed by local heat. Neuralgia is often periodical, as regularly so as fever and ague, and with the same intervals. The same persons have often had ague and neuralgia alternately; and in particular, many who have suffered first from severe intermittent fever have afterwards had neuralgia. Bark and its preparations, and sometimes arsenic, have rapidly cured these periodical cases.

The following is quoted from Macculloch, in his *Essay on Marsh Fever and Neuralgia* (London, 1828):—

"In this case, the situation was so decidedly subject to malaria, that scarcely an individual, out of many different families which had resided in it, had escaped intermittent at some period of their stay. In one season, and in one family consisting of twelve or fourteen persons, the following were the effects on as many individuals: one tertian, one double quotidian headache, one diseased spleen; in one individual, aged only eighteen, a temporary hemiplegia, with obscure quotidian, and symptoms of diseased spleen; a regular neuralgia of the face, of double tertian type. In a following distant season, and in some of the same persons, there occurred palsy of the face, with imperfect speech—an attack lasting beyond a week, and replaced by quotidian neuralgia (tic); a double tertian, common intermittent, terminating in a quotidian or double tertian neuralgia; a quotidian with neuralgia in the shin-bone; the same patient having had, in a preceding season, a common tertian so obscurely marked that he was ordered to Italy for a consumption (a consumption which was cured by two ounces of bark and a change of place to ten miles distance); and, in a following one, having been attacked again with a double tertian, of which one fit was attended by neuralgia of the skin, and the other by a headache."

It is well to mention that the disease is not always of malarious origin when distinctly periodical. Many cases, even coming from a permanent organic cause, recur regularly, with the same intervals as the common forms of intermittent fever.

Lastly, whatever vitiates the blood, by withholding healthy nourishment, or interfering with healthy assimilation, may be the source of this painful disease. Anæmia itself is a most frequent cause of nervous disturbances, both of movement and sensation. They have been observed in limbs after ligation of the main artery, in women blanched by flooding, and in the anæmia of Bright's disease, and chlorosis. Many of these phenomena have been best investigated by Dr. Marshall Hall, to whose *Essay on the Effects of the Loss of Blood*, I may refer.

But the constitution of the blood, and healthy state of the nervous system, may be impaired by unhealthy modes of living as well as by actual disease.

The debility caused by neglect, voluntary or forced, of the great rules of living, is a most common influence predisposing to neuralgia. Our bodies cannot be kept sound without enough suitable food and oxygen, with exercise to quicken the reception of these elements into the system, and to distribute them in due proportion through our organs. The habit of sewing too closely, and the use of such articles of diet as give a temporary unnatural stimulus in the place of substantial nutriment—tea and coffee in particular—are the hygienic errors most needing to be noted.

III. The subject of this essay (as at first announced) requires something to be said of the "nature" of neuralgia. Our knowledge on this question is so slight and unsatisfactory that we shall devote very little space to it. In the majority of cases, where dissection has been had, no morbid changes have been found in the nerves. When found, they have been various, and not always in the part where the symptoms appeared. The knowledge derived from the symptoms and course of the disease is almost equally negative; we learn what neuralgia is not, and some of the laws it observes; but do not know what it commonly is. In truth, while we do not know the nature of the processes by which healthy sensation is effected, how can we understand the mechanism of diseased sensation?

1. Some rare examples of neuralgia depend on inflammation of the nerve. We related a case at page 421, which shows this by its history. In other instances, the nerves have presented redness, serous effusion among their filaments, bloody infiltration, enlargement, hardening, softening, or finally purulent effusion, indicating that inflammation had existed, sometimes acute and at other times chronic. A remarkable instance of the acute kind is mentioned by Gendrin. In this case, and the one just before alluded to, numbness in the parts below the inflamed part was a prominent symptom. We shall presently see that Fuller mentions as signs of effusion in sciatica, "a dull, aching, benumbing pain in the limb, causing it to feel swollen." Simon says: "It is practically of great importance, as a point conducive to accurate diagnosis, to notice whether the neuralgic affection be or be not accompanied by anæsthesia (or numbness.) Attention to this point will often enable us to give confident prognosis as to the issue of a particular case by determining whether the symptoms are such as to imply destructive disease in the nervous apparatus of the part. Neuralgia of long duration can hardly exist separately from anæsthesia, as a result of such disease." Other more obvious signs of inflammatory neuralgia are tenderness on pressure over the whole course of the nerve, felt enlargement of the nerve sometimes observed, and the greater fixedness and constancy of the pains.

But the inflammatory character of this affection is seldom shown, either by its symptoms, the effects of treatment, or *post-mortem* examination. For discussions of this whole subject, we may refer to the able essays of Drs. Oliver W. Holmes¹ and Richard Rowland.²

2. In some forms of neuralgia, we may convince ourselves that it arises from a morbid state of the blood, in particular from a poison circulating in it, which we may believe from analogy—perhaps in the case of lead it will be proved by analysis—comes into direct relation with the nerve-substance or neurilemma.

3. We find neuralgia in certain instances a manifestation of disease in the nervous centres; and we may feel that we know something of its nature,

¹ Boylston Prize Essay. Boston, 1838.

² A Treatise on Neuralgia. London, 1838.

when we connect it with those laws of the nervous system by which affections of the nerve near its origin are referred to points along its course.

4. Those cases in which disturbances of one organ excite pain in a remote part, through the means of nervous connections, are illustrations of what is called *sympathy*. But this term only serves as a convenient name, like the x or y of the algebraist, to represent an unknown thing; and though we may study its phenomena, assort them under different heads, and generalize our statements into laws, we are still ignorant of its nature. Interesting methodical accounts of the phenomena of morbid sympathy may be found in the recent works of Henle and Simon on General Pathology.

IV. *Treatment of Neuralgia*.—We have dwelt at length on the conditions under which neuralgia arises, because they afford the most important hints for treatment. Many of them admit of being removed or lightened. It is true that the removal of the apparent causes may not cure the disease, yet even here this knowledge is useful, because the continued operation of the causes may be stopped, and also because powerful nervines have a great control over those cases in which the pains persist after the condition that first excited them has ceased. But there are generally several causes or influences that keep up the disease, and a physician's tact will be best shown in detecting and disentangling them. Thus there is often a general state, such as anæmia or the rheumatic diathesis, making a person liable to nervous pains, and some local exposure or accident determines the place where this constitutional cause shall take effect. If our management be not founded on a comprehensive understanding of the case, its success will be at best very uncertain.

Let us develop these ideas a little in detail. Exposure to cold and wet must be avoided. A person living in a damp and chilly situation, or obliged to work in the water, can hardly expect a cure, unless he can change his business, and go to a dry and comfortable dwelling, or hospital. This applies most strongly to sciatica, yet not exclusively. The best writer on neuralgia of the face, Halliday, urges the same measures, an omission of which may, he says, compromise the good effect of any other treatment. The patient should be taken also from a situation exposed to marsh miasmata. Non-conducting substances should be worn over the affected part, if possible. Flannel and silk are useful, and in sciatica, buckskin drawers. Flannel covered with oiled silk is worn by some, attached to a sort of visor, to prevent, if not to cure, attacks of facial neuralgia. The usefulness of many plasters depends upon their protecting the surface against changes of temperature.

The existence of rheumatism, gout, and the various poisons in the blood, should all occur to the mind as possible causes. We cannot give in full the symptoms or treatment of either of these diseases, yet the following sentences of Fuller are apposite enough to be quoted. They are written with a special reference to sciatica: "If the patient is thin, pale, sallow, and extremely sensitive to atmospheric vicissitudes; if he has experienced pain, or threatenings of pain, in other parts of the body; if, at some former period, he has suffered from rheumatism affecting the joints; and, above all, if his present attack is the result of exposure to cold and damp, the disease under which he is labouring is rheumatic, and is to be relieved by vapour-baths, guaiacum, alkalies, and similar remedies. On the other hand, is he stout, florid, and a free liver, taking little exercise and sleeping much; is he plagued with heartburn, acid eructations, and occasional lowness of spirits; or has he

previously suffered from gout, his malady is certainly of gouty origin, and is to be cured by colchicum, alkalies, and alteratives. Again, is he cachectic, and out of health; has his throat been ulcerated, or his skin disfigured by blotches or eruptions; has he taken mercury, or experienced pain in his bones, the mischief is probably due to a syphilitic taint, and is to be cured by sarsaparilla, with iodide of potassium."

In the pains from syphilis, benefit is also derived from nitric or nitromuriatic acid washes.

When an individual, once strong, has become weak and pallid, without a rheumatic diathesis, and we recognize anemia as a source of the neuralgia, we have a case for the use of iron. The strength and size of pulse, appearance of red blood in the complexion, and presence or absence of other symptoms that are known to be often produced by general anemia, are to be inquired into. Iron is inappropriate, not only in full-blooded persons, but also in those who are predisposed to inflammation or irritation of the stomach and bowels. Its best effects are seen in cases where no local cause is known. Its effects on the action of the bowels are different in different persons. In moderate doses it is apt to constipate, and require some cathartic joined with it. But only a small part of the large doses of the carbonate of iron is absorbed. When a drachm or more is given, most of it passes off in an insoluble form, the sulphuret, and by its mechanical irritation in some persons it promotes peristaltic movement. The large doses, therefore, cannot be more useful than moderate ones, by their increased power of furnishing iron to the blood. Several authors who recommend the carbonate, empirically, insist on the greater efficacy of large doses; and perhaps this is owing to a revulsive action on the bowels. Thirty grains three times a day are often given; some failures are attributed to this *small dose*! It is said that half an ounce every four hours can be well borne, producing no inconvenience unless some feeling of weight in the stomach, as long as the bowels are kept freely open. In employing the carbonate, I have usually given not more than five to ten grains at a dose, combined commonly with powdered rhubarb and an aromatic (cinnamon).

After endeavouring to give some indications for the use of iron in what we may call the *rational* treatment, we must glance at its *empirical* use. Like most other drugs, it has been used too indiscriminately in neuralgia. The subcarbonate of iron was first distinctly brought forward, as a cure for neuralgia, by B. Hutchinson (London, 1822). He reported twenty-seven cases, all but two or three of which were cured, under a course in which this was the leading remedy. In those few, it gave more relief than anything else. Though it has been less uniformly useful in the hands of others, still, it has kept its place as one of the most valuable remedies.

Sir B. Brodie says of the use of iron: "We have never known any benefit to accrue from increasing the dose of the carbonate beyond a drachm thrice daily." He insists on the occasional administration of a purgative to prevent accumulation in the colon.

Whatever the nature of spinal irritation, there is reason to believe it means something, and that this something keeps up neuralgic and other nervous complaints. We must dwell a little on remedies applied over the spine. Simple rubbing and shampooing, even the pressure of a warm hand over the seat of tenderness, and anodyne liniments, give great relief. As a palliative, the following is excellent: one part of solid camphor and chloroform, each, mixed with two parts of olive oil, making a liniment. The belladonna plaster is of more permanent benefit. These harmless measures ought to be tried, unless symptoms are very severe, before leeching and blistering.

After the name of spinal irritation came into vogue in England, the use of leeches and counter-irritation over the tender vertebræ became very common, almost a national peculiarity, which has followed the Anglo-Saxon destiny to this side of the Atlantic. The little book of Teale, written to enforce this doctrine, has gained a reputation in both countries; the edition before me being printed at Concord. It recounts twenty-one cases of external neuralgia, angina pectoris, and other nervous affections, accompanied by some spinal tenderness, and relieved almost constantly by leeches and blisters over the spine. His cases show that these affections may be very much mitigated by such appliances, and some of an acute character even cured. But the long-standing cases were treated by so many different means at the same time, that it is hard to judge how much the local measures aided in a permanent cure, where such was obtained. A great many of these affections occur in weakly persons, who cannot well bear repeated loss of blood, which increases the general irritability. For myself, I have seen *permanent* good come from local depletion only when the cases were very recent. Persons who have suffered long may, however, be relieved of suffering, and, on this account, leeching and cupping may be, on the whole, beneficial in some circumstances. The harsher external applications, according to the best observers, have often failed; I have seen them followed by an increase of spasmodic disturbance, not, that I now remember, of neuralgic suffering.

Sir Benjamin Brodie, in his able lectures on local nervous affections, has raised his commanding voice against the indiscriminate use of these applications, particularly in young and nervous females, in whom we commonly find the symptoms in question. He believes that many are made invalids for life by the repeated losses of blood, each of which may have given relief at the time. He condemns in the same way the practice of enforcing a recumbent posture, and establishing a discharge over the spine, whether by tartarized antimony, caustics, or setons. Good diet, air and exercise, agreeable occupation of the mind, cheerful feelings, with some tonic medicines, are, he says, the best remedies. He also advises the use of belladonna plasters, which I have already spoken of.

From my own limited observation, I am convinced of the truth of Brodie's statement, and that many patients are kept invalids under treatment for "spinal complaint," who would be able to lead a comparatively active and happy life. Have we not all seen such cases? Granting that the condition of a portion of the spinal cord is one of congestion, or too great fulness of blood, is it not a result and indication of weakness? The phenomena of blushing, a result of moral weakness, have been very happily employed to illustrate this kind of congestion, which springs from physical weakness, or from moral and physical combined. To cure it, says an excellent reviewer of Brodie, "we should not leech and blister, or enjoin rest, but we should expose abundantly to fresh air, insist on regular and plentiful exercise, with sufficient intervals of perfect rest, feed with a most generous and nutritious diet, and with but little apprehension, in the majority of cases, of failing in effecting a cure."¹

Those who consider neuralgia to be very often a result of deranged digestion, will insist much on the management of the digestive organs. I have already mentioned some cures reported by Bell, &c. When an overloaded state of the primæ viæ is suspected, emetics and cathartics may be useful. I have read of remarkable cases where purgatives given for some days, with

¹ British and Foreign Medical Review, July, 1841.

no good effect for awhile, have at last brought away accumulated matters, and given immediate relief. In sciatica, they are often successful. The abdomen should therefore be examined, in any case where constipated habits, with any fetor of breath and loaded tongue, set our suspicions on this track. The following is an outline of treatment which has been very useful in my hands, especially in some cases of dorso-intercostal neuralgia of the left side, where the complexion was muddy, and tongue coated. A blue pill every fifth day, followed by a saline cathartic; extractum taraxaci in pills—say six a day; anodyne applications outside; bitter tonics after pain was removed, and a plain, nutritious diet through the whole period. One case was remarkable for having been aggravated by abstinence from meat or other hearty food, and for the prompt disappearance of symptoms under this treatment.

Dr. Hunt, who gives the most rational advice as to management of the stomach and bowels, ascribes many cases to atony in these viscera, and recommends arsenic after evacuants, to be continued until pain is relieved, and then some tonic to take its place. He combines the arsenic with camphor, or some sedative; or, in case of acid stomach, with bicarbonate of soda, giving small doses of liquor arsenicalis (four minims three times a day at first, increased to ten.)¹

I have nothing interesting to add as to the treatment of other constitutional causes of neuralgia, and will next speak of indications drawn from local causes. Several cases have already been mentioned, where neuralgia was relieved or cured by treatment applied to old scars, &c. A complete removal of the cicatrix by the knife has done this most effectually; and when the scar is tender, pressure on it exciting the lancinating pains, and the situation admits, this is the best remedy. In other situations, as on the scalp, a crucial incision or cauterization may be substituted. A line of cautery carried round the cicatrix will sometimes give temporary relief, and may be advantageously practised at the same time that the cicatrix itself is converted into a slough. This treatment has often succeeded.

“In neuralgia depending on cutaneous eruptions,” says Rowland, “relief may often be effected by touching the vesicle or pustule with lunar caustic. In herpes zoster, for instance, I have removed the severe neuralgic pains almost instantly by this means. Sometimes in these cases, one or two vesicles are pointed out as the chief source of the suffering; but, in general, it is better to apply the caustic to the whole of each group, or to cover them with a strong solution of this substance.”

Neuroma on superficial nerves is said to have sometimes disappeared spontaneously; and, in some recent cases of an inflammatory nature, arising from injury, leeches are stated (by Frank of Turin) to have succeeded in dispersing it. But it usually presents itself to observation as a chronic disease, curable only by operation. The safest and most eligible mode is to cut away both the tumour and piece of nerve to which it is attached. The tumour is usually too closely implicated with the nerve-fibres to be dissected out. In one instance, where a large tumor was dissected out from the axilla, death ensued, and there was no evident cause for death shown on *post-mortem* examination except inflammation of a nerve.²

Neuralgia after amputation is one of the most discouraging varieties. It has returned after three successive amputations of the same limb, and after the sciatic nerve had been exposed and an inch and a half of its length cut out. It has been cured in one instance by acupuncture, and in one by quinia.

¹ Braithwaite's Retrospect, p. 10.

² For further directions, consult Mayo's Pathology.

Indications for treatment may be derived from the type of the disease, which may be *inflammatory* or *periodical*. Neuritis may be known by its acute course, by tenderness, perhaps swelling over the nerve, and numbness and thrilling sensations, with little intermission; sometimes by spasms in the muscles, and the presence of some fever. General bleeding is seldom required. Dr. Rowland mentions having procured relief by it in a case of sciatica. Leeches, anodyne fomentations, and poultices, were used with best results in the cases best recorded (those of Martinet). Immersion in warm water, and irrigation with it, as practised in a Boston hospital, have both had very kindly effects. Dr. Holt, in commenting on some cases which he considers inflammatory, says: "Cold applications in these cases are commonly agreeable and beneficial, as a means of relief. The application of ice, or cloths wet in cold water, is often a very valuable auxiliary in this form of the disease."¹

The writer on rheumatism, Fuller, from whom we have already drawn quite freely, recommends means particularly to meet the condition of effusion within the sheath of the (sciatic) nerve. The signs of this effusion he thus enumerates: "When a patient who is suffering from sciatica complains of a *dull, aching, benumbing pain in the limb, causing it to feel swollen*; when this sense of numbness and increased bulk has succeeded to pain of greater intensity, accompanied by cramps and startings of the limb; and more especially when, in addition to these symptoms, there is more or less inability to move the limb, inability arising from loss of power, and not as a result of excessive pain," &c. In these circumstances, he recommends that measures be taken for the specific purposes of checking further effusion, promoting the absorption of that which exists, or mechanically evacuating it. For these purposes he uses cupping and leeching, if the effusion is supposed to be recent. In cases of longer standing, he uses long and narrow blisters along the track of the nerve, dressed with mercurial ointment, or sprinkled, if necessary, with powder of pure morphia, also mercury internally. In somewhat obstinate cases, when inflammation has gone by, and only its result, effusion, exists, he recommends acupuncture, to *mechanically evacuate* the fluid. "Six or eight pairs of needles, specially adapted for the purpose, are carefully inserted into the thigh, along the course of the sciatic nerve, the object being to puncture the neurilemma, and thus to allow of the escape of the fluid." This operation he has seen give complete relief in three cases, and in one the relief was permanent. He thinks failure may have been owing to the operation being tried when there was no effusion, or when inflammation was not subdued, or to the fluid not being reached.

Acupuncture has been used without this definite indication. Cloquet reported many successes from it, the needles being left in for some minutes, or even hours. Drs. Renton and Elliotson, in England, also used acupuncture extensively, with benefit, in sciatica. The former left them in only five or ten minutes, inserting sometimes ten along the thigh and leg. He effected many immediate cures in old obstinate cases. He inserted the needles with a slight rotating motion, and "*without pain*."² The effects of acupuncture, employed *empirically*, have been, on the whole, quite uncertain.

When neuralgia is *periodical*, following any of the common types of intermittent fever, whether caused or not by miasmata, it is usually cured for some months, if not permanently, by preparations of bark.

Even where the attack is not exactly regular, but tends to return about the same part of the day, there is no one medicine more to be relied upon, in the neuralgia so common in females of weak constitution and sedentary habits.

¹ Bost. Med. and Surg. Journ. May, 1845.

² Lond. Med. Gaz. July, 1841.

Many patients form a habit of using quinia on the return of a paroxysm of *tic douloureux*, cutting it off speedily, and relieving them for several months, or even a year or more. Some physicians employ grain doses three or more times in a day. Some, who have gained a good deal of local reputation, and self-confidence in the treatment of facial neuralgia, administer ten or fifteen grains at a dose. I have known the small doses to succeed immediately after the large had repeatedly failed.

Brodie says that very large doses may be needed. He had to raise the dose, in one instance, up to half a drachm daily, of something called quinia. It is used in as large quantities as that in France. Brodie speaks with perfect confidence of succeeding by the use of cinchona or quinia, combined; if needful, with

Arsenic. This and the carbonate of iron are both considered particularly useful in periodical neuralgia. Having no extensive experience in its use, I will once more quote Dr. Hunt: "Arsenic operates most favourably on persons who are of lax fibre; accompanied by a languid state of the circulation, and whose secretions are rather profuse than otherwise; the urine pale and plentiful, and more especially on those whose skin is cold and moist. In persons of this description, whilst arsenic, to an extent far beyond other medicines, relieves the neuralgic pain, it improves the general health, and gives firmness and vigour to the constitution. When the urine is of a deep colour and scantily secreted, or when it deposits the lithate of ammonia, the tongue loaded, especially if the tip or edges of it are red, arsenic almost invariably disagrees, and aggravates the pain. But as this morbid state of the system frequently depends on or is complicated with disorder of some important viscus, arsenic will often agree, and relieve the neuralgic pain, after the visceral disorder has been removed by appropriate remedies."¹

Our distinguished countryman, Dr. Gross, has found Fowler's solution less serviceable than the arsenious acid in substance—less effective in curing the disease, and more likely to nauseate.

In some intermittent cases, M. Cazenave applied ligatures or tourniquets to the limbs, with very happy effects. One patient had excessive pains in the head, returning periodically, and baffling all treatment. Ligatures were applied to each limb a few hours before the expected attack, moderately tight. The pains were lessened the first day, still less the next, and did not appear at all on the third day. He advises that the application be continued after the disappearance of the neuralgia, for five or six days.

But supposing that we have no obvious indications for treatment, drawn from causes of disease to be counteracted, or from the peculiar features and type of the case in hand. We have still many resources, and some light—alas! too little—to guide us in their use. It will be necessary to name a good many medicines that have gained the best reputation in this painful disease. They are undoubtedly used too much at random; and what we need is, less to have new medicines, than to learn how to use what we have. Let me once more enjoy the luxury of transferring to my humble pages the counsels of one of the most judicious, observing, and enlightened physicians, Brodie. He is addressing students.

"Nor can I think well," says he, "of this modern fashion of resorting on all occasions to novel methods of treatment. I advise you, if you wish to succeed in your profession, and to be useful to society, to pursue a different course. Make yourselves masters of the old remedies. Learn how to handle them, and what good they will do, and, as a general rule, have recourse to

¹ *Prov. Med. Journ.* April, 1844.

them in the first instance. If the old remedies fail, and you are at a loss as to what you should do, then, and not till then, have recourse to the new ones."

In estimating the value of any medication for this disease, we must remember that intervals of ease, extending over a period sometimes of several months, may give us a hope that the disease is finally cured, but the pains may afterwards return. Many modes of treatment are successful only up to this point, and in the most exquisite forms of neuralgia it is a blessed boon to obtain even this respite.

The medicines which have done most service by their employment internally, might be arranged, not very strictly, however, under the general heads of sedative or narcotic medicines, tonics, and medicines whose operation is not understood—unclassifiable, alterative medicines.

As mere palliatives, sedatives are useful by cutting off the paroxysm, giving relief to pain, and lessening the power of that unhappy habit which makes future paroxysms more likely to ensue. In this respect, the anæsthetic vapours appear to me to be more useful than any other substances. Believing that chloroform is unsafe in the best hands, I always prefer sulphuric ether, though much less convenient and elegant. It need not be inhaled long enough to make one insensible; the power of perceiving pain being lost before consciousness of identity of perception of surrounding persons and objects. In forms of neuralgia depending on mere nervousness and irritability, the pains will not always return as the effects of ether disappear, and inhalation, therefore, does not require to be continued long. Many interesting facts of this kind are related by English physicians.

These substances are all more applicable to neuralgia excited by irritation reflected from a distant organ, or some temporary exposure, than to those cases where some local mechanical cause affects the nerve itself.

With a curative object, the best effects from sedative medicines are got by giving some of the vegetable narcotics, in full and frequent doses, until the pain is quelled, then continuing them with larger intervals for a while, and finally substituting a tonic. Among these narcotics, it is difficult to say which has the greatest weight of testimony in its favour. Leaving out aconite, as being a substance of very peculiar properties, we find different practitioners preferring belladonna or conium—less generally, stramonium or hyoseyamus, or the preparations of opium.

I have collated the testimony of many authors whose communications appear in the journals, as well as the systematic writers on neuralgia; but as the same principles govern the administration of all these remedies, I will not attempt to give the various facts on which different practical men found their preference for this or that medicine. Belladonna, employed on the principles just named, has, I think, the strongest testimonials in its behalf. The following mode of employment has succeeded very well with M. Trousseau: A quarter of a grain of the extract is given, in pill, every hour, until some dizziness or effect on vision is produced, by which time the pains are quieted. Then the medicine is continued with longer intervals. This course must be resumed, if the pains recur. After they have disappeared, a tonic is used; usually quinia or iron. This plan succeeds best in facial neuralgia. According to all who have used it, belladonna must be continued until some slight effect is perceived in the throat or state of vision, or vertigo. In full-blooded persons, it may be combined with ipecac., as recommended by Golding Bird for neuralgia dysmenorrhœa; or, in those who are pale and feeble, with iron or zinc.

Conium is preferred by many. Mr. Lawrence says it is the only one of these narcotics on which we can have any reliance for checking the paroxysms.

"I have seen in several cases, where it has been given largely and at short intervals—and it must be given in such doses as to produce some of its peculiar effects on the nervous system—that it has put a stop to the paroxysms, and for such a length of time that I have thought it had cured the disease; but in some instances, where persons have remained well for several months, and even in one case for more than a year, the pains have again showed themselves; but the agony has been considerably controlled by it." He does not mention what has been the result of following up this temporary cure with a tonic course.

Such has been the experience with all these narcotics. To state who has succeeded best with one, and who with another, would be hardly a profitable use of our remaining space. It is necessary, and often difficult to procure the substances pure and strong, especially the extracts. We must also seek carefully for any circumstances that may aggravate or keep up the irritation, and attend to the state of the bowels, and several secretions.

Opium and its preparations, are considered to be better suited to internal than external pains. Morphia and its salts are most employed. Their use may be profitably confined, I think, to cases that recur at night, or early morning, when an evening anodyne may be extremely useful. The acetate of morphia is stated, however, to have produced some cures, in the hands of Dr. Bardsley and others. But the more common opinion is, that it tends to increase nervous irritability and costiveness, more than the other narcotics, without having so much control over neuralgic pain. For immediate relief to agonizing paroxysms, it is every way inferior to sulphuric ether, used by inhalation.

Aconite.—The internal administration of aconite has often disappointed physicians, yet when used in full doses, and continued for some time, permanent relief has often been derived from it. Dr. Fleming has called particular attention to its power of depressing nervous and vascular action, and to its use in neuralgia. "He gives a table of forty cases; ten of these have been published by others, and they are all permanent cures. Of the thirty cases which fell under his own care, there were seventeen permanent cures, and in thirteen the relief was only temporary. In some of these, the medicine was used internally, in others externally, sometimes both." Aconite is considered an antiphlogistic, not well adapted for feeble persons. Dr. Fleming suggests that its internal use is most likely to be beneficial, when neuralgia depends on inflammation in any part of the nerve (a condition quite unusual), or on sympathetic irritation; its external use, when the disease arises from local functional irritation. The strong tincture which he recommends has occasioned several deaths, by its accidentally being swallowed in an overdose. He uses five minims three times a day, increasing cautiously. The article we get here is apt to be quite weak. On the whole, the results of aconite used internally, as I find them reported, do not seem to me so favourable as those of belladonna, while it is much less safe and manageable.

I have obtained good effects from the following combination of aconite with tonics. R.—Iron by hydrogen ʒij; tinct. aconit. rad. (sat.) ʒj; M. ft. pil. lx.

Give one pill after each meal, until the neuralgic pain ceases, and then substitute the following tonic pills. R.—Quin. sulph., iron by hydrogen, aa ʒj; M. ft. pil. lx.

All these narcotics, including aconite, have been known to cure obstinate cases, when given by accident in excessive quantities. I have collected from different sources a number of cures, said to be permanent, gained by means of narcotism produced suddenly by large doses of belladonna, opium, and aconite. The same thing has happened with strychnia. A lady who had suffered

severely for years, from facial and cervical neuralgia, took by mistake a tea-spoonful of the strongest tincture of aconite root, which had been prepared for use by friction. The immediate effects were most distressing and dangerous, her life being saved only by the prompt employment of stimulants. After her recovery she continued wholly free from the neuralgia for more than a year, after which time it returned, in a milder form.

One Dr. Chabert, of New York, proposes to only poison his patients with prussic acid, not quite dead, and then bring them to. He succeeded in one instance, if we may believe him (not only in bringing the man to, but in curing the neuralgia.) These results are curious, whether practically valuable or not.

Nux vomica and strychnia are medicines of great utility in the neuralgia of feeble persons. The former was long ago recommended by Linnæus as a cure for gastralgia. The cases it is believed to be best fitted for are those occurring in pallid, lymphatic persons, not those subject to derangement of the motor functions. Rowland thinks strychnia most valuable in intermittent neuralgia. It acts as a tonic, particularly promoting the digestion of food. It can never be administered without anxiety, and should be closely watched. I have known myself of but one instance of its having poisonous effects, when given for neuralgia. But whether from a cumulative property, or from varying susceptibilities of the system, it will sometimes break out unexpectedly, as in the case I alluded to. The first spasmodic symptoms were violent trismus, with a sense of suffocation, &c. The patient was saved, and the gastralgia she had been long suffering from, was almost perfectly cured. I suspected its employment in homœopathic practice, from tetanoid symptoms connected with some mysterious pills, in one young lady, whose disease was, however, a form of chorea.

In the *Medical Gazette*, 1840, Dr. Pidduck relates his experience with strychnia, which is important. He gives six cases, two of infra-orbital neuralgia, one sciatic, one in the arm and hand, accompanied by *loss of power*. The strychnia was given usually one-twelfth of a grain twice or three times a day, and always with benefit. In one case, relief began after the third dose, and was complete before the twelfth; the pain returned in slighter degree a few days after, and the medicine was resumed with equally good effect, after which there was no relapse. When any mechanical irritation exists, at any part of the nerve, it is not likely to do good. If I understand Dr. Pidduck rightly, he ascribes much or all of the power of strychnia over paralytic and painful diseases, to its tonic properties, and its producing more complete assimilation of food. From all the reports of strychnia in neuralgia, I should infer that it was a medicine promising much good, and particularly deserving of further trial.

Among tonics, having already spoken of the two most important, iron and quinia, I need only allude to the employment of zinc. The preparations of zinc are believed to be valuable tonics, more particularly in affections of the nervous power of motion, as chorea. The pills of Mèglin, celebrated in France as a remedy for facial neuralgia, consist of equal parts of extract of hyoscyamus, extract of valerian, and oxide of zinc. How much the zinc adds to their efficacy, I have no means of knowing; though it appears that the zinc is sometimes omitted.

The valerianate of zinc is a preparation of great value. It combines a soothing with a tonic power, and has, so far as I have observed, no tendency, like so many of the sedative drugs, to derange the secretions and impair digestion. It is found most beneficial in facial neuralgia, not presenting a regular periodical type, and not connected with rheumatism or syphilis. I have before

me the notes of three cases, in which its good effects were obvious, after many other powerful remedies had failed. I am in the habit of administering three-quarters of a grain, in pill, two or three times a day.

Turpentine has succeeded so often, in sciatica especially, that it ought, I think, to be ranked next in value to the carbonate of iron and quinia, if any general comparison could be instituted between remedies all adapted for different varieties of neuralgic disease. Of all the testimony and advice in regard to it, none is so important as that of Martinet. He settled down on a preference for doses of about twenty drops, three times a day, allowing of its absorption. When it produces diarrhoea, he finds its good influence thwarted. It produces a strong sense of heat in the affected part, as well as in the stomach and bowels, sometimes a general perspiration. This sense of heat in the part is most marked in those who are cured or relieved.

"This medicine is of approved efficacy," says one of the notices of Martinet's essay, "in all cases of neuralgia affecting the extremities, and particularly in sciatica, when this disease is simple in its character, and evinces no sign that the nerve is either altered in its structure, or compressed by the formation of a contiguous tumour. M. Martinet affirms that, whether the complaint be recent or otherwise, the chance of cure by this remedy is greatest, *cæteris paribus*, when the pain is so intense as to indicate distinctly the course of the nerve, and so obstinate in its nature as to yield to no other treatment whatever." "Twelve days," he says, "usually suffice for curing neuralgia, when it affects the extremities, and more commonly only half that time. To continue it too long would only injure the organs of digestion, if not the urinary organs."

Of seventy individuals, affected chiefly with sciatica and other kinds of neuralgia of the extremities, fifty-eight were cured, viz: three by frictions, and all the others by taking it internally; ten (two of whom intermitted the medicine) obtained only temporary relief from its use, and five received no benefit. Of these seventy-one cases of neuralgia (for one of the patients had two affections of the kind) forty were acute, and thirty-one chronic. Of the acute, thirty-four cases were cured, five relieved, and only one continued in the same state. Of the chronic, twenty-four were cured, three relieved, and four received no benefit.

Dr. Copland speaks highly of the use of turpentine in other neuralgiæ, besides sciatica. He says: "A strong recommendation of it is found in its being equally appropriate to the inflammatory and non-inflammatory states of the affection, and in the fact of relapses being much less frequent after its use than after any other remedy."

Colchicum and croton oil may both be considered as having more power over some forms of neuralgia than they owe to their mere effect as evacuants. Both act at once on the kidneys and the bowels, but they have a curative influence beyond this. Croton oil, as has been observed both by Andral and English physicians, has produced cures when given so as to have no more cathartic effect than purgatives that had been previously used in the same cases. As it has been proved that colchicum often increases the proportion of lithic acid and the lithates in the urine, and its best effects are observed in gouty neuralgia, we may infer that it acts by promoting the excretion of these elements from the blood. Croton oil also increases the density of the urine; what solid ingredients it acts upon particularly, I do not know. Professor Easton suggests some analogy as probably existing in the curative agency of these two substances.¹ This agency has not won such general confidence as many of the medicines before named.

¹ London Lancet, 1842.

External applications form an important part of the resources for treatment. Some of those which affect in the simplest manner the circulation and nervous activity near the skin, are very useful as palliatives. Pressure and rubbing are often employed instinctively. We have stated that, generally, pressure on certain radiating or focal points aggravates the pain. But in ordinary cases, not inflammatory, pressure bearing pretty evenly along the whole course of a nerve relieves it. I have more than once completely stopped a paroxysm of cubito-digital neuralgia by careful bandaging of the arm. The pain is often pressed away to another spot, and I have succeeded in thus chasing it off from the fingers' ends.

Both cold and warm applications have been found excellent palliatives, in different cases; just as with weak eyes and some forms of chronic inflammation, it can only be determined which will be most soothing, by actual trial. Compresses with cold water and ice; douches of cold water have sometimes carried off paroxysms.

Dr. Warren applies very frequently warm and hot fomentations; and the hot douche. For the latter he has a vessel of hot water, a stopcock and hose, and lets on a stream of water, say a quarter of an inch in diameter, at a temperature of 110° F., continuing it as long as the patient can bear it, which is usually for five or ten minutes. Then he applies frictions with olive or palm oil for a few minutes. He has met with much success from these applications. It is only in the severer cases that he adds to hot fomentations some narcotic plant, as conium, hyoseyamus, or hops.

Rubefacients—the ginger or mustard poultice—deserve their reputation in tic douloureux. I have seen a paroxysm completely cut short by a weak solution of ammonia applied to the cheek, producing most abundant flow of tears.

Blisters are among the most valuable remedies. Dr. Warren says they are perhaps the most frequently useful of all. They should not be kept open long, or allowed to run into suppuration, but fresh ones should be applied from time to time. Two modes of application have been most successful. One is, by placing rather long blisters along the track of the nerve, and the other consists in putting small repeated ones over the uppermost point affected, or that which serves as the radiating centre of pain. It is in this respect that Valleix thinks his investigation of the "focal points" of neuralgia will be useful in guiding our treatment. He relates a number of cases, some of them highly interesting and striking in their results, in which a succession of small blisters over these points was employed. The plan is stated to have produced a cure in many cases, and to have always given great relief. Blisters are also very beneficial in inflammatory cases, after the acute stage has passed. Fuller recommends them, when symptoms of effusion exist, the active signs of inflammation having disappeared—and advises long and narrow blisters, along the course of the nerve, to be dressed with mercurial ointment, to aid in obtaining the constitutional effects of mercury, or with powdered morphia, to relieve severe suffering.

In the obstinate affections, sciatica especially, issues and the moxa at times succeed, when nothing else will. A distinguished physician of this city relies mainly on issues made with Vienna paste, over the upper part of the ischiatic nerves in obstinate sciatica. When the disease shows a disposition to change place from a nerve it has long occupied, we may favour this disposition by making an issue on the part it seems inclined to invade. This is illustrated by an interesting case in Dr. Rowland's Essay, where facial neuralgia of the severest kind had lasted nearly twenty years, when the pain showed an inclination to attack the shoulder, and an issue applied there cured, almost com-

pletely, for two years at any rate, the excruciating affection of the face, while a slight neuralgia persisted in the new seat.

Many of the sedatives are used externally with even more advantage than internally. We have already alluded to the fomentations of hyoseyamus, &c., which have often been very beneficial in the hands of Dr. Warren—and also to the use of belladonna in plaster, over the spot of spinal tenderness. This last I have also found exceedingly serviceable in many simple neuralgiæ, as also chloroform, by itself or combined with camphor in a liniment.¹ In many country districts, where belladonna grows abundantly, poultices made by boiling down together the roots and leaves of the fresh plant, have attained much celebrity, both in rheumatic and neuralgic affections. The narcotic effects often follow this mode of employment. We are assured by a physician of such a region, that this treatment is often successful without any internal medicine. Permanent cures are effected, within a fortnight, of facial neuralgia that had existed for years. Ointments and watery solutions of belladonna are reported, by Dr. De Brayne, Dr. Todd, and others, as having been successful in producing permanent cures. Dr. Lombard has given very satisfactory results from the use of lotions of hydrocyanic acid and its compounds; in neuralgia of the more superficial nerves, not complicated by inflammation.

Aconite, its saturated tincture, and alkaloid, possess an extraordinary numbing power, substituting for acute pain some disagreeable tingling sensations. In my own observation, I have not found them so useful palliatives as chloroform, and can judge of any curative power they may have only from books, which give cases treated by so many other means at the same time as to have little real scientific or practical value. In some of Dr. Fleming's reports, the drug was only used in this mode. One crural neuralgia, confined to a spot near the patella, had existed three years, and been relieved by nothing but the potential cautery; it was cured in eight days by the external application of the tincture of aconite. This application has sometimes produced the general symptoms of poisoning by aconite, and should be used with great care, particularly if there be any abrasion of the skin.

Of both aconite and veratria, the results have not been near as favourable in other hands as they were represented by Turnbull, and some others who first brought them most prominently forward. Pereira, Brodie, Rowland, all agree in this statement. Pereira found the veratria generally fail, and considered it far inferior to aconite. Brodie employed it in several cases without any advantage. Rowland never obtained permanent relief from it in any case of long standing. He says the first friction with an ointment of veratria often gives very great relief; perhaps this continues a good deal longer than the usual intervals of ease, but the pain keeps recurring and the intervals of ease keep shortening, until they hardly outlast the unpleasant prickling occasioned immediately by the application. Atropia, the alkaloid of belladonna, has cured an obstinate facial neuralgia, an ointment of five grains in three drachms of lard being rubbed in a small piece as large as a pea, three times a day.

The endermic application of narcotic and anodyne substances deserves to be more used than I have ever known it to be in this country. A blister should be raised, better by ammonia than cantharides, and the exuded coagulable matters should be removed at each application of the drug, since they would interfere with absorption if allowed to remain. The substance applied should be in the form of powder, or perhaps watery solution, not cerate. Fuller mentions that the salts of morphia give much more pain than pure powder of

¹ Cazenave employs a "pommade," containing chloroform ʒiv , cyanide of potassium ʒiiiss , lard ʒiiij .

morphia. The acetate, however, has been most employed. The place of application should be well chosen—the same as in the ordinary use of blisters for neuralgia, along the track of the nerve, and at a point where this nerve is superficial, and especially one of the *focal points*.

M. Magistel published his results obtained by the endermic use of acetate of morphia, about twenty years ago. His were cases of hemicrania and neuralgia of the face. His success was great, but Trousseau since thought he gained great advantage by substituting the more soluble salts, the sulphate and muriate.

An interesting paper in the *Lancet*, 1846, contains some successful cases, treated by Dr. White, of Newcastle. The following are some remarks of the reporter:—

“This mode of treating pains which appear to depend principally upon nervous irritation, or it may be chronic inflammation, and are confined in their extent, has been extensively employed here, and in no cases has it been attended with any disagreeable effect; but, on the contrary, with the most surprising benefit in many, or, I might say, in most of those patients who, from their condition, seemed to be suitable for its application. The pain is generally soon relieved, and a comfortable feeling of ease, with sometimes a little drowsiness, is experienced. In fact, in sciatica, or indeed in any case where the pain can be distinctly traced along the nerve, this plan is often the only one that affords even partial relief. The application must be used as near the trunk of the nerve as possible, particularly where the pain most nearly centralizes. It sometimes requires to be reapplied nearer and nearer the terminating branches, as the pain is gradually expelled. Hence, in sciatica, the nates first, then the knee, and lastly the foot, sometimes in succession, require the vesication, and subsequent sprinkling of the morphia. It is quite certain that this treatment has entirely succeeded when leeches, blisters, moxas, belladonna externally, &c., had entirely failed.”

I regret that the interesting narrations now before me, of old cases of neuralgia in the sciatic and trifacial nerves, treated with great sagacity and happy results, and very well reported, by M. Mondière, cannot be copied into this essay, with their suggestive details. Some of them were of the most exquisitely acute form; some had tortured the patients for years, and resisted the greatest variety of scientific treatment; and relief was obtained in two or three applications, increased day by day, and the results as to permanent cure were most happy. His cases were published earlier than the investigations of Valleix; and it is interesting to observe that his best successes were obtained by vesication and the sprinkling of morphia over points, exactly such as this later observer has indicated.¹ In these cases no internal treatment seems to have been employed at the same time, though the cures were afterwards followed up and confirmed by tonics, or other management as required. The inoculation of solutions or pastes containing morphia has very often failed, and deserves to be mentioned rather for curiosity. Its practical advantages over the application to a blistered surface do not appear, unless in a slight saving of pain, while its success is much less. Dr. Rowland obtained good results from it in but one case, out of “at least twenty.” But I have been much impressed with the success of the “endermic” treatment as above described; and cannot but think it more deserving of trial than almost any other measure we have been considering, which is at all novel.

After reading a good many papers, on the use of electricity and galvanism in neuralgia, the result is that I am a good deal disappointed in the accounts

¹ Répertoire de Clinique, &c. 1836.

of their efficacy as means of cure. In the *London* and *Edinburgh Journals* are some communications exhibiting facts of interest. M. C. James attributes the failures to the imperfect application—recommending *electro-puncture*, the needle (of platina or gold, and fine) carried to the nerve itself, not necessarily deep enough to transfix it. Needles are inserted near the origin, and near the end, of the affected nerve, and a current passed between them from a voltaic pile of say five or six plates, continued for a few seconds only, the positive pole being made to communicate with the needle towards the origin of the nerve.

M. Hermel has reported cases of cure by electro-puncture, almost all of lumbo-sacral and sciatic neuralgia, sometimes accompanied by partial paralysis. His conclusions are thus stated:¹ “1. Electro-puncture is applicable to idiopathic or essential neuralgias. 2. The violence of the pains is not a counter-indication to the employment of this therapeutic agent; they have never in any case been aggravated by its use.² 3. The paralysis which supervenes in the progress of idiopathic neuralgias yields to the same treatment.” It may be mentioned, for the gratification of our homœopathic friends, that Halliday knew of a case aggravated by it, and Bérard, of Paris, gave himself a neuralgia by experimenting with acupuncture for the purpose of applying electricity.

Some successes have been obtained by the electric spark and by galvanism. The latter has been used by Dr. Thomas Harris, with the result of curing five out of seven cases, but only after intervals of from ten days to five weeks. Among various affections which I have known to be somewhat relieved by the use of galvanic rings, was one of sciatica, of thirty years' standing. The relief coincided with the appearance of an eruption of an eczematous character, which spread from the point of application down to the leg, and was more troublesome than the sciatica itself, while it lasted. After four or five weeks the eruption was nearly healed and the sciatica returned, but much mitigated in severity. I am also acquainted with ladies who count confidently upon cutting off an attack of neuralgia by applications of the wires connected with the galvanic pile. Their frequent resort to it shows that it has no curative powers for them.

The effects of all these agencies are not so well understood that we can safely foretell their operation in any particular case. Empirically employed, with the slight indications we can gather from our imperfect knowledge, their workings appear to be capricious. The words of Pereira, applied to the magnet, have too much truth as an expression of our experience with these other powers. “In some instances it has appeared to exercise a most remarkable influence over neuralgic pains and spasmodic affections; at one time apparently curing, at another palliating, and occasionally augmenting all the patient's sufferings. But, in a large proportion of cases, it fails to produce any obvious effect.”

It remains to speak of surgical operations for neuralgia, and we shall do it briefly. Besides operations already spoken of as called for by the known existence of a scar or other local source of irritation, surgery has been called to render her aid empirically, in cases which resist other modes of treatment. Three principal operative procedures have been performed, all with occasional success, namely, section of the nerve, excision of a part of its length, and cauterization. Mere division of the nerve generally gives ease only for a time, pain returning in a few days sometimes, oftener a few weeks. The case of Dr. Haighton, who performed this operation on his own mother, is commonly cited among the instances of success; but it appears that the pains reappeared

¹ Quoted from Velpeau, in Copland's Dict.

² A mistake.

after a long time.¹ Cases of reported success by this mode are numerous, but those of failure far outnumber them.

Excision and cauterization count a much larger proportion of favourable results. Dr. Warren has frequently practised the former on parts of the tri-facial nerve. He exposes the nerve, passes a curved knife under it, raises it and cuts out half an inch or so of its length. He then lets the wound heal immediately. He believes that this operation would be generally successful, if performed within a few weeks of the origin of the disease. It should be done, in his opinion, as soon as other modes of treatment have been tried and have failed. By longer delay, "the organic habit" of pain is set up, and the whole course of the nerve becomes diseased. He has observed several instances where the paroxysms did not disappear at once, but the pain abated only slightly for a while, and then either continued to diminish gradually, or else disappeared suddenly, after a time. "After the paroxysms have been cured," says he, "exposure to cold on the occurrence of an injury has reproduced them in the same or in neighbouring nerves."

Cauterization has been thoroughly accomplished by first cutting the nerve across, and then touching the ends with nitrate of silver, or a small heated iron. It is considered by Boyer the most effectual operation of all, as "it destroys not only the whole substance of the nerve for a certain distance, but it also attacks all the nervous branches proceeding from it through a considerable extent, which as they may participate in the disease, might be sufficient to keep up the pain, after the resection of the principal trunk." In the detailed accounts of cases, cauterization is not mentioned as having been followed by the same partial relapses and remnants of disease as Warren has observed after excision. In one case of final success, however, dangerous general symptoms, convulsions, &c., occurred.

The successes of either excision or cauterization surpass those of mere section of the nerve. Between the first two, the choice must be determined, not so much by any general law, as by the convenience of each case; in particular the depth and size of the nerve, and the importance of avoiding deformity in the situation it happens to occupy. The difficulty of finding and isolating the nerve, so as to be sure of dividing it, and nothing else with it, is spoken of by Boyer as very great, and a serious objection to the operation by excision.

It seemed to me more convenient, though less logical, to consider the different modes of operating first, and the question of the expediency of any operation afterwards, inverting the order which we should follow in practice. It is certain that comparatively slight operations have sometimes gained long respite, in a disease which is one of the most painful we are acquainted with. It is a disease which has led several sufferers to submit, for the uncertain chances of relief, to repeated amputations in the large joints, even when the disease had first originated from amputation, and before anaesthesia had charmed away the shadows of fear and agony from the atmosphere of the operating-room. On the other hand, we know that many forms of neuralgia tend to travel from one nerve to another, and seem more disposed to invade a new region when they are dislodged from an old haunt; that many spring from a constitutional cause; that others depend on disease in the nervous centres, the presence or absence of which we cannot always ascertain; and that in the limbs we have motor and sensitive fibres in the same nervous trunk, not separable in an operation, so that a permanent loss of function paralyzes the muscles it supplies, and would cause the greatest inconvenience, if it occurred in the main nerve

¹ Rowland, p. 90.

of a limb. Yet the sciatic nerve has been cut down upon by Malagodi, and eighteen lines in length of it were cut out, with the effect of leaving a paralysis of the leg and foot, instead of the neuralgia.

To show that there is room to question in a general way the utility of operations for neuralgia, let me once more refer to the highest surgical authorities. Dr. Mott's views are stated as follows by Reese, in his *Notes to Cooper's Surgical Dictionary*, 1839:—

“Dr. Mott has adopted the practice of dividing the nerve in almost every case of neuralgia, where it is practicable. He has repeated this operation, on the infra-orbital, mental, and other nerves, so frequently and with so great success that he confidently recommends it to his pupils and patients.” In Dr. Lee's additions to *Copland's Dictionary of Practical Medicine*, 1847, is a sentence apparently referring to Dr. Mott's opinions expressed in his *Lectures on Neuralgia*, 1842, and containing these words: “After extensive trials he laid the operation aside, as he found the disease very certain to return as severe as ever.”

Brodie says of the operation for facial neuralgia: “It is altogether an unscientific operation, from which we have no more right to expect benefit than we should have from the amputation of the testis, in a case of pain referred to that organ, in consequence of a calculus being lodged in the urethra.” We may urge in reply: 1. That the analogy does not hold between an irritation temporarily reflected from one organ to another, and a fixed affection, always haunting one particular nerve. 2. That the operations have frequently succeeded, at any rate, in obtaining a welcome respite of months or years.

Many points pertaining to the general management, or regimen of neuralgic patients have come up incidentally in this essay. Their modes of living ought in all cases to be regular, the diet simple, generally nutritious and compact, except in cases of plethora or inflammation. Abstinence from tea and coffee ought always to be enforced, at least long enough to test its effect. The habitual use of these nervines keeps up a morbid sensitiveness of the nervous system. I have observed this particularly in regard to strong tea used by feeble females. With some of this class, it is unfortunately impossible to avoid a confining and toilsome life, often in confined air and constrained position, bent over the monotonous labours of the needle. Thoughtfulness and kindness on the part of employers must here be looked for to accomplish what science can only indicate. An early hour of going to bed and rising should be observed. The mind should be preserved from overstraining and from trying excitements. Worrying, and particularly angry feeling, are a frequent exciting cause of paroxysms. Regular exercise of the different muscles should be insured, as systematically as is consistent with cheerful employment of the mind and feelings. A few cases have been cured by exchanging an inactive life for one of fatigue. Pinel mentions a gentleman cured at last of sciatica, by becoming a soldier. Another patient got rid of a facial neuralgia by aid of the practice of boxing, carried often to the point of great fatigue.

In quoting often from writers of reputation, I have been influenced only by a desire to convey, in the simplest and surest way, the results of varied observation. When men of sagacity and large experience have given us their views, in regard to the disease in hand, or to the several collateral subjects, which each man has studied separately more thoroughly than one man can study all, we may well borrow their statements, not indeed to adopt them blindly, but with all the deference due to practical knowledge and mature thought.

REVIEW.

ART. XIV.—*Trial of John Hendrickson, Jr., for the Murder of his Wife by Poisoning, at Bethlehem, Albany County, N. Y., March 6, 1853.* Reported by Messrs. BARNES & HEVENOR. Albany, 1853. Svo. pp. 176.

THIS report contains the history of one of the most interesting and important trials in the records of medical jurisprudence in our country. It is the first, and we believe the only instance in which the question of poisoning by *aconite* has been brought before our courts of law; and the facts elicited on the trial, though of a negative rather than a positive kind, are of great importance in legal medicine, and deserving of permanent record.

The present report has been published under the sanction of the counsel who were engaged both in the prosecution and defence of the deceased, and may therefore be regarded as a correct and authentic document. It contains, in addition to the testimony elicited upon the trial, the arguments of the counsel, the charge of the judge to the jury, and the sentence of the court, after the verdict of guilty was rendered by the jury.

A brief history of the case will be necessary in order to appreciate the nature of the testimony, on which we design to make a few comments.

The accused, John Hendrickson, Jr., a young man twenty years of age, born of respectable parents, married, at the age of eighteen, Maria Van Dusen, a young lady of seventeen, also of highly respectable connections, well educated, amiable, and intelligent. There is no evidence that they lived together unhappily; and although the District Attorney, in his opening speech, spoke of the prisoner having communicated syphilis to his wife, there is no satisfactory proof that such had been the case; but, on the contrary, it is quite evident that she had laboured under a severe form of leucorrhœa. On the night of the 7th of March, 1853, after attending church in the evening, she retired to bed with her husband at her father-in-law's, between ten and eleven o'clock, complaining of a severe pain in her head, hips, and loins, and at two o'clock was found dead by her husband, "occupying nearly the centre of the bed, lying at full length on her back, with her hands either crossed, or lying down by her side, the bedclothes covering her person." A coroner's inquest was held the same evening, and a *post-mortem* examination ordered, which was made thirty-six hours after death by Dr. J. Swinburne, Dr. Ingraham, and the coroner, Mr. Smith, being present. Four days after, the body was disinterred, and further *post-mortem* dissection made, the first, for some reason not specified, having been incomplete. The principal appearances noticed are the following: Great pallor of the surface; calm expression, and no distortion of features; sugillation on the posterior parts of the body; great rigidity and *elasticity* of the muscles; lungs and heart healthy; heart empty of blood, except a small clot in the right auricle; the vena cava partly full of dark fluid blood; stomach and small intestines empty; liver healthy; gall-bladder half full of bile; mucous coat of stomach very red, lined with a thick reddish mucus, corrugated, the stomach itself contracted to two inches in diameter, and its coats hypertrophied; the mucous

coat of duodenum also corrugated, and more congested than that of the stomach; uterus enlarged to twice its natural size, hardened, and its cervix slightly ulcerated, with adhesions to the rectum and small intestines; ovaries considerably enlarged; spleen and pancreas healthy; urinary bladder contracted; brain healthy, no congestion; tongue white, and a little swollen; a small ecchymosed mark on inside of lower lip, showing a cut about a quarter of an inch in length (throat, œsophagus, spinal cord, and lower portion of intestinal canal not examined). At the next examination, five days after, the kidneys were found healthy, feces in the cæcum;—portions of lung, liver, and pancreas, with four ounces of blood from cavity of chest, were removed, placed in jars, and carefully covered.

To the question, on the subsequent trial, "What was the cause of death?" Dr. S. replied:—

"Acrid poison. I base it on this: I find entire émpitness of the stomach and small intestines, so far as fecal matter is concerned; also contraction and corrugation of the same to a great extent. I find in place of that a reddish *viscid* mucus adhering to the coat of the stomach and intestines; the emptied condition of the gall-bladder; the appearance of the tongue. *I inferred from these that vomiting had taken place*, and that, too, induced by some acrid matter, which would not only expel the contents of the stomach, but of the small intestines, the presence of which acrid matter would induce the vomiting."

To the question, "What would induce your belief that she vomited?" the witness replied:—

"I believe the act of vomiting is accompanied by more or less contraction of the stomach; where that act is induced by the presence of acrid matter, *the contraction will be proportional to the material used, be it more or less irritating.*" "The corrugation would be owing, in part, to the contraction of the muscles, and part to the irritating matter applied to the mucous surface." "The rigidity of the body presented the appearance of a person destroyed by anything *which would produce a sudden spasm or contraction of the muscles.* The appearance of the stomach and intestines proved to me *conclusively* that she had vomited, and to such a degree as could not be produced by ordinary causes; and I think the effort at vomiting continued until exhaustion took place. My reasons are these: *the blood, in the first place, was thrown from the centre to the surface; also, the extreme pallor of the countenance, which always attends exhaustion from vomiting.*"

To the question, "Will you state, in your judgment, from what poison she came to her death?" Dr. Swinburne replied:—

"*I suppose the deceased died from aconite, from the fact of the appearance on the post-mortem examination being so identical with those of the dogs and cats,*" (experimented on by Dr. Salisbury with tincture of aconite.)

The counsel then asked: "What is the strength of your opinion, that she died of poison?" Dr. S. replied:—

"*I have no doubt of it; I have no doubt that she vomited; one of my reasons for thinking she died of poison, was her having vomited, and also the absence of congestion. Had Mrs. H. died of natural causes, more or less natural contents would have been found in the small intestines, and they would have presented a healthy appearance, relaxed instead of contracted; also, the circulating system would have presented a different appearance.* In all forms of death by asphyxia, syncope, apoplexy, epilepsy, and all the other forms of convulsive diseases, you have fulness or engorgement of the heart and all the important viscera." Again: "From my reading, knowledge, and experience, I am prepared to express an opinion as to what caused the morbid condition of the stomach and bowels—*it was aconite, or the principle of aconite.* Had the poisonous matter passed off with any of the fecal matter, I think the *rectum* would have pre-

sented the same appearance as the intestines, only in a less degree; can assign no possible way, except by vomiting, how the fecal matter could have been removed."

"What external appearances would you look for, where death has been caused by aconite in three or four hours?"

"Should expect to find rigidity of all the voluntary muscles; also, extreme pallor from vomiting."

The testimony of Drs. Ingraham and Coroner Smith, though lacking the positiveness of that of Dr. S., just given, is of similar tenor, so far as regards the *post-mortem* appearances. Both express the opinion that death was caused "by some acrid substance introduced into the stomach." Both also were willing to swear, from the appearance of the stomach, that severe vomiting had taken place previous to death. Dr. Smith, however, was inclined to think that death was occasioned by the influence of some poison on the nervous system.

We hardly dare trust ourselves to comment on the above testimony as it deserves; for, if there is any principle established in legal medicine—one in which all pathologists agree—it is, that no positive proof of poisoning can be derived from the *post-mortem* appearances, either in the internal or external parts of the body.

"Any evidence," says Taylor, "derivable from the appearances in the body of a person poisoned, will be imperfect unless we are able to distinguish them from those analogous changes, often met with as the results of ordinary disease. These are confined to the mucous membrane of the stomach and bowels. They are redness, ulceration, softening, and perforation; all of these conditions may depend upon disease, as well as upon the action of irritant poisons."¹

So also Guy: "The symptoms and *post-mortem* appearances produced by poisons are not peculiar to them, but may be produced by natural causes, and form a part of common diseases."²

And yet Dr. S., on his cross-examination, states that "he never read in any work on medical jurisprudence that a physician should not give an opinion of the death of a person from poison from the mere appearances on *post-mortem* examinations;" and thought that "a prudent person would express such opinion; and that a person could give evidence of death by aconite merely from the inspection of the person after death!"—also, that "inflammation never takes place except from the presence of some irritant!"

With regard to the degree of "congestion" of the mucous membrane of the stomach, &c., the sum of the medical testimony is as follows: Dr. Smith remarks, that he "did not examine the bowels or duodenum particularly, only they appeared smaller than usual." Dr. Ingraham speaks of a "red-den appearance of the folds of the stomach." If the redness of the mucous surface had presented anything very remarkable, it would probably have been brought out more prominently. Dr. Swinburne's testimony is, that "the vessels were so filled that the mucous surface looked very red, and the mucous coat was lined with a thick reddish mucus." Dr. S. regards the redness as simply "congestion, and not inflammation." There were about two ounces of reddish mucus in the stomach, which was hypertrophied, but its villous coat less congested than that of the duodenum; paler, but more corrugated than the latter. The duodenum was empty, and smaller than natural. The remainder of the intestinal canal was not examined till four days afterwards, when the body was exhumed.

¹ Taylor on Poisons, Am. ed. p. 100.

² Med. Jur., Am. ed. p. 544.

Admitting, then, that there was an unusual degree of redness or congestion of the mucous membrane of the stomach, it is now well established that it is not uncommon to find such appearances, not dependent on the action of poison or any assignable cause. Dr. Taylor, in his work on Poisons (p. 101, Am. ed.), remarks that—

“A person may die without suffering from any symptoms of disordered stomach; but on an inspection of the body, a general redness of the mucous membrane of this organ will be found, not distinguishable from the redness which is so commonly seen in arsenical poisoning. Several cases of this kind have occurred at Guy’s Hospital; and drawings have been made of the appearance of the stomach, and are now preserved in the museum collection. A record has been kept of four of these; and it is remarkable that, although in not one of them, before death, were any symptoms observed indicative of irritation or disease of the stomach, yet in all, the stomach was found more or less reddened, and in two exclusively so. Such cases are only likely to lead into error those who trust to this *post-mortem* appearance alone as evidence of poisoning; but no medical jurist, aware of his duty, could ever be so misled.”

He then gives a case of a young woman far advanced in pregnancy, who died suddenly in a fit of syncope, and where, after death, “the mucous membrane of the stomach was reddened, and thrown into rugæ.” There is also an interesting case recorded in the *Annals of Hygiene*, 1835, vol. i. p. 227, where it is probable that this pseudo-morbid appearance of the mucous membrane was mistaken for the effects of irritant poison. Dr. Yellowly has also shown very clearly that the mucous membrane of the stomach often presents a high degree of vascularity and redness in cases of sudden death (*Med.-Chir. Trans.* 1835). He met with this appearance, as we have also in the stomachs of executed criminals, &c., and after presenting a great array of facts, adduces from them the following conclusions: 1. That vascular fulness of the lining membrane of the stomach, whether florid or dark-coloured, is not a special mark of disease, because it is not inconsistent with a previous state of perfect health. 2. That those pathologists were deceived, who supposed, from the existence of this redness in the stomach, that gastritis sometimes existed without symptoms. 3. That erroneous conclusions as to the cause of death are frequently owing to the same mistaken observations, the effects of putrefaction and spontaneous changes induced by the loss of vitality, being sometimes attributed to the action of poisons. 4. That the vascularity in question is entirely venous, the florid state of the vessels arising from the arterial character of the blood remaining in the veins for some time after its transmission from the arterial capillaries at the close of life; the appearance is, however, sometimes due to transudation only. 5. That the fact of inflammation having existed previously to death, cannot be inferred merely from the aspect of vessels in a dead part; there must at least have been symptoms during life. These positions are now also maintained by Andral and the best French pathologists, as well as those of Germany and this country; so that redness of the mucous membrane of the stomach and intestinal canal can no longer be regarded as proving the past existence of inflammation, unless there have been symptoms during life, or other marked effects of the inflammatory process in the alimentary canal, or the discovery of the poison itself. We may also, in this connection, refer to the cases recorded by the late Prof. W. E. Horner, in the first volume of this Journal, for evidence of the same fact. Prof. Horner was one of the first to prove that there may be great congestion of the mucous membrane of the stomach, abundant mucus, and great corrugation and contraction, without previous inflammation, and where death has resulted from other diseases. In one case, where death was sudden, and no

suspicious of poisoning, he found the gastric coats thick and dense, the mucous coat thrown into numerous folds, or well-marked, elevated rugæ, and almost universally of a deep arterial red. The red corpuscles were extravasated in numerous spots and blotches. Dr. Beek also remarks:—

“MM. Nigot and Trousseau, and M. Billard, have pursued the investigation of this subject to a great extent. The former have proved by experiment, that various kinds of pseudo-morbid redness may be formed, which cannot be distinguished from the parallel varieties caused by inflammation; that these appearances are produced after death, and not until three, five, or eight hours after it; that they are to be found in the most depending parts of the stomach, and turns of the intestines; and that, after they have been formed, they may be made to shift their place, and appear where the membrane was previously healthy, by simply altering the position of the gut.”¹

It is very evident then that redness, quite equal in degree to that observed in the case of Mrs. Hendrickson, is no uncommon appearance in *post-mortem* examinations, and the redness would seem to be intense in proportion as the death has been sudden and the circulation active. The redness in the present instance was no greater than is usually met with in cases where death is as sudden. The “reddish mucus” in the stomach may, perhaps, be satisfactorily accounted for from the presence of the colouring matter of tomatoes, which the deceased ate freely shortly before her death. The microscope might have settled this question definitely.

The other phenomena mentioned, on which considerable stress appears to have been laid, viz: pallor of the surface, and rigidity, and elasticity of the muscles, &c., are even less significant or characteristic of poisoning than the slight congestion already noticed. Neither can be considered as indicative of modes of death; their absence, indeed, might be worthy of note in a suspected case, but not their presence. In all cases of death from irritant poisons, especially the narcotico-irritants, which we have had an opportunity of observing, there have been crimson or livid-coloured patches on various parts of the body, and considerable tympanitis, though we cannot affirm that these phenomena are invariably present in such cases. There has also been considerable bloody, frothy mucus in the mouth, fauces, and œsophagus, especially when vomiting has been severe. No such phenomena were noticed in the present instance; only we are told that the tongue was “swollen, and very white,” an appearance not particularly indicative of gastric irritation. It is also worthy of note, that the “pallor” is attributed to the “blood having been thrown from the centre to the circumference, by vomiting,” which we should suppose would have the opposite effect.

“The rigidity of the body,” says Dr. S., “presented the appearance of a person destroyed by anything which would produce a sudden spasm or contraction of the muscles.”

Here it is assumed that the deceased died from spasm, and that the spasmodic state of the muscles continued after vitality was extinguished (thirty-six hours after death). No allusion is made to the *rigor mortis*, as a common cadaveric phenomenon; but it is claimed throughout the direct testimony, that the stiffness of the body was owing to some poison which had caused severe spasm; and because the witness had noticed the same rigidity in dogs, destroyed by aconite, he does not hesitate to express the opinion that the deceased came to her death from the same poison. Now, cadaveric rigidity takes place in all classes of animals alike; coming on as soon as muscular irritability ceases, confined to the muscular system in many cases, giving extreme rigidity to the

¹ Med. Jur. vol. ii. p. 310.

body, its degree and duration, *cæteris paribus*, being directly as the muscular development; continuing longer the later it occurs, and *vice versâ*; influenced greatly by the nature of the disease, or the cause of death; appearing more speedily, and lasting a much shorter time, when death has occurred from some chronic wasting disease, as phthisis, fever, scurvy, &c., coming on slowly and being strongly developed, and lasting often for several days, when the death has been occasioned by acute inflammation of the stomach, or by irritant poisons. We have known cadaveric rigidity continue four or five days after death from cholera. Any one conversant with these facts, would hardly dare derive any positive conclusion as to the cause of death from the presence of muscular rigidity. We dismiss it, therefore, with the "pallor" and "congestion," as wholly insufficient to justify the conclusions deduced from it.

Considerable stress is also laid by the witnesses, on the "corrugation and contraction of the stomach;" but these are so often met with in death from natural causes, that no significance can be properly attached to them. Prof. Horner has shown that *rugæ* of the stomach are quite common in *post-mortem* examinations, and that they appear in cases where no stimulants or irritants have been applied, and in stomachs perfectly healthy, and that they are more frequently met with in cases of sudden death; and yet it is assumed, in the case of Mrs. H., that these *rugæ* were owing to the *astringent* properties of aconite, which has heretofore been regarded as a *paralysant*, and not a stimulant.

We do not hesitate to say that aconite has no power to "shrivel vegetable membranes," as claimed upon the trial; though it will destroy vegetable life; the subsequent "shrivelling" is doubtless the result of the evaporation of water, or drying.

The diminished caliber of the stomach and intestinal canal, was in all probability the result of sudden death, leaving the organic contractility of the muscular fibre unimpaired.

Extensive experiments have been made with aconite upon the lower animals; but corrugation of the stomach has never been claimed as one of its *specific* effects. It was never observed by Orfila, or by Fleming. The presence of two ounces of viscid mucus in the stomach, is regarded by Dr. F. as positive proof that an irritant had been swallowed, and yet Dr. Horner gives cases (*loc. cit.*) where the same amount of similar mucus was found in healthy stomachs. It is not unusual to meet with considerable quantities of mucus in the stomach, after death from various diseases; nor was its colour by any means remarkable under the circumstances; and yet the witness states that he "had never seen viscid mucus in a stomach after death from natural causes," nor "clinging to the coat of the stomach." The deceased had eaten little or nothing during the day preceding her death (Dr. S. says ten or eleven hours), and speaks of the empty condition of the stomach and small intestines as a remarkable circumstance, and one which proved conclusively that severe vomiting had occurred previous to death. "I believe," says he, "that all the contents of the stomach and small intestines could be thrown up, and not a particle or trace be discovered in any part;" "the quantity in a healthy person would be from a pint to two quarts;" and "this might be thrown up in an hour." To another question he replied: "A healthy stomach might expel its contents in about three or four hours;" and "considerable portions of the fecal matter would remain in the small intestines twenty-four hours."

Criticism on such statements is a work of supererogation; according to Beaumont, the *longest* time required for digesting any substance, in the stomach of St. Martin, was five and a half hours, the average less than three. Food does

not long remain in the duodenum, and the *jejunum* is so called, because it is generally empty. It would have been strange, indeed, and contrary to the usual course of things, if anything had been found in the stomach and small intestines. Had the deceased eaten supper at six o'clock (and there is no evidence that she ate any dinner or supper, but very satisfactory proof that she did not, to any amount), it would not have been strange, death occurring eight hours after, to find the stomach and small intestines empty.

It was proved by at least two witnesses, that there were no evidences of vomiting in the room where the deceased slept, when found dead, there being but one vessel in the chamber, and that half filled with urine alone. There were also several persons sleeping in the house; but no one heard any noise from vomiting, or any other cause. The inference, then, that the empty state of the stomach, &c. (two ounces of mucus excepted), indicates previous vomiting, is just as unfounded and unsustained by facts as the former. Dr. Salisbury testifies that

"The *stomach* was much contracted, and drawn into folds and ridges; not more than half as large as the stomach in its natural state; much congested; the mucous coat covered with a whitish substance resembling viscid mucus, and this was covered with the blood." "The *duodenum* was contracted longitudinally and transversely, highly congested, the inner coat covered with viscid mucus, mixed with a slight quantity of a whitish matter, resembling chyme, and this was mingled with blood." "The *jejunum* was in a high state of congestion, contracted, its mucous coat covered with mucus, and a whitish substance resembling chyle, tinged with blood, the contraction less than in the duodenum."

The appearance of the *ilium*, Dr. S. states, was similar to that of the other small intestines, while the *cæcum*, colon, and rectum, were half filled with fecal matters, those in the rectum being "dry and hard." Dr. S. inferred that there was a "*tendency to purging*, because the fecal matter in the *cæcum* was thin and watery;" also, that vomiting had occurred from "the contracted condition of the stomach, its emptiness; the emptiness of the small intestines, and the high state of congestion and effusion of blood in these organs." "Had Mrs. S. taken her usual meals on *Saturday*," says Dr. S. (death occurred between one and two o'clock on Monday morning), "I should have expected to have found food in the small intestines."

Comment on this evidence is altogether unnecessary. Why the mucus in the *ilium* should be more deeply tinged with blood than that of the stomach, we are not informed. No one, on reading the evidence, can doubt that the stomach and small intestines were empty in obedience to the laws of their economy; the natural result of the performance of their healthy functions.

The gall-bladder was half full of bile, which, as we believe, is entirely inconsistent with the idea that severe vomiting had occurred. Dr. Swinburne says: "The gall-bladder does not become necessarily empty by vomiting; it has no peristaltic action."

Such violent vomiting, however, as is claimed to have occurred in this case, must, by compression, have forced all the bile from its cyst.

In short, the *post-mortem* appearances do not justify the inference that Mrs. Hendrickson vomited during the last hours of her life. This belief has been expressed by all the physicians, whose opinions have been sought for, including Prof. A. Clarke, of New York, A. March, P. Van Olinda, M. F. Cogswell, T. Hun, J. H. Armsby, J. P. Boyd, B. P. Staats, and T. R. Beck, of Albany. The New York Pathological Society, by a unanimous vote,

"*Resolved*—That the statements made by Dr. Swinburne, as printed in the *Report of the Trial of Hendrickson*, by Barnes and Haverhor, Albany, 1853, con-

cerning the *post-mortem* appearances, as described by him in the case of Mrs. Hendrickson, in nowise justifies the opinion that death was produced by vomiting, or was caused by the administration of aconite; such appearances, especially those relating to the condition of the stomach, being often found in *post-mortem* examinations where no vomiting had occurred, and where no aconite had been taken before death.

“*Resolved*—That the *post-mortem* examination, as detailed by Dr. Swinburne, is faulty, wanting in detail as regards the condition of several important organs, and omitting to examine altogether the trachea and larynx, affections of which are known to produce sudden death.”

The spinal cord was only examined down to the second cervical vertebra, and it is well known that death sometimes suddenly occurs from the rupture of a bloodvessel within the spinal canal.

In this connection, it will be appropriate to consider briefly the properties of aconite. Belonging to the natural family *Ranunculaceæ*, the *Aconitum Napellus* has generally been supposed to participate in the acrid properties of that class of plants, and hence it has been usually described as an *acrid narcotic*. It is now, however, well ascertained, that the aconite has very feeble, if any, acrid properties, the effects, such as nausea, and vomiting, &c., commonly produced by it, being occasioned by its violent action on the nervous system. Christison says its *acrid* powers are “doubtful or feeble.” Pereira and Fleming do not admit that it possesses any. Taylor and Thomson also coincide in this opinion. Its peculiar effect upon the organs of taste, such as tingling, numbness, sense of heat, &c., Dr. Fleming shows to be a property belonging to its narcotic or sedative principle, and the measure of its activity as a poison. The aconite is not a stimulant, but a pure *paralysant*.¹

The importance of a correct knowledge of the properties of plants, is well illustrated in the case of Mrs. Hendrickson. Had it not accidentally come to light that some druggist at Albany had sold an ounce of *tincture of aconite*, to some unknown person previous to the death of Mrs. H., and had it not been erroneously assumed that aconite was a powerful *acrid*, no one would have ever suspected it to have been the cause of death, nor would any such interpretation have been given to the *post-mortem* appearances.

It is claimed in the present case, that nearly or quite an ounce of the saturated tincture of aconite (of which ten drops have proved fatal), was administered to Mrs. H., in consequence of which she vomited an hour or more incessantly, and then died from exhaustion, with composed and placid features, &c., with the *post-mortem* appearances already noticed.

The experiments of Dr. Fleming upon the lower animals with aconite, prove conclusively that neither the plant, nor any of its preparations, produces vas-

¹ MM. Geiger and Hesser, two distinguished French chemists, have investigated the properties of aconite with much success. They speak of it as follows: “Cette substance alcaline, ne cristallise pas; elle est inodore; elle a une saveur amère sans âcreté; elle n’est pas volatile; peu soluble dans l’eau très soluble dans l’éther et surtout dans l’alcool,” &c. (*Traité Thérap.* &c., par A. Trousseau and H. Pidoux, tom. ii. p. 118.)

MM. Merat and De Lens, in their *Dictionnaire Universel de Matière Médicale*, vol. i. p. 58, have described the properties of aconite very accurately, and in few words as follows: “Pris à la dose d’une drachme ou deux il [the root], produit un véritable empoisonnement. D’abord les sujets éprouvent une ardeur brûlante, une soif ardente, des vertiges se déclarent; de la cardialgie, des vomissements ont lieu, ainsi que des coliques atroces, avec déjections alvines; de la somnolence le manifeste, accompagnée des convulsions et d’une agitation extraordinaire; des sueurs froides et la mort viennent terminer cette scène de douleur au bout de deux à trois heures.”

Sir B. Brodie as well as M. Orfila made many experiments with aconite on animals, and the latter describes the stomach and intestines as *free from inflammation*.—*Med. Leg.* ii. p. 54.

cularity in any membrane to which it is applied, even the lips and tongue when burning and tingling from its topical action; that this is purely a nervous phenomenon, and that inflammatory redness of the alimentary canal is never observed in animals poisoned by it.¹ The prominent symptoms, according to this careful observer, are weakness and staggering, gradually increasing paralysis of the voluntary muscles, slowly increasing insensibility of the surface, more or less blindness, great languor of the pulse, and convulsive twitches before death, with great contraction of the pupil and impairment of the muscular irritability, and, of course, loss of muscular power. When it proves fatal to the human subject, it generally does so by inducing extreme depression of the circulation, or paralyzing the muscles of respiration. Dr. Fleming states "that it may also kill, by an overwhelming depression of the nervous system, proving fatal in a few seconds, without arresting the action of the heart; and secondly by asphyxia, or arrest of respiration, the result of paralysis, gradually pervading the whole muscular system, respiratory as well as voluntary. Such are the results when very large doses are taken." The least variable symptoms in the human subject are, first, numbness, burning and tingling in the mouth, throat, and stomach; then sickness, vomiting, and pain in the epigastrium; next, general numbness, prickling and impaired sensibility of the skin, impaired or annihilated vision, deafness and vertigo; also frothing at the mouth, constriction at the throat, false sensations of weights or enlargements in various parts of the body; great muscular feebleness and tremor, loss of voice, and laborious breathing; distressing sense of sinking and impending death; a small, feeble, irregular, gradually vanishing pulse; cold clammy sweat, and pale, bloodless features, together with perfect possession of the mental faculties, and the tendency to stupor or drowsiness. Finally, sudden death at last, as from hemorrhage, and generally in a period varying from an hour and a half to eight hours. In a case observed by Fleming, where the *tincture of the root* had been taken, the symptoms began in a few minutes. But in a case recorded by Pereira (*Mat. Med.* vol. ii. p. 1806), where the root was eaten in mistake for horseradish, no effects were observed for nearly an hour. Generally, however, the tincture and the alkaloid act with very great rapidity, the effects following on absorption, which may happen in from two to eight minutes, according to the condition of the stomach and the general system at the time.

It must be recollected, however, that the effects of aconite on the system are not uniformly the same. Some anomalies have from time to time been observed; such as convulsions and slight spasmodic twitches of the muscles, owing doubtless, to venous congestion, the result of partial asphyxia; stupor and insensibility, due to the same cause, though they may have been confounded with extreme nervous depression and faintness; delirium; congestion of the mucous membrane of the stomach, &c. This, however, we have seen, is generally found in cases of sudden death. Nausea, vomiting, and pain in the epigastrium, are not regarded by Dr. Fleming as evidence of gastric irritation, as they may all be owing to the same local nervous impression which is produced on the organs of taste. He denies that *purgings* is ever produced by aconite. Pallas, however, mentions that three out of five persons who took *tincture of aconite* died in two hours, with burning in the throat, vomiting, colic, swelling of the belly, and purging. (*Thèse Inaugurale*, Paris, 1822.) Degland relates an instance where four persons took the tincture by mistake, and

¹ Prof. A. T. Thomson remarks (*London Dispensatory*): "Although aconite operates topically, yet dissections of fatal cases have not displayed any particular marks of inflammatory action." (P. 181.)

three of them were seized with burning pain from the throat to the stomach, sense of swelling of the tongue and face, colic, tenderness of bowels, vomiting, and purging; one died in two hours, and one in two and a half hours.

The authenticity of these cases, of Pallas and Degland, is, however, doubted both by Fleming and Pereira, who suppose that it was the tincture of some other root that was taken. In the cases recorded by Pereira, no purging occurred. Diarrhœa, if it does occur in aconite poisoning, is an extremely rare phenomenon.

Morbid Appearances.—Ballardini, who met with twelve fatal cases of poisoning with aconite,¹ represents the pia mater and arachnoid as much injected; much serosity under the arachnoid and at the base of the brain; lungs considerably engorged with blood; heart, and great vessels contained a little black fluid blood; villous coat of stomach spotted with red points; the small intestines presenting red patches, and much mucus. In a case mentioned by Pereira, there was venous congestion of the head and chest, and great engorgement of the lungs and right side of the heart.² It cannot be denied that great redness of the mucous membrane of the stomach and small intestines, has been occasionally observed in cases of poisoning by aconite—as in the cases which occurred some years since at Lille, in France, and recorded in the *Edinburgh Medical and Surgical Journal*, vol. xxviii. p. 452. In some cases, the mucous membrane of the stomach has been found of a light reddish-brown colour; and in others still, quite natural in appearance. There is no instance on record, we believe, where aconite has produced inflammation of any of the mucous membranes or organs of the body, but merely congestion; and this may be of the brain, lungs, liver, spleen, and mucous surfaces; no softening or effusion of lymph, or other mark of inflammation, has ever been found in connection with the patches of redness produced by aconite. Its immediate action in lowering the action of the heart, and arresting or retarding the respiratory movements, proves conclusively its purely sedative and paralyzing power. The appearances on dissection in Mrs. H.'s case do not correspond with those generally observed in cases of poisoning by aconite.

Chemical Testimony.—Chemical evidence is very justly regarded as the most decisive of all the kinds of proof in medico-legal inquiries. In the present instance, however, the anatomical evidence appears to have been regarded as very important.³ The case, however, undoubtedly turned chiefly upon the chemical evidence. This was deemed conclusive. It may have cost John Hendrickson, Jr. his life. Alas! as Raspail has observed, "it is never too late to unlearn an error, but an incorrect testimony, once given in a court of justice, can never be recalled. The sword of the law does not retrace its steps, as the opinion of an experimental chemist may."⁴

Dr. James H. Salisbury, chemist, having charge of the New York State Laboratory, testified as follows:—

"With the portions of the subject submitted to me, I proceeded to make my tests for poisons. In this case, my chemicals were all pure, my implements

¹ *Annali Universali di Medicina*, 1840, iii. 635.

² Christison on Poisons, Am. ed. p. 668, 669, &c.

³ Judge Marvin, in his charge to the jury, says: "We learn from the evidence of Dr. Swinburne, and other medical men, that *post-mortem* examinations will generally disclose the cause of death!" After minutely describing the morbid appearances, because, he says, "they form the facts or basis on which the professional witnesses found their opinion," yet he states that the coroner, Dr. Smith, could not find (though present at the first *post mortem*) the marks of inflammation or congestion in the stomach described by Dr. Swinburne!

⁴ *Organic Chemistry*, p. 525.

and vessels clean. First, I took a small portion of the stomach, its mucous surface, and a small portion of the duodenum; tested first for prussic acid—did not detect its presence; then tested for some of the mineral poisons, first for arsenic, then for corrosive sublimate, the antimonial compounds, the mineral acids—such as muriatic, nitric, and sulphuric acids—also tested for oxalic acid; did not detect the presence of any of these. Next tested for morphine, strychnine, stramonine—also for other poisons, none of which I discovered. I then tested for *aconite*; the tests indicated *aconitine*, the poisonous principle of *aconite*. Took a small portion of the stomach and duodenum, digested it in alcohol over a water-bath, then filtered; evaporated the filtrate partially; the oily matter rose to the surface; this I separated by decantation, and then absorbed it from the surface by bibulous paper; then mixed the solution with purified animal charcoal, agitating it for some little time after mixing; filtered; and to this solution I applied my tests, as follows: I boiled a small portion of this solution with sulphuric acid; the solution was turned a deep port-wine red colour. I then boiled a small portion of the solution with hydrochloric acid; this turned the solution to light port-wine red colour. Then boiled a small quantity of the solution with nitric acid; the solution remained clear, with no change of colour. *From these tests I inferred the presence of aconitine.* I repeated these tests several times with the same results. The stomach and duodenum were what I made my preliminary tests with; these tests are what are laid down for *aconitine* by the best authors. I had made these tests previously, and had also made *aconitine* my special study. My tests produced the same results. I have applied them since with similar results. For two years previous to this, I had paid much attention to vegetable alkaloids, and among them especially *aconitine*, the poisonous principle of *aconite*. On the 15th of March I commenced the process of *analysis* for the purpose of separating *aconitine*, if present in sufficient quantity. I divided a portion of the remaining portions of the stomach and duodenum, and their contents, the small intestines, a portion of the liver, and a portion of the blood, into two equal parts. One of these portions I digested in alcohol for several hours over a water-bath; then filtered; partially evaporated; separated the oily matter by decantation and absorption; evaporated nearly to dryness; mixed with the alcoholic extracts pure caustic potassa; distilled; heated the distilled matter with dilute sulphuric acid, sufficient to neutralize it; evaporated this over a water-bath; treated it with pure alcohol, between 74 and 80 per cent.; filtered it; evaporated it nearly to dryness; treated the residue with pure caustic potassa, and again distilled; evaporated this slightly, and set it aside for future use. The other and second portion was digested in alcohol over a water-bath for several hours; filtered; evaporated partially; separated oily matter by decantation and absorption; evaporated nearly to dryness; treated the alcoholic extract with dilute sulphuric acid and distilled water; filtered; then evaporated it partially; treated the solution with ammonia to a slight excess; a precipitate was formed; this was carefully washed by a small quantity of water; this precipitate was redissolved in dilute sulphuric acid and distilled water; added to this solution a small quantity of purified animal charcoal, agitating for some minutes, and then filtered it; evaporated the filtrate slightly at a low temperature; added ammonia in slight excess; a precipitate was formed; this I carefully washed with a small quantity of distilled water; this result I mixed with the result obtained by the other process; in all there was about two-thirds of a teaspoonful. I was from the 15th to the 19th of March in getting through this process; it was going on day and night. In testing for this matter, I placed it on my tongue; it had a bitter taste; a sparkling (?) sensation at first, which, in three or five minutes, turned into a numbness, producing a stiffness of the surface; the sensation produced was very much like that in the foot when it is said to be asleep. This matter, which I separated by the process just mentioned, I gave to a cat; gave it in small pieces of beefsteak; in about half an hour she exhibited a choking sensation and swallowing; this was followed by slight contraction of the muscles, twitchings, which moved the limbs slightly, and this by a tendency to vomit. These spasms lasted from one to two minutes; considerable stupor succeeded; she lay down upon her

side and breathed heavily, as though she was under the influence of some narcotic; this lasted for some time; it gradually passed off, and in about three hours she was quite natural again. On the 29th of March, I gave this cat six drops *tinct. of aconite*. In administering it, I opened her mouth, held her head back, and poured the tincture immediately down her throat; after five or ten minutes she commenced swallowing; in fifteen minutes she commenced vomiting slightly; this vomiting continued for twenty-five minutes, when she became very weak, and fell upon her side; the vomiting here ceased; she breathed heavy and slow, and in one hour and a half after the poison was taken she died. The *post-mortem* was made seven hours and a half after she died. The stomach and intestines were found very much contracted; about one-third the usual size. The intestines were very much contracted and rigid; the walls thick. We then opened the stomach, and found a very high state of congestion; it was very much contracted on itself, and thrown into folds. The mucous coat was covered with a small quantity of mucus, tinged with blood. The duodenum was contracted; very much congested. The mucous coat was covered with mucus, tinged with blood. There was one or two places in the lower portion of the duodenum, where the mucus was tinged with bile; the whole was tinged with blood. The jejunum was considerably congested and contracted. The mucous coat was covered with mucus, tinged with blood. The ilium had much the same appearance as the jejunum, except that the mucus which covered its surface was of a slightly different colour—pure white; there was a little bile here. I found no fecal matter thus far; in the cæcum I found fecal matter; it was partially digested food, mixed with white frothy matter; the upper portion of the colon also contained fecal matter, which was thin and watery; not as thin and watery as that in the cæcum. The fecal matter grew harder as it approached the rectum; there was no purging. The urinary bladder was very much contracted; there was no water in it; the gall-bladder was about half full.”

Question. “After your researches and anatomical experiences, are you ready to swear that Mrs. Hendrickson was poisoned by aconite?”

Answer. “In my opinion she was poisoned by aconite.”

Dr. S. further testified that—

“The *post-mortem* appearances of the stomach and intestines, and those of the animals killed with aconite, are not produced by any other substance known to him; and that he knows of no disease which will produce such appearances.”

In examining the chemical evidence, we must first express our regret that the details of the processes employed by Dr. Salisbury, are so imperfectly given. We can only judge of the correctness of the results from the very meagre account presented in the evidence as published.¹ We shall, however, if we mistake not, discover such errors in the proceedings as to vitiate in a great degree, if not wholly, the conclusions which are drawn from them. We are first given to understand, that with “a small portion of the mucous surface of the stomach and duodenum,” Dr. S. first tested for *prussic acid*, then for some of the *mineral poisons* (arsenic, corrosive sublimate, the antimonial compounds, the mineral acids, muriatic, nitric, and sulphuric), then oxalic acid; also for *morphine*, *strychnine*, *stramonine*, (daturia?) and also “for other poisons,” but without success. These various processes, we presume, must have taken a considerable portion of the “small portion of mucous membrane” employed. We have no means of judging of the accuracy of the tests employed, for few, if any, details whatever are given.

Dr. S. then states “that he *tested* for aconite, and found it by proceeding as

¹ We assume, however, that the evidence of Dr. S. is correctly printed, as it was done at the place of his residence, and contains notes by himself, added after his testimony was given.

follows: He took, as before, a small portion of stomach and duodenum, digested it in alcohol over a water-bath, then filtered, evaporated the filtrate partially; the oily matter rose to the surface; this he separated by decantation, and then absorbed it from the surface by bibulous paper; then mixed the solution with purified animal charcoal, agitating for some time after mixing, filtered, and applied his acid tests as follows: First, he boiled a small portion of the solution with sulphuric acid; the solution was turned a *deep port-wine red colour*; then the same process with hydrochloric acid gave a *light port-wine red colour*; then with nitric acid, and the *solution remained clear*; and from these tests, Dr. S. says, "*he inferred the presence of aconitine*," but on what grounds we are totally at a loss to discover. *Sulphuric acid* boiled with aconitine gives a *dark brown tint*, according to Taylor, instead of a *deep port-wine red colour*; and animal matters, boiled with sulphuric acid, will give the latter colour *without the presence of aconitine*; so say Taylor and the best authorities on legal medicines. (*Taylor on Poisons*, Am. Ed. p. 615.)

Prof. Emmons, in a published letter to Gov. Seymour, of New York, states as the result of his experiments, that "sulphuric acid boiled with the tincture of aconite, obtained from the same sample as that supposed to have been sold to Hendrickson, *lost most of its red colour*, and became quite pale; boiled with pure aconite, the solution remained colourless, and the same result took place when boiled with nitric acid;" but that when he added oil or animal matters to the mixture of aconite, then he "obtained the red colours spoken of by Dr. Salisbury, and the same also occurred *when he employed the same tests on these animal matters alone*."

We think there can, therefore, be no doubt whatever that the results arrived at by Dr. S. were entirely due to the presence of organic matters, and not to aconitine, although the colour obtained by Dr. Emmons with sulphuric acid differs from that laid down by Taylor as resulting from a solution of pure aconitine.

But we are not called upon to reconcile this discrepancy; our aim is merely to show the incorrectness of the inference that aconitine was discovered by the process employed by Dr. S.¹

To confirm the conclusion arrived at by these tests, Dr. S. then proceeded to separate a sufficient quantity of aconitine for experimenting with it on animals; and he "divided a portion of the remaining portions of the stomach and duodenum and *their contents*, the small intestines, a portion of the liver, and a portion of the blood, into two equal parts;" one of these he digested in alcohol over a water-bath, for several hours, "then filtered, partially evaporated, separated the oily matter by decantation and absorption, evaporated nearly to dryness, mixed pure caustic potassa with the alcoholic extracts, distilled, treated the distilled matter with dilute sulphuric acid sufficient to neutralize it; evaporated this over a water-bath, treated it with pure alcohol; filtered and evaporated it nearly to dryness; treated the residue with pure caustic potassa, and again distilled; evaporated slightly and set aside for future use."

The above process is that which was sometimes formerly pursued by chemists when examining *for a liquid and volatile alkaloid*, but why it should have been resorted to in the present instance, we are at a loss to understand; as Dr. S. states, in his cross-examination, that he believes aconitine "*is a fixed body*." We are still more at a loss to know why Dr. S. did not test the matters obtained, in order to ascertain whether he had discovered aconitine or any

¹ Before commencing the analysis, it would have been as well, perhaps, to have tested the substances employed, and seen whether they were *acid* or *alkaline*.

other alkaloid by this process. He neither tasted of it, examined it with a microscope, or gave any of it to the lower animals.

But the course pursued by Dr. S., even on the supposition that a volatile alkaloid was present, is not one which will approve itself to the minds of scientific chemists, as the most eligible.

In the first place, in examining the tissues of organs for a vegetable alkaloid, the organ should be divided into very small portions, then the mass moistened with pure alcohol, and expressed strongly; and so, by further treatment, exhaust with alcohol the tissues of everything soluble; then the liquid so obtained should be treated in the same way as a mixture of suspected matter and alcohol. Moreover, it is a principle now well established in medico-legal researches, that we should never use *animal charcoal* for decolorizing liquids, while searching for the alkaloids; for the very good reason that we, by so doing, may lose all the alkaloid in the suspected matter, animal charcoal having the power, as proved by M. Stas, of absorbing these substances, while, at the same time, it fixes the colouring and odoriferous matters. This error, then, would vitiate the results obtained by Dr. Salisbury, even were his other processes unexceptionable.

The true and only correct mode of proceeding, whether the alkaloid be fixed or volatile, would be that pointed out by M. Stas, who adopted it with such brilliant success in the case of M. Fougues, who was poisoned by his brother-in-law, Count Bocarmé, with *nicotine*. (See this *Journal* for Jan. 1854, p. 263, *et seq.*)

To the matter obtained by digesting the different organs suspected of containing the alkaloid with strong alcohol, twice their weight of pure alcohol should be added, and afterwards, according to the quantity and nature of the suspected matter, from ten to thirty grains of *tartaric* or *oxalic* acid should be added (the *tartaric* is preferable), we are then to introduce the mixture into a flask, and heat it to 160° or 170°. After it has cooled, it is to be filtered, the insoluble residue washed with strong alcohol, and the filtered liquor *evaporated in vacuo*; or, if the operator has not an air-pump, the liquid should be exposed to a strong current of air, at a temperature of not more than 90° F. If the residue, after the volatilization of the alcohol, contains fatty or other insoluble matters, the liquid should be filtered a second time, and then the filtrate and washings of the filter evaporated in the air-pump till nearly dry; or, if no air-pump is at hand, it should be placed over a bell-jar, over a vessel containing concentrated sulphuric acid. The residue is then to be treated with cold anhydrous alcohol, taking care to exhaust the substance thoroughly; and the alcohol should be evaporated in the open air, at the ordinary temperature, or what would be still better, *in vacuo*. The acid residue should then be dissolved in the smallest possible quantity of water, and the solution introduced into a small test-tube, and a little pure powdered bicarbonate of soda, or potash added, little by little, till a fresh quantity produces no further effervescence of carbonic acid. The whole should then be agitated with four or five times its bulk of pure ether, and left to settle, when the ether swimming on the top is perfectly clear, some of it should be decanted into a capsule, and left in a very dry place to spontaneous evaporation. We are now prepared to proceed to examine for a volatile or a fixed alkaloid, as the case may be; and no process less precise or carefully conducted, should be deemed worthy of confidence, in a case where life or death is hanging on the result.

Now if a *liquid alkaloid* be present, if we evaporate the ether, we shall have remaining on the inside of the capsule some small liquid striæ, which fall to the bottom of the vessel; and by the heat of the hand alone, the con-

tents of the capsule will expel an odour more or less disagreeable, according to the nature of the alkaloid (if it possesses odour); it may be pungent, suffocating, irritant, or simply disagreeably narcotic, modified by an animal odour. If any traces of a volatile alkaloid be discovered, then we should add to the contents of the vessel, from which we have decanted a small quantity of ether, one or two fluidrachms of water, acidulated with a fifth part of its weight of pure sulphuric acid, then agitate for some time, leave it to settle; pour off the ether swimming on the top, and wash the acid liquid at the bottom with a quantity of ether, as most of the sulphates of the alkaloids are insoluble in ether, and the others but partially so. The water acidulated with sulphuric acid will contain the whole, or a greater portion of the alkaloid in the solution, while the ether will retain all animal matters which it has taken from the alkaline solutions. To extract the alkaloid from the solution of the acid sulphate, an aqueous and concentrated solution of potash or caustic soda is to be added; the mixture is agitated and exhausted with pure ether; the ether dissolves the ammonia, and the alkaloid is now free. The ethereal solution is exposed, *at the lowest possible temperature*, to spontaneous evaporation; nearly all the ammonia volatilizes with the ether, and the alkaloids remain as residue; and in order to separate every particle of ammonia, the vessel containing the alkaloid is to be placed, for a few minutes, in a vacuum over sulphuric acid, and the organic alkaloid is obtained with all the physical and chemical characters belonging to it.

By pursuing the process above detailed, M. Stas succeeded in detecting *nicotine* in the blood from the heart of a dog, poisoned by a very minute quantity of this substance, introduced into the œsophagus. He also positively determined its existence in the blood generally, by the tests of *odour*, *taste*, and *alkalinity*; also the chloroplatinate of the base *perfectly crystallized in quadrilateral, rhomboidal prisms*.

We believe if a volatile alkaloid had been present in the matters operated on by Dr. Salisbury, he could not have succeeded in detecting it by the processes which he employed.

Nor was the course pursued in relation to the second portion, which was examined for a *fixed alkaloid*, less liable to criticism. This was digested in alcohol over a water-bath, for several hours, then filtered and partially evaporated; the oily matter separated by decantation and absorption; evaporated nearly to dryness; the alcoholic extract treated with pure distilled water and filtered; the filtrate evaporated nearly to dryness; the water extract treated with dilute sulphuric acid and distilled water filtered; then evaporated partially, and the solution treated with ammonia to a slight excess; the precipitate formed was carefully washed with a small quantity of water, and redissolved in dilute sulphuric acid and distilled water. To this solution a *small quantity of purified animal charcoal* was added, then agitated for some minutes and filtered; the filtrate evaporated slightly at a low temperature, and ammonia added in slight excess. The precipitate formed was washed with a small quantity of distilled water, and the result was mixed with that obtained by the former process; "and in all there was about two-thirds of a teaspoonful," it requiring four days and nights to complete the process.

The same fatal error was here committed as before, by using animal charcoal, supposing the alkaloid to be a fixed body. Going back to our first process, it may happen that the evaporation of the solution resulting from the treatment of the acid matter, to which bicarbonate of soda has been added, may leave or not a residue containing an alkaloid. If it does, then a solution of caustic potash or soda is to be added to the liquid, and it is to be agitated

briskly with ether. This dissolves the vegetable alkaloid, now free and remaining in the solution. In either case, we exhaust the matter with ether; and whatever be the agent that has set the alkaloid free, whether bicarbonate of soda, or potash, or caustic soda, or potash, it remains by the evaporation of the ether on the side of the capsule, as a solid body, or more commonly as a colourless, milky liquid, holding solid matters in suspension, and having the physical, chemical, and toxicological properties of that peculiar alkaloid.

After an alkaloid has been discovered, the next thing which scientific accuracy demands is, that it should be obtained in a crystalline state, so as to determine its form; and this is generally done by putting some drops of alcohol in the capsule with the alkaloid, and leave the solution to spontaneous evaporation. It may, however, be too impure, by contamination with foreign matters, to crystallize, when some drops of water, feebly acidulated with sulphuric acid, should be poured into the capsule, and then moved over its surface, so as to bring it in contact with the matter in the capsule. The matter contained in it will separate into two parts, one formed of greasy matter, which remains adherent to the sides, the other alkaline, which dissolves and forms an acid sulphate. The acid liquid is then cautiously decanted, which ought to be limpid and colourless if the process has been well executed; the capsule is then to be well washed with some drops of acidulated water, added to the first liquid, and the whole is evaporated to three-fourths, *in vacuo*, or under a bell-jar, over sulphuric acid; a concentrated solution of pure carbonate of potash is then added to the residue, and the whole liquid treated with absolute alcohol. This dissolves the alkaloid, while it leaves untouched the sulphate of potash, and excess of carbonate of potash. The evaporation of the alcoholic solution gives the alkaloid in crystals, if crystallizable. With regard to aconitine, it is apparently crystalline, the fragments, according to Pereira, appearing under the microscope like thin plates of chlorate of potash, varying greatly in shape, though the triangular form is most common. Dr. Pereira states that he could discover no distinct crystals. But Dr. Salisbury neither gives us the physical nor chemical properties of what he supposed to be aconite.

We have given the improved process of M. Stas of searching for the vegetable alkaloids, because of its superiority to former methods;¹ and it is now acknowledged by the ablest chemists as the most accurate of any yet known. We have seen that the evidence of the existence of aconitine, from the mode of proceeding, and the tests employed by Dr. S., is inconclusive, and comparatively worthless; and it is no less evident that that furnished by his subsequent analysis is equally valueless. It is so, because there is observable, through the whole processes, a departure from those rules which apply to such cases, and because the most common precautions for securing accuracy were neglected. The results, colourations, which were supposed to indicate the presence of aconitine, have been demonstrated to be owing to the action of the acids employed on organic matters. Reasoning by exclusion, which should be rigidly carried out in such cases, and to its utmost limits, would have led, and without much labour, to the correction of this first fatal error. If the substance found was aconitine, as believed, then, as no fact is better known than that this is a *fixed body*, why the very unscientific process of separating it by distillation, as a volatile substance? Dr. S. states, in his testimony, that he had made aconite a subject of particular study for two years previously, and yet he had not learned whether it was or not a fixed

¹ Orfila on Nicotine. Paris, 1851.

alkaloid; and that "he was not sure by which of his processes aconitine was obtained, as he tasted of neither before they were mixed." He might have been very sure he had not obtained it by distillation, for it was a chemical impossibility; he might have been pretty confident, however, that *ammonia* could thus be procured. We shall not, however, enter into a detailed criticism of the second analysis, because we conceive it to be unnecessary. With regard to the character of the precipitate formed, it is most probable that it was *phosphate* and *lactate of lime*, derived from the animal fluids employed in the analysis; but, as the physical and chemical characters of this precipitate are nowhere described, we are wholly left to conjecture. Here, at this point, by simple chemical methods, proof could have been accumulated which would have put the matter forever at rest; as it is, no one is satisfied, and every one is incredulous. All the first chemists of the country, including the Sillimans, Dana, Torrey, Chilton, Wells, Ellet, Bacon, Hayes, Porter, Kent, Emmons, Beck, &c., have published to the world their opinion, that, in their judgment, "no chemical result stated by Dr. Salisbury furnishes satisfactory evidence of the existence of aconitine or its compounds, in the fluids or organs submitted to examination."¹

If the reader will compare the process of M. Stas, as we have detailed it, with that actually pursued by Dr. S., the inaccuracy and unscientific nature of the latter will be obvious. In his cross-examination, Dr. S. states that he believes that he obtained from $\frac{1}{10}$ to $\frac{1}{25}$ gr. of aconitine in the matters analyzed. Prof. Emmons states that there does not exceed $\frac{1}{3}$ gr. in \mathfrak{Sj} of Burrough's tincture; and Dr. Reid stated, in his testimony, that he had found, by actual experiment, that there is $\frac{1}{64}$ gr. in \mathfrak{Sj} of the tincture. (It is only claimed that \mathfrak{Sj} was given to Mrs. Hendrickson!)

Dr. S. states that he tested the matter which he had obtained (the precipitate) by placing it on his tongue; that "it had a bitter taste, a sparkling (?) sensation at first, which turned into a numbness in a few minutes, producing a stiffness of the surface," &c. The substance, being the precipitate ($\frac{1}{25}$ gr.), to which a small quantity of water had been added, amounted, we are told, to "about two-thirds of a teaspoonful," probably. But a few drops of this was used for testing; if two drops, then but about $\frac{1}{700}$ of a grain was employed, to which these effects must have been owing; and this upon the supposition that the aconitine was equally distributed throughout the mixture. The whole remaining portion was administered to a *cat*, without any one present to notice the effects but himself, and the life of an individual hanging on the result!² The substance was given in small pieces of beefsteak. "In half an hour she exhibited a *choking sensation* and swallowing, followed by slight contraction of the muscles; twitchings, which moved the limbs slightly, and then a *tendency* to vomit; considerable stupor succeeded; she lay down on her side and breathed heavily, as if under the influence of a narcotic; this lasted some time; it gradually passed off, and in three hours she was well again."

This same cat was killed in one hour and a half, a week afterwards, by giving it *six drops of tinct. of aconite*. Symptoms: "swallowing, slight

¹ Alb. Eve. Journ. April 29, 1854.

² In the case of Count Bocarmé, M. Stas experimented with *nicotine* obtained, on small birds, as sparrows, which are extremely sensitive to the action of the vegetable alkaloids; and the *remainder* was carefully sealed and labelled, to exhibit at the trial, and for experiments before the jury. Such a course, we believe, is not unfrequently pursued on the continent, especially in Germany and France, and it is one which must commend itself to every lover of science and humanity.

vomiting, weakness, and heavy, slow breathing." *Post-mortem* appearances identically the same as in the case of Mrs. Hendrickson, or, as Dr. S. testified, "they were so similar, that it was almost impossible to distinguish a shade of difference between them." (P. 53.)

It certainly seems a strange anomaly, that while $\frac{1}{25}$ of a grain of aconitine did not injure the cat materially, six drops of the tincture should have proved fatal!—the whole ounce, according to Prof. Emmons, only containing but the $\frac{1}{7000}$ part of a grain, which would give about $\frac{1}{350000}$ of a grain in six drops. We do not vouch for the accuracy of this calculation; we only record it as one of the difficulties in the case.¹ Well did Mr. Wheaton, in his able defence, remark that "the cat should have died (by the first experiment) out of deference to the Doctor's opinion; or, the Doctor should have given up his opinion out of deference to the life of the cat."

We have not designed to offer any comments upon the *moral* evidence submitted in this case—any further, at least, than it is connected with the medical and legal evidence. We may, however, be allowed to say, that it lends little or no confirmation to the belief that poison was administered to Mrs. H. It was not proved that the prisoner ever purchased aconite, knew of its properties, or had it in his possession. The charges brought against his moral character, in the opening speech of the prosecuting attorney, particularly in regard to his having had gonorrhœa, and having imparted the same to his wife, were wholly unsustained by any evidence offered on the trial. The object aimed at, viz: prejudicing the minds of the jury against the prisoner, was, however, perhaps as fully attained as if the charges had been proved. There was no evidence that any vomiting had taken place; indeed, everything went to show that it had not; the countenance of the

¹ It is true, we have no precise information as to the quantity of aconitine contained in the root of the plant. Prof. L. Reid, of N. Y., testified that he had obtained but one grain from one pound of the root, and believes that no more than that amount can be obtained. He also states that the taste of aconitine can be detected in $\frac{1}{10000}$ part of a grain. Prof. Emmons expressed the opinion that $\frac{3}{4}$ of the tincture of aconite contains but $\frac{1}{700}$ of a grain, allowing $\frac{3}{4}$ of the root to $\frac{3}{4}$ of alcohol. Dr. Burroughs, in a letter recently received from him, states that he followed the U. S. P. of 1850 in preparing his tincture, which directs one pound of the root to two pints of alcohol, which would give four times the strength as estimated by Prof. E. But even this, on Prof. Reid's estimate, would give less than $\frac{1}{400}$ of a grain to $\frac{3}{4}$ of the tincture; but, as half a drachm of this tincture would be a hazardous and perhaps a fatal dose, and as this would be but the twenty-fourth part of an ounce, or the $\frac{1}{4800}$ part of a grain, we must acknowledge that there are some difficulties about these calculations which require to be cleared up. Dr. Pereira states that the one-fiftieth part of a grain of aconitine has endangered the life of an individual, and that it is by far the most poisonous principle known. Prof. Christison found 30 grains of an alcoholic extract kill a rabbit in two hours and a quarter; and this was the whole produce of three-quarters of an ounce of the fresh leaves. And, in another experiment, one-tenth of a grain introduced into the cellular tissue of a rabbit, killed it in twelve minutes. Orfila gave five drachms to a dog, and it killed him in twenty-one minutes. But the results were very various in his experiments, which he accounts for from the different pharmaceutical processes employed in making the preparations. In one instance, *e. g.*, he gave $\frac{3}{8}$ of an extract of aconite to a dog without any effect; in another case, one-fourth of an ounce of the extract killed a dog in two hours. All experimenters agree in the opinion that it is not *narcotic*, and Pereira found that it never produced stupor, or affected the mental faculties.

With regard to *animal charcoal* absorbing the active principle of aconite along with its colouring matter, Prof. Emmons has stated that he "had employed the tincture of aconite as the substance operated upon, and had not only insulated the active principle in the charcoal itself, but had subsequently dissolved it out by means of alcohol."—*Letter to Gov. H. Seymour of New York, in Alb. Eve. Journ.* April 29, 1854.

deceased was mild and placid; no distortion, no disheveling of the hair, no wrinkle or spots on her clothes, no signs of violence upon the exterior of the body, no marks of suffocation; the deceased lay as if asleep, and everything in the room in the same condition as when she retired to rest. This does not look like severe vomiting, or death from violence—unless, possibly, from chloroform—but this is not charged. The Judge (Marvin), in his charge to the jury, seems to take the ground that, if the medical witnesses, called for the defence, could not prove what was the actual cause of death, it was right to assume that the deceased came to her death by violence on the part of her husband! “It would be more satisfactory,” he says, “if these medical witnesses had assigned some natural cause for her death;” as if the burden of proof rested on the prisoner, instead of the people, although he expressly disclaims any such ground. We confess that the case is enveloped in considerable mystery, but no more than enshrouds hundreds of cases of sudden death. We were called once to see a lady who, in the enjoyment of a comfortable state of health, while attending a social gathering at her own house, was seized with a fainting fit, and died in spite of all the means employed before our arrival. We made a careful *post mortem* the next day of all the vital organs, but there was no cause of death discoverable. The walls of the heart were thinner than usual, and syncope was occasioned by violent mental emotion, which resulted fatally. We have known several cases of sudden death from syncope, without any *post-mortem* lesion sufficient to account for the death. Fatal cardiac asthenia may be produced by mental emotions, and no *post-mortem* change or lesion discovered. It may have been so in the present instance. There was considerable *uterine* disease, as ulceration and congestion of the os uteri, and leucorrhœa; sympathetic disturbance of the heart may have been occasioned by mental emotion acting upon this predisposition, and death occurred as in the case already alluded to. Or, she may have inhaled chloroform to relieve pain, of which she had complained much during the day; or, she may have taken an overdose of *homœopathic* pills of aconite, of which she had a considerable quantity on hand just before her death, and which she carried in her pocket (of which none were found afterwards.)¹ Many causes might be assigned to account for her death, any one of which would be more probable than that she was poisoned by an ounce of tincture of aconite. There is one fact stated by the Judge in his charge, as of very great importance, and one which we have not as yet particularly noticed, and that is, a small *ecchymosis* or bruise on the inside of the lip. The Judge says this “furnishes overwhelming evidence of guilt to his mind.” Dr. Swinburne describes it in his testimony as “a black and blue bruise between the size of a sixpence and a ten-cent piece, inside of the lower lip, and a little to one side; in it there was a cut about a quarter of an inch long; it must have occurred before death.” This slight mark is exaggerated into a degree of importance it does not deserve. It was proved by the mother that the deceased complained two days before her death of a sore on the same lip. The evidence that the *ecchymosis* was caused during life is by no means satisfactory; indeed, no evidence is gone into, or facts detailed, which have any bearing on this point. It is stated by the mother, that violent attempts were made by her to open the mouth, for the purpose of administering a little camphor and water, as soon as it was discovered that her daughter was dead; and probably this was not long after she ceased to breathe. The *ecchymosis* may

¹ It is also held by some pathologists that death may suddenly occur from apoplectic congestion of the brain, and cerebral fulness disappear after death.

have been thus produced, though we think it more probable that it existed before death. Such marks, moreover, are not unfrequently caused by the teeth during a convulsive paroxysm. Standing alone, it by no means proves any violence on the part of the accused.

Another feature in this trial is worthy of mention, perhaps, and that is the great importance attached to the confident and positive statements of Drs. Swinburne and Salisbury, both young men, and comparatively inexperienced, and the little weight which seems to have been allowed to the more careful and judicious testimony of Drs. Emmons and Staats, men of age, professional skill, and enlarged experience. The *positive* statements of the former would probably, with such a court and jury, have outweighed the negative testimony of all the first pathologists and chemists of the age. The result shows very clearly the importance of qualifying and guarding our statements and opinions where facts will allow of another interpretation, and where they fall short of actual demonstration.

We have thus successively passed in review the most important features in this interesting trial, and made such comments as seemed to us appropriate to the occasion. We know none of the parties, nor are we influenced by any personal considerations whatever. Medico-legal science demands thus much, at least, at our hands. If we have spoken with undue severity, in regard to any testimony offered, it will ever be to us a source of regret. But the interests of science and humanity require that the medical and chemical evidence, on the strength of which the life of a human being has been taken, should be closely scrutinized, and in all the lights which observation, reason, common sense, and true science may furnish us. The case will stand as a precedent in medico-legal questions that may hereafter come before our courts. It is all-important to know whether the questions it involves have been decided correctly or not; whether it may be referred to as a safe or an unsafe precedent for witnesses, courts, and juries in future times. We have sufficiently indicated our opinion. We are ready, however, to retract whenever our reason is convinced that we have been led into error—not before. It is in vain, for it is too late, to lament the probable sacrifice of an innocent man; but, if what we have written serves but to throw an additional safeguard hereafter around the wrongfully accused, our purpose will have been fully accomplished. We will venture one remark more. It seems to us no less strange than lamentable, that in a case which appears to have rested solely on anatomical, or pathological and chemical evidence, the counter opinions and statements of the leading men in these departments of science in the country should have had so little weight with the executive or judicial power as not to lead to a commutation of punishment, nor even to an order for a new trial, which was demanded. We pretend not to fathom the hearts or the motives of men, but we think we see in the present instance an example and illustration of the force of outward pressure—of popular excitement and prejudice—on witnesses, counsel, judges, and jury, which makes us question, at times, whether the boasted right and privilege of trial by jury, be, indeed, a blessing or a curse.

C. A. L.

BIBLIOGRAPHICAL NOTICES.

ART. XV.—*Transactions of the Medical Association of the State of Alabama, at its Seventh Annual Session, begun and held in the City of Montgomery, Jan. 10-12, 1854.* 8vo. pp. 190.

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Transactions of the Medical Association of Southern Central New York, at the Eighth Annual Meeting, held at Homer, June, 1854. 8vo. pp. 120.

Transactions of the Fifth Annual Meeting of the Medical Society of the State of North Carolina, held at Raleigh, N. C., May, 1854. 8vo. pp. 97.

THE annual volumes of *Transactions* issued by the several State Medical Societies we have always looked upon as publications of very great interest. Although the reports embraced in them may be, occasionally, commonplace or superficial; although the facts they record may not always be detailed with sufficient fulness and precision, nor examined with proper care in all their bearings and correct relations; and although, too often, undue space may be given to matters of mere speculation or of little value, while those of primary importance are overlooked or carelessly treated, still, we never fail to find embodied in these *Transactions* observations of great value, calculated to correct, confirm, or enlarge our views in relation to the etiology, character, and proper treatment of the diseases incident to the several sections of our country, or which prevail among the different classes of its population.

1. We have examined with a very great degree of satisfaction the reports and papers constituting the published *Transactions of the Medical Association of the State of Alabama*, at its last annual session; all of them are replete with interest, while several are particularly valuable.

The session was opened by an address from the President of the Association, Dr. A. Denny, "On the Magnitude of Science, or a Contrast between the three Learned Professions." The style of this address is perhaps too rhetorical, as well for the subject discussed as for the character of the assembly addressed. Many of the bold expressions of the speaker are liable to be misunderstood; they will lead some to attribute to him sentiments which he certainly never meant to inculcate.

The annual oration of Dr. A. Lopez is an address of a very different character from that of Dr. Denny. Its subject is an important and interesting one: "The Mutual Relations that should exist between the Representatives of a Commonwealth and its Medical Men;" or, in other words, the necessary co-operation of the sciences of legislation and of medicine for the promotion of the common weal. With commendable dignity, and in clear and energetic language, Dr. Lopez urges upon the attention of legislators the necessity in every State of a body of well-instructed physicians, not merely to minister to such of its citizens as may be overtaken by disease or suffer from accidental violence, but, by their professional knowledge and skill, to aid the legislature in the enactment of laws having for their object the protection of the health and the promotion of the vigour, comfort, and general well-being of the entire community; to assist the officers of justice in the detection of guilt and the protection of the innocent; and to render efficient the efforts of the philanthropist to diminish crime, pauperism, and suffering. In consequence of the important services thus rendered by the physician to the community at large, Dr. Lopez insists upon it as the duty of the legislature to do everything in their power to promote medical education, and to protect the legitimate members of the pro-

fession from the encroachments of ignorant pretenders, and of charlatans of every class and description.

The following extracts will give to our readers some idea of the manner in which Dr. Lopez enforces the claims of the medical profession upon the attention of what should be the ever-vigilant conservator of the people's best interests—an enlightened legislature:—

“The progress of science, and consequently of truth, cannot fail to impress the reasoning man with the conviction, that degenerating influences are reflected upon the intellectual integrity of communities, accordingly as their physical organization improves or deteriorates. Not only intellectual, but also moral character feels the injury which a defective physique imparts. History attests this truth, medical history corroborates the assertion; and the world's chart, with its multiplied statistics, presents daily to every government interested in the inquiry, causes to show how necessary it has become to legislate by acts that ‘grasp beyond a grain, and look beyond an hour.’ Nor can governments be deaf to the call of reason, or blind to the evidences of political revolution. The bureaux of census and registration, inquiries into sanitary laws, and commissions of health, are daily growing into importance among nations; and the affinity of the parish pauper with the parish sick, create the thought, how much the last two influences the others, as correlative causes of much that bears upon their external as well as internal safety.

“Hence, you must infer that the *people's health constitutes the people's safety*. I desire you to give this a latitudinous construction. It is not alone the health of an individual, but of a generation. It is not an isolated disability, ending with the victim of its attack, but extending through a mysterious series of hereditary transmissibility to every collateral branch of society, in the present, and for the time to come.

“Permit me, therefore, Gentlemen Representatives, to invite your serious thoughts to how much you should be committed in the encouragement and protection of every measure that can rouse up and foster in your State a well-educated, moral, and scientific corps of professional men, to whose skill and care you can confide the well-being of her citizens. Ask yourselves, whether it be prudent and wise to adopt a policy that retards their advancement, discourages their efforts, and represses the spirit that should prompt them, rather than throw your buckler between them and sinister influences, deeply calculated to operate as fatally upon yourselves and your constituents as upon a profession whose object is exalted, and whose utility is unquestioned.

“There is scarcely an electric flash that pulsates along those winged messengers, momentarily flying through space, from one continent to another, that does not tell how sensibly alive are the nations of the earth to the great problem of human life. How is its percentage established? what constitutes the bases of your life insurances? or, how test the acumen of your political economists? Surely, by the reliability that you can place upon those who are not only called upon to testify in cases of doubt, but who also have in their keeping the preservation and fortifying of those sanitary conditions by which the citizen is qualified to do service to his government; by perpetuating and transmitting a healthy and vigorous race into her commonwealth; by guarding her against the incubus of pauperism, vagrancy, and mendicancy, which drain her resources, disorganize her morals, and disgrace her estate; by preserving intact, through healthy physical organization, the intelligence of her subjects, and filling her stations of power and dignity with moral aliment, rather than crowding her hospitals and jails with the unseen, though certain consequences of depraved and ruined constitutions. Would you understand this?—ill health, with ruined or unsound constitutions, begets inability to labour, destitution, desperation. Upon these follow temptation, crime, contempt of law, insanity, death by self-immolation, and consequently, impoverishment and debility of the body politic.

“This is no fancy sketch, pleading for a profession that has emerged from its long infancy, and now feels its own strength in the consciousness of a well-earned reputation; but figures and statistics verify my statements. They prove that in proportion as a provident legislature invests qualified persons with all things pertaining to sanitary and hygienic regulations—to the extent

that it works consentaneously with the profession most conversant with requisitions for the public health, and upholds its members by its strong arm and liberal interpretation, closing the door against dangerous and untutored men, so far will it accomplish its highest duty, and so much will it enhance the obligation under which it lays its citizens."

"Does the statesman seek to thread the labyrinth of human progress; the lawgiver ask for light upon the dark and hidden phases of natural laws; or municipal power grope in vain for guidance when pestilence and death overshadow the land? Who is it that elucidates and instructs? It is the scientific physician, whose eye no form visible eludes; from whose ear no sound audible escapes; beneath whose hand the mystic law resolves itself into shape, and in whose crucible of untiring exploration, elements apparently indivisible are simplified and distinguished. The forest and the cave, the ocean and the air, things animate and inanimate, yield up their properties at his behest; and their most recondite virtues are by his knowledge brought forth, that man should triumph over pain."

"With what are we not in close alliance wherever the cause of man can be served, his woes assuaged, his condition meliorated? Jurisprudence calls us to its aid; crime can elude the call for justice where the means of detection are confined to legal phraseology, or to human laws, indicating and defining punishment according to testimony. But evidence erected upon human fallibility, discordant statements, the workings of prejudice, revenge, or misconstruction, might fail in justice and in mercy. Innocence might satiate the avenging law, guilt escape its desert, *if there was no surer road to truth*. Under whose touch, then, shall the door of the temple, wherein sits truth enshrined, be opened? Where is the tongue that never lies?—where the accuser and the witness, beneath whose unflinching and irresistible gaze the man of crime stands abashed, and by whose revelations he stands condemned? The *knife*, the *microscope*, the *crucible*—these bring their labours to the courts of justice, and throw them into the common inquest: and yet, the knife of the scientific explorer, which gives to the dead this tongue of truth—the tests of the laboratory, which convert the lifeless mass and its incongruous contents into a living oracle, to confront crime, and pronounce the murderer's doom, are never confided to other hands than ours. It is *medical science that directs that tongue and that eye*; and the *lex non scripta* of the book of nature explains what to the *lex scripta* would be a hidden light."

"The medical man of science allays the wide-spread panic when affrighted cities seek his counsel; and his dictum, like oil upon the troubled waters, calms those hasty outbreaks of popular impulse, so frequently subversive of the best interests of commerce. Social order, in all its bearings, receives the benefit of his judgment, and through his acquirements and moral influence public quiet is insured. But should doubt give way to sad reality, and the dark cloud, portentous with coming woe, threaten to cover the city with its funeral pall, to whom do the people fly for prevention and for preservation? The scientific physician is again at the helm, and dictates the police by which the blow may be either averted, or, if it must fall, shorn of its severity. Passing from the anticipation to the advent of the disease, and you find the medical profession at its post. With it, all distinction is levelled, all appeals equalized. The voice of squalid misery rings the alarm at the portals of its conscience as loudly as the proffered gold of the man of wealth, and as readily is it answered. His benevolence sees no duty save in the exercise of a common charity; his humanity requires no other stimulus than the stricken and the afflicted. For them he braves the lurid heat; he breathes the pestilential vapour; for them he is deaf to the strong appeals of self-preservation, until, alas! too often the shaft averted by his own skill from its fatal destination to another's heart, strikes, in its rebound, the envenomed barb to his own life-blood."

But it is time for us to turn our attention to the reports from the different counties of Alabama embraced in the volume before us. They differ in interest, and in the completeness and value of the facts they communicate; they are all, nevertheless, replete with interest, and will amply repay an attentive perusal.

From the report on the diseases of Line Creek, Montgomery County, we quote the following remarks of Dr. M. G. Merriwether on the treatment of rheumatism. In several very severe cases of the acute form of this disease we have ourselves prescribed, after bleeding, and the full evacuation of the bowels, infusion of the cinchona bark with the most decidedly beneficial results. What Dr. M. says in regard to oleaginous inunction is deserving of attention.

"There were several cases of acute rheumatism. I am satisfied that the quinia treatment is the best possible in this climate. I treat a case of acute rheumatism as I would an attack of bilious remittent fever; that is, so far as regards the constitutional treatment of the two diseases. Calomel and Dover's powder, followed by quinia and Dover's powder, in full doses, will quickly arrest rheumatic fever. That this should be so is not surprising, since it is a well-established fact, that there is not, in the whole *materia medica*, a remedy so efficacious in subduing inflammation, as calomel in combination with opium, in some of its various preparations." "As a local application in rheumatism, I have the limb rubbed with volatile liniment, with gum camphor—oil of turpentine and laudanum in it—and then have it swathed with flannel. I should like to call the attention of physicians to the importance of oleaginous inunction as a hygienic measure, and to its curative effects in all chronic diseases." "To restore the healthy condition of the skin, there is nothing so good as to have it rubbed with a little oil—animal oils are the best, as they are more penetrating, and do not dry up so quickly. Personal cleanliness is necessary to health, but after a liberal application of soap and water to the body, a little oil should be applied to supply the natural oil of the skin." "The ancient Jews, Greeks, and Romans, who probably excelled modern nations in longevity, applied oil to the skin after every bath. The costly ointments spoken of in ancient history, both sacred and profane, were oils highly perfumed. It would be well for the people of our day to follow the example of the ancients in this particular. It was stated when the cholera was in France in 1832, that no one who was engaged as an oil-porter took the disease. Dr. Simpson, of Edinburgh, states that the operatives in the woollen factories in Glasgow are not subject to cholera and tubercular consumption, owing to the fact that the large amount of oil employed in the manufacture of woollens keeps the hands almost reeking with oil. Dr. Tilt, of England, in an article on hygiene, states that when King Augustus asked a centegenarian how he retained his health to such an advanced age, the latter replied: 'I apply oil to my skin, and mead to my stomach.'

"In all chronic diseases the skin is more or less affected; it loses the natural oil so necessary to keep it pliant, and permeable to the escape of perspiration. In all fevers, also, of a low grade, it is very necessary to apply oil to the skin. In typhoid fever, it is an excellent remedy; it should be applied every day, and every third day the whole surface should be sponged over with strong lye of wood ashes before reapplying the oil; the lye should be warm."

Of the remedial properties of the *veratrum viride* in pneumonia, we find the following favourable account given by Dr. W. H. Anderson, in the report on the diseases of Mobile. Speaking of the prevalent diseases during the first quarter of the year, he remarks:—

"Pleurisy and pneumonia were quite frequent, and the latter was often severe. We cannot say, however, that the mortality was any greater than common." "The *veratrum viride*, which enjoyed some reputation the previous winter in the treatment of pneumonia, was again tried, and lost none of its laurels. It was regarded by some of our leading physicians as a most valuable remedy, powerful and immediate in its effects, and quite heroic in its nature. It never failed to reduce the pulse to as low a standard as could be desired, and to make an impression on the disease in its active stage highly desirable to both physician and patient. Experience still proved, however, that the utmost caution was necessary in its administration, and that the patients subjected to it should be always under the eye of the physician or of an intelligent nurse. In several instances extreme prostration followed its use in the medium dose, and great apprehension was caused for the recovery of the patient. We believe, however, that it never proved fatal in a single instance. Your reporter admi-

nistered to a child, twelve months old, a single drop, and in the course of an hour, so incessant was the vomiting, and so great the prostration, that he was compelled to resort to a powerful stimulant to prevent the little patient from sinking entirely."

Of the effects of the *veratrum viride* in typhoid fever, Dr. Anderson speaks as follows:—

"Typhoid fever was more prevalent than it was the previous winter; and, from all accounts, was more easily managed. The *veratrum viride* did not exercise the influence over this disease claimed for it by some of the practitioners of the interior, and of other States. In the hands of several physicians with us, its effects were not appreciable. Others thought it a remedy of some value, though of doubtful use after the third week of the disease. Your reporter administered it in perhaps a dozen cases, but never pushed it to its full extent. He was satisfied that it often acted beneficially, and would not, with his present experience, like to treat a case of typhoid fever without occasionally making use of the remedy in its minimum doses.

"We doubt very much whether the typhoid fever that occurs in Mobile is accompanied with as much abdominal derangement as appears to be the case in many other places. There is certainly a good deal of meteorism, but the ilio-cæcal region cannot be so gravely implicated as it often is in the Northern States, and on the continent of Europe. We have seen many cases run their course, and end in recovery or death, without any undue tenderness in the right iliac region, and we have examined several cases after death, when no lesion of consequence was found among the patches of Peyer. Another reason that lends value to our position is, that hemorrhages from the bowels are rare, even in those cases that end in death. The degree of *tenderness* on continued pressure in the ilio-cæcal region, is a very good index to the amount of ulceration within, and we should like to know from physicians, practising in those localities where the disease is so deadly, in what proportion of cases there is much tenderness and trouble in that region. We cannot help thinking that the reason why the disease is so seldom fatal here, is because there is so little dangerous ulceration of this portion of the bowels."

Dr. J. W. Crawford, in the report on the diseases of Centerville, presents his experience of the *veratrum viride* in fevers generally. He states that, during the past year, he used Norwood's tincture somewhat extensively, and, he thinks, with decided benefit in most cases. "That," he remarks, "it will control the action of the heart to any reasonable extent, and this without much inconvenience from its nauseating effects, my experience with it teaches me; and I am satisfied that it also reduces the heat of the surface, in fevers, more quickly than any other remedy with which I am acquainted—cold water excepted—and that, too, without nausea or much reduction of the pulse. I have, also, in three or four instances where it was the remedy relied on, seen the tongue become clean, the mind clear, and all the symptoms improve, the patient becoming rapidly convalescent, where, from observation in such cases of other remedies, I did not expect it. I have not given it in either intermittent or remittent fevers, being fearful of increasing the tendency these always have to gastric irritability. In two cases of fever, one an adult and the other twelve years of age, both proving fatal, I discontinued it after the second dose, owing to the excessive nausea in the case of the adult, and to prostration without vomiting in the child. To the adult thirteen drops, and to the child seven were given, in two doses each. I am not prepared to say that the fatal result was in either case attributable to the remedy—indeed, my conviction is, that it was not; but neither *perfectly* recovered from the prostration which I attributed to the medicine."

The estimate of the remedial virtues of the *veratrum viride* formed by the authors of other of the reports contained in this same volume of *Transactions* is by no means so favourable as that of the gentleman whose testimony is given above. Thus, Dr. J. C. Marks, in the report on the diseases of Selma, presents the details of an attack of pneumonia treated by the *veratrum viride*, of which he was himself the subject, and remarks that—

"This case, we think, will show, and equally others which we could adduce

bearing upon the same point, that while it—the veratrum—reduces and controls the pulse—the arterial circulation—and sickens a man, even to indifference of life, it yet, in very many instances, exerts no curative influence over disease—does not remove the essential conditions, whatever they may be.”

In the report on the diseases of Sumterville and vicinity, Dr. L. H. Anderson presents some very interesting remarks on the character and treatment of pneumonia, as it occurs in South Alabama. The following are the general conclusions to which he has been led by his personal observations.

“1. The pneumonia of South Alabama is probably less severe than the same disease in more northern latitudes; and the intermittent or remittent fever accompanying it is more dangerous than the pulmonary inflammation.

“2. The treatment should be directed chiefly against the malarious element of the disease. Quinia should be freely given for its antiperiodic effect, and mercury for its influence on the secretions, particularly that of the liver.

“3. An exception to this pathological treatment is to be made when the attending fever is of a typhoid character. Here the dothinerterite is the more important disease, and the pneumonia should be treated with strict reference to it.”

The treatment of the yellow fever as it occurred in Alabama during the year 1853, is very fully discussed by Dr. J. C. Marks, in the report on the diseases of Selma. Of bloodletting he remarks—he is speaking of the violent forms of the disease attended by “extensive and dangerous congestion.” “Venesection exerted the happiest effects in every case, whether employed by myself or others, in which its use was restricted to the form of the disease under consideration, and the robust constitution. Indeed, we know of no remedy whose immediate and necessary influence over the essential pathological state better fits it for such cases, especially in their early stages; and we confess ourselves at a loss to conceive how such a weapon of successful conflict with so terrible a disease could have fallen into disrepute, save only from an indiscriminate resort to it, in mild or low forms, and weak and enfeebled constitutions, or the failure to follow up and perpetuate its good effects over the congestion by the prompt and judicious exhibition of the *great remedy*, whose action in turn it so much favours.”

Dr. Marks here alludes to mercury, some form of which, he remarks, was prescribed in every case; and of all the curative means employed, it was deemed the most reliable. It was customary, he tells us, to precede the use of the mercury, at least when given with a view to its constitutional effects; by cathartics, the warm bath, or bloodletting, or all of these remedies, for the purpose of removing to a certain extent the congested condition of the organs, and thus preparing the system for the prompt absorption of the mercury, and by this means securing its full effect.

“By some,” he continues, “the opprobrium has been made to rest upon mercury, that it is incompetent to the relief of aggravated cases; that these will march on to a fatal issue in defiance of the remedy. We believe the source of this stigma to reside chiefly in the culpable neglect of physicians to employ a preparatory treatment, such as will insure its specific action upon the system. Despite that other irrational and illogical dogma which we also now sometimes hear, that even in such cases where the constitutional effects of mercury are obtained, it is more than probable recovery would have ensued without it, we hazard the remark, that there is no remedy known to medical science the rationale of whose therapeutic action so adapts it to the cure of the disease under consideration, especially in its more violent forms. It exerts a powerful influence on the pathological condition, and removes the congestion through an increase of the secretions, and a permanent stimulant impression upon the capillaries. It is evident, then, that the great desideratum must be in the adoption of such measures as will insure its absorption and permanent specific effect upon the system, without which, of course, its use can be of little or no avail. Let physicians studiously seek the attainment of this end, and we must believe that many of the terrors that now wait upon the disease will soon be no more.”

“As before intimated, we generally commenced the mercurial treatment, for

its specific effects, soon after the operation of our purgative, or the employment of other preparatory measures, and by the exhibition of calomel in small and divided doses. The quantity prescribed was usually about two grains, combined with from two to three grains of quinia and one-eighth grain of morphia, which was repeated every two or three hours, as circumstances seemed to require. It was continued in this manner until salivation was produced, after which we in no case witnessed anything but the happiest results."

"We did not, in this form of the fever, prescribe the quinia, in the large doses which have been recommended, to cut short the attack, because it was not regarded sufficiently paroxysmal in its nature to allow of such a result. By a few physicians, however, this was attempted to be done with the remedy; but no success of consequence, I believe, was achieved. The morphia, when it failed to quiet and allay pain in the doses prescribed with the calomel, was given in larger quantities alone for that object."

"Cups and blisters were, in the early stages, sometimes employed over the stomach and spine as revulsives, and frequently effected the removal of vomiting, as well as determined to the surface. The cups were occasionally applied to the temples and nucha when the head symptoms became troublesome. Sinapisms were not unfrequently used over the region of the liver, and sometimes applied to the extremities.

"In addition to quinia and morphia, which were employed as stimulants through the disease, in the manner mentioned, it frequently became necessary, in the latter stages, to resort to some of the more direct remedies of this class, and we found ammoniated milk punch and port wine, or the oil of turpentine, with ether, spirits of lavender and camphor water, as recommended in a recent number of the *London Lancet*, to meet as near as possible the indication."

Of the "quinia treatment" of yellow fever, Dr. Anderson, in the report on the diseases of Mobile, speaks as follows:—

"With due deference to the opinion of several highly respectable physicians, who oppose what is called the Quinia treatment, we must give our testimony as decidedly in its favour. This treatment was much used in this city. Your reporter and his associate in practice being much prejudiced in its favour, from the experience of former years, used it from the very commencement of the epidemic to its close. They treated upwards of eleven hundred cases; and, without claiming any superiority in success, they feel sure that their tables of mortality will compare favourably with those of any other physicians in the city. They used quinia in almost every case, regardless of age, sex, idiosyncrasy, or any other circumstance. They have every reason to be pleased with their manner of treatment, and, with their present experience, would not exchange it for any other that they have heard of. The marked and almost magic effect of a large dose of quinia at the outset was so apparent, that they should have considered it little short of trifling with human life to have adopted any other treatment. They will not deny that there were cases in which it did no good; in fact, in those cases where there was at the commencement decided congestion of the brain, it may sometimes have done harm; but such cases were very few, and could hardly have been aggravated by any medicine that could be given."

The quinia was, however, never trusted to alone; it was always given in combination with calomel or blue mass. The quinia, we are assured, had a powerful influence in allaying nausea and vomiting. Very often, on a first visit, the patient would be found with a distressing sick stomach, which was "immediately relieved by a large dose of quinia and calomel."

Dr. Anderson believes that the yellow fever, as it prevailed in Mobile during the past year, was contagious; "as much so as smallpox, or any other disease that we know of."

"Within our own experience," he says, "we could enumerate more than one dozen families, that were isolated from the disease, and escaped until late in the season, but took it within a few days after some transient person came into the family, either sick with the disease, or on the eve of being down with it. If such cases occurred only in one or two families, we might pause and doubt; but they happened so often, and under such circumstances, that we cannot attribute them to anything else but absolute contagion. We have heard the

arguments of others, whose capacity for observation we have the highest respect for; but they have failed to convince us that the fever did not spread in *many* families by contagion." "A family living eight miles from Mobile, in a healthy pine country, has no communication with the city, and remains in perfect health until the middle of October. About this time, a stranger arrives, just from the city; he is seized with the fever, and in a week from that time four or five of the family are taken down, and half the number dead with black vomit."

Startling as these facts may at first sight appear, they cannot be received as conclusive evidence of the contagiousness of yellow fever. To determine their full and true bearing upon the question at issue, there are a number of collateral circumstances to be taken into account, of which Dr. A. has left us in entire ignorance, that, we are convinced, would materially change their etiological import.

Dr. Marks, in his report on the diseases of Selma, bears very different testimony on the question of the contagion of yellow fever to that of Dr. A. He assures us that—

"There was nothing in the history of the epidemic that could, by the most favoured construction, be made to support the opinion of the contagionists; but, on the contrary, everything was presented that could be desired to confute and overthrow it."

"It has long," he further remarks, "been a vexed and contested question, whether, between yellow fever and our ordinary periodic fevers, there be not an identity of cause; or, in other words, whether the same local conditions which engender intermittent and remittent fevers, may not also originate yellow fever. Now, connected with the origin of yellow fever here, there are some circumstances which seem to favour such a view, while other reasons and facts stand in opposition. It is well known that, while yellow fever generally prevails where periodic fevers are common, yet that it occurs only at long intervals or on rare occasions, and is not usually produced when the causes engendering endemic fevers are in operation. If, then, it be produced by them at all, it must be when they become exceedingly active and powerful. Now, in the present instance, before the conclusion of an identity of cause can, we conceive, be logically deduced, we must prove, not only that the ordinary causes of intermittents and remittents, such as malarial influences, hygrometric conditions, &c., were much concentrated and intensified ere yellow fever manifested itself, but, also, that a similar concentration obtained at other places where it prevailed. Should we be incapable of doing this, we are forced to admit a new, and, as we have called it, atmospheric condition, as alone requisite to its production, or at least conspiring with and aiding the common causes of fever.

"We think we may show that there was some concentration of our endemic causes here, at the time yellow fever appeared, as exhibited not only by the greater violence and malignancy of our common periodic fevers, but also from the prolific source of malarial emanations in our midst. We had at that time a general pulling down and upsetting of old filth-incrusted houses, and, besides, a great deal of dirt and decayed vegetable matter thrown up in our streets, especially the one bordering on the river, along which were several large buildings in the course of erection, and to obtain a foundation for which extensive excavations were made; thus, in different ways, exposing to the desiccating and evaporating action of the sun, old pestiferous decomposing matters, which had accumulated in the city for years. Now, while the impure malarial air, in this way engendered, has long been regarded as a fruitful source of disease, and even of yellow fever, yet, as we have every reason to believe the disease occurred at other places where no such agencies, nor even other causes of concentration were present, we cannot conclude that our ordinary causes of fever, heightened as they were, alone produced it, and are, therefore, led to the conviction that an atmospheric epidemic influence of some kind was, if not essential, certainly accessory in its development."

An able and valuable report on goitre, by William Taylor, of Talladega, is replete with interesting facts in regard to the particular localities in Alabama, in which that affection is endemic.

So far as his own observation extends, and so far as he has been able to obtain

information from others, Dr. T. has found goitre to prevail to a much greater extent in the silurian and metamorphic regions of the State, than in other portions of it; and that, while it prevails in the silurian districts to a greater extent than can be readily accounted for by sporadic causes, it prevails to a still much greater degree, in point of frequency, in the metamorphic districts.

"But," remarks Dr. T., "it does not appear that goitre is found alike in all parts comprised in the metamorphic system. In Talladega, the disease does not seem to prevail to so great an extent in the metamorphic region of the country as in some parts of the silurian; while in parts of Randolph County, which is wholly metamorphic, it is becoming so prevalent as to demand the serious attention of the profession. In Fishhead Valley, in Randolph, where the disease is, perhaps, as prevalent as in any other part of the county, much of the water, on standing, precipitates a deposit of mica, similar to what I saw in the water-vessels at Mindereri and Masaya (South America). Yet I do not hold that this agent has any influence, either negative or positive, in producing the complaint, although I have never seen it prevalent, except in the older systems. This fact holds true, so far as my own limited observation extends on this continent, and I doubt not but the same holds with regard to the old world. I have interrogated several intelligent Englishmen in regard to the goitrous districts of England, all of whom tell me that the shires of Nottingham, York, and Derby are metamorphic in their geological aspect, and abound in mica. But if, as M. Chatin reasons, goitre is dependent on the absence of iodine in the water, then the absence or presence of mica becomes a mere adventitious coincidence. Indeed, I would not pretend to assert that mica had any direct agency in producing goitre, but merely mention it in connection with the disease; because I believe there is as much ground for assigning it as the cause of the malady, as there is for attributing it to lime, magnesia, and divers agents, to whose influence it has, at different times, been charged. And if future observation and experience should confirm the suggestions and reasoning of M. Chatin, then will goitre and the more primary systems be found to have a relative connection, because little iodine is found in these systems, while it is more liberally diffused throughout the more recent formations. In other words, we may expect to find the disease confined principally to the older formations; while, on the other hand, it will rarely be found occurring in systems of more recent origin."

"From facts, the causes of which are as yet unexplained, there are reasons for supposing that the metamorphic formation offers a more prolific field for the production of goitre than the purely primitive; since it is a remarkable fact that countries where goitre prevails to any considerable extent, are decidedly metamorphic. In the central Swiss Alps, notorious throughout the world for the production of goitre and crettenism, Sir Charles Lyell asserts, 'the primary fossiliferous, and older secondary formations disappear, and the cretaceous, oolitic, and liassic strata graduate insensibly into metamorphic rocks, consisting of granular limestone, talc-schist, talcose gneiss, micaceous schist, and other varieties.' This condition of things exists in all the goitrous districts which we have visited, and we should hesitate but little in venturing the opinion that the same holds true in every part of the world where goitre is known."

The following are the general conclusions at which Dr. T. has arrived, from the facts and reasoning set forth in his report:—

"1. That goitre exists, in some sections of the State, to the extent of about *one per cent.* of the population, and will, in time, unless proper measures are adopted to prevent it, assume an endemic form.

"2. This disease being confined principally to the older geological formations of the State, it will, in all probability, never prevail to any considerable extent in the more recent systems.

"3. That the cause of goitre is pretty certainly traced to the character of the water used in the infected districts.

"4. There is strong reason to believe from the observations of M. Chatin, and the scarcity of iodine in goitrous districts, that the disease is not produced by any positive agent, but has its origin from the want of a due amount of iodine in the waters of said districts.

"5. That goitre is not found in regions where the waters are impregnated with an appreciable quantity of iodine; hence it will never exist, to any considerable degree, in the tertiary and cretaceous systems, nor the coal meadows of our State, since iodine is abundantly diffused throughout these formations.

"6. Iodine is the remedy, in the treatment of goitre; and this fact, under the circumstances, is strong corroborative evidence that the disease originates from the want of that agent; and if the absence of iodine is the cause of bronchocele, it is but reasonable to expect that great good may be obtained by impregnating with it the water used as drink, as a prophylactic measure in goitrous districts. May not the remedy in time become as certain a preventive of goitre as vaccination is of smallpox.

"7. Iodine does not seem to form any chemical combination with the blood, nor with any of its elements; but seems to impart something to the system—the lymphatic system especially—necessary to the maintenance of its healthy functions.

"8. Surgery has done but little for the relief of the goitrous patient. Extirpation has been attempted, with unfavourable results; it is now totally abandoned by the profession. The seton has been resorted to with limited success. The formation of an abscess in the tumour has, in some instances, resulted in a spontaneous cure; tying the thyroidal arteries has also been attended with partial success.

"9. Certain gum resins are applied externally in South America, and also in some parts of the republic of Central America, with reputed efficacy."

Besides the regular reports from the several counties of the State; that on goitre, noticed above; a report on the botany of Wilcox County; reports on the number and character of the physicians of Dallas and Bibb counties, the volume of *Transactions* before us contains an account of a case of distorted pelvis, in which abdominal section was practised, followed by death from nervous shock; another of leucorrhœa in an infant twenty months old; a paper on the unity of disease, by Dr. H. Backus, and one on the pathological relations of the organic nervous system, by Dr. M. Troy. These last two, though highly creditable productions, and not devoid of a certain degree of interest, are out of place, in our opinion, in the *Transactions* of a State Medical Society, the communications to which should be confined, as much as possible, to a record of the personal observations of its members on the several questions connected with the pathology and treatment of the diseases prevailing in their respective localities, whether as endemics or epidemics, with a comparison, it may be, of their own observations and conclusions with those already recorded by others.

2. The *Transactions of the Medical Society of the State of Pennsylvania*, at its annual session of 1854, in their intrinsic value and general interest fall far short of those of former years. It is with surprise and deep regret that we perceive that in so small a portion of the counties the medical profession is represented at the sessions of the State Society, and that of those represented, so few present full and satisfactory reports, as a contribution to the common stock of medical knowledge. To what cause this apathy to the best interests of the profession, this neglect to perform what should be considered a pleasing task as well as a positive duty, is to be attributed, we pretend not to determine. It is to be hoped, however, that efforts will be made to effect, at an early date, a complete organization of the regular physicians in every county in the State; and that, when such an organization is accomplished, a full representation from each county will be found in attendance at the yearly meetings of the State Society, prepared to report on the character, causes, and usual course of the diseases that occur in the several sections of country included within their respective limits, and the plans of treatment which experience has shown to be most successful in conducting them to a favourable termination. A very common excuse offered by physicians residing in the interior of the State, remote from any large town or densely settled neighbourhood, for their neglect to become active members of the medical society of their county, is the little leisure which the hard labour

and constant travelling of a country practitioner leaves him, even to attend the meetings of the Society, much less to communicate to it in writing, the results of his observations and experience in relation to the several cases of disease which fall under his notice. That in some cases, the country physician may not be able, without a very great sacrifice, or very considerable inconvenience, to attend regularly the meetings of his county society, we can readily suppose;—we must, however, insist that the advantages to be derived from such attendance, will, in nearly every instance, amply repay the sacrifice and inconvenience it occasions. But we cannot admit the plea of want of time and opportunity as a valid excuse, on the part of any physician, for not recording his personal observations on the diseases he is called upon to treat, and his experience in reference to their proper treatment. Short, pertinent, and accurate notes are all that is required; for the recording of which an opportunity may be found amidst the occupations and cares of the most extensive and wide-spread practice—without interfering in the slightest degree with the duties the physician owes to his patients, or encroaching upon the time necessary for his rest and refreshment. Were such notes to be furnished, by even a majority of the members of a county society, to the committee appointed to draw up its annual report to the State Society, we should no longer be under the necessity of hearing the constantly recurring and mortifying apology for the incompleteness of these reports, the want of the materials requisite to furnish a complete and accurate medical history of the county for the years they embrace.

The annual address by the president, Dr. John P. Hiester, of Reading, is on the means of suppressing the evils of quackery.

We entirely coincide in opinion with Dr. Hiester, that these evils can never be totally eradicated, even under what may be considered the most efficient means, and by the wisest and most persevering efforts. All that we can reasonably expect to attain, is to bring the evil under wholesome restraint.

One of the leading means for staying the evils of quackery in our midst, is the cultivation by the profession of everything calculated to increase its legitimate influence with the public. By the proper organization of its members, with the view of insisting upon the moral and educational qualifications of all who would enter its ranks, and their strict adherence, subsequently, to a code of ethics, clearly defining their duties to the community generally, to their patients, and to each other. Let the entire profession raise itself to its proper standard in education, scientific attainments, and skill; in moral worth and gentlemanly deportment. Let its members be truly united among themselves; banishing all petty jealousy, and every mean art of rivalry, and prepared mutually to defend each other's rights and interests, and a more effectual barrier we shall have erected against the progress and encroachments of quackery than can be expected from penal enactments, the shafts of ridicule, or the most convincing arguments addressed to the public ear.

We agree with Dr. Hiester, as to the beneficial results to be anticipated from a general diffusion, through the medium of our public and private schools, and by popular courses of lectures to the members of the several institutes and lyceums of our towns and cities, of a knowledge of the structure and functions of the human body, and the laws of health and disease deducible from such knowledge; though we are not so sanguine as he is, that by this means a "radical remedy" to quackery will be put in operation. The adaptation of therapeutic agents to meet the various and varying conditions of disease, can never be understood by those out of the profession; and the cunning, intelligent quack, and we have, unfortunately, such in our midst, will base the success of his nostrums upon this very ignorance, let a general knowledge of anatomy, physiology, and pathology be ever so widely diffused.

The reports contained in the present volume of *Transactions* bear testimony to the very general exemption from disease, especially of an epidemic character, enjoyed by the entire State.

Although the question of the relationship of particular diseases to different geological formations has, as yet, not been examined with any degree of care, yet, so far as any notice is taken of it in the reports before us, it is very evident, in relation to the dysentery, at least, that it may prevail to as great an extent

upon the limestone, as on the gravel, or old red sandstone, contrary to the inference derived from the statements presented in some of the reports of former years.

The diseases that prevailed most extensively during the year 1853, in those portions of the State from which reports are presented, were dysentery, typhoid fever, rheumatism, mumps, whooping-cough, and erysipelas.

In Logan's Valley, Blair County, Dr. T. M. Confer reports the prevalence of mumps to have been more extensive than for many years previously.

"Its first appearance was in the early part of December, and, by the end of the month, some two hundred persons had suffered from it. These were generally the young, between the ages of two and thirteen years. Some few cases were met with in adults. It was carried from the public schools, and so generally distributed, that every family in the township was visited by it. In three cases that I noticed, the glands were suppurated. This process was confined to the superficial integuments, and over not more than one gland in the same patient. Many of the cases were attended with violent fever and delirium, continuing for two and four days, and requiring an active antiphlogistic treatment. Several experienced a second attack, though with less violence than the first. These were adults, having large families, and being constantly exposed. Some few cases of metastasis were noticed; three to the testes, and one to the brain. This latter case occurred in a scrofulous subject, who had glandular enlargement previous to the attack of parotitis. The inflammation passed gradually from the salivary glands to the mucous coat of the eye on the same side, then to the opposite eye, and finally to the brain; when it terminated fatally in five days and a half, notwithstanding every application was made use of to invite back the disease to its original location, and to arrest its progress in the brain.

"In one of the cases of metastasis to the testes, suppuration took place, and by the neglect of the patient to make known his real situation to his parents, the left testicle was destroyed by the suppurating process." "When first seen, the part was discharging profusely, and the suppurative process, for a time, seemed to threaten the extinction of the opposite testicle. This condition of things finally yielded to an appropriate treatment, and the patient was restored to health, with the loss of one testicle."

"The suppurating process in parotitis, which, we believe, is of rare occurrence, is generally superficial, seldom extending further than the integuments, which must render this case one of singular interest; yet we believe it may be accounted for by the exposure of the patient to cold, and the fatigues of labour, the friction of his clothing, combined with the accumulated filth, acting as constant sources of irritation to the inflamed gland."

In the Report from the Medical Society of Berks County, we find the following statement:—

"Dr. Greis mentioned, at the last meeting of our County Society, that he regards erysipelas as almost entirely a constitutional affection, and that he so treats the disease; after having evacuated the bowels with a mercurial purge, he restores the secretions by the use of the *ammoniated tartrate of iron*, in the dose of grs. viij, every four hours; and if much prostration exists, every two hours. From its ready solubility and palatable taste, it forms an excellent preparation for children, and, according to the doctor's experience, is more efficacious than the muriated tincture of iron."

A most interesting communication occurs in connection with the report of the Medical Society of Montgomery. It presents the detail of a number of cases of erysipelas occurring in children subsequent to vaccination, by Dr. Hiram Corson. These cases are certainly of a very strange character. Several of them, in connection with the local disease, which made its first appearance in the neighbourhood of the part at which the vaccine virus had been inserted, presented a series of symptoms indicative of a serious affection of the brain and upper portion of the spinal cord. Unfortunately, no *post-mortem* examination was made in either of the fatal cases. Of the true cause of the erysipelatous attack in Dr. Corson's cases we can form no accurate opinion. Excepting, perhaps, as a mere exciting cause of disease in subjects otherwise predisposed, we cannot suppose the vaccination to have had any agency in its production. Thirty-one children

in the same neighbourhood were vaccinated with the same matter. Of these, eight only were attacked with erysipelas; and of these eight, five died.

"The last fourteen of the children," remarks Dr. C., "were vaccinated on the mild days preceding twelve days of northwesterly wind, which occurred in March last, beginning on the 25th, and continuing without intermission, until the last of April. This was a remarkable spell of weather; the wind never shifted, but continued steadily to blow for twelve days; was very dry and cold.

"On the very day on which this change to cold began, one child, then only vaccinated two days, was seized with convulsions. On the next day, or rather on the night of the second day, three others sickened, of whom two died. On the next day, another sickened in the morning, and died at 11 P.M. On the following day, two others began to be affected with erysipelas of the arm; one of whom died at the expiration of two weeks from the time of vaccination, and all sickened during the continuance of the cold weather.

"At the time those children were vaccinated, I was attending a woman who had been ill with erysipelas since the 14th of February, supervening on a small injury of the toe-nail, and which had involved the foot and leg almost to the knee, when I was called in; and finally involved the whole thigh, suppuration taking place deeply, throughout the whole limb; and it is only now, May 1, fairly free from the discharge of matter. This was the only case of erysipelas within my town until April 15, and was one mile distant from the nearest of these cases, and two miles from many of them.

"On the 3d of April, a child next door to two of the patients, aged nine months (female), was scalded on the arm, and slightly on the neck. On the 14th, those scalds were nearly well, and erysipelas began to show itself on the very crown of the head, from whence it spread rapidly, involving, in two or three days, the whole scalp and nose, with portions of the cheeks. Used mercurial oint. and mur. tinct. of iron. Recovered. I also attended several other small children, in the region about, suffering with slight catarrh, with thin, green, watery stools. But all of them recovered save one very small, weak twin, four and a half months old.

"I have been informed by my friend and neighbour, Dr. Samuel Smith, that, during the illness of these children, he was called to a child of only a few months old, with sickness, green stools, and other symptoms similar to those which existed in the small children mentioned by myself; that in four or five hours after the invasion of sickness, erysipelas showed itself on the point of the nose, and spread rapidly; and the child died in nine hours from the time of its attack, with symptoms of disease of the brain. This case was two miles from any of mine.

"That the matter was pure and good, I have not the least doubt; for a great majority of those vaccinated took the disease well. In two instances, when two children of the same family were alone vaccinated, in each case one took the erysipelas. The disease was severe almost always in proportion as the child was young, and milder as it had more age; and yet, some of the quite young ones escaped, whilst some of even five and six years took the disease."

While the majority of the reports embraced in the present volume of *Transactions* contain much matter of an interesting and valuable character, the most complete and satisfactory of the series is unquestionably the one from the Medical Society of the County of Philadelphia. We have, however, already extended the present notice to too great a length, to permit us to enter upon an examination of the facts and observations it presents in relation to the more prevalent diseases of the period it includes.

3. The volume of *Transactions of the Medical Association of Southern Central New York* contains the history of a few cases of disease which are not devoid of interest, but no report presenting any prominent facts and observations demanding especial notice. But two reports, properly speaking, appear, in fact, to have been made at the session whose transactions are here detailed; namely, one from the Committee on Surgery, from Chemung County, and the other from the Committee on Endemics and Epidemics, of Tompkins County; neither of them very full or satisfactory.

The annual address by the President of the Association, Dr. Frederick Hyde,

is a creditable production, and appropriate to the mixed audience before which it was delivered. Its main topic is the importance of every one acquiring a correct knowledge of the human physical organization, and the vital laws by which that organization is governed; in order that each individual may be enabled to pursue a plan of treatment, which shall encourage a healthful development of all its powers, and preserve them in their full and vigorous exercise for the longest period, thus securing his or her own comfortable existence, and the transmission of a perfect organism to his or her offspring. In the close of the address, are set forth the paramount claims of legitimate medical science as carried into practice by the well-instructed and conscientious physician to public confidence and protection. Both themes are judiciously treated, and strongly enforced.

An Essay on Opium and the Sulphate of Quinia as Remedial Agents, by Dr. Nelson Nivison, demands but a passing notice. To believe the writer, these two articles, in varying doses, either separately or conjointly administered, and appropriately timed, are of themselves adapted to the arrest of fevers, inflammations, and a host of other diseases—that there is scarcely, indeed, “any species of disease in which, either primarily or secondarily, in some stages of its progress,” either one or both of these remedies cannot be made to subserve important purposes.

All that Dr. N. advances, in general terms, may be strictly correct, and yet we fear that, neither in principle nor in practice, the views he inculcates in the essay under consideration, will be found to advance in any great degree our acquaintance with the therapeutical application of either of the remedial agents indicated.

An Essay on Medical Education, by Dr. Daniel Holmes, breathes the true spirit, and utters correct sentiments on this all-important subject. A subject we are pleased to find that is attracting the notice of our State and local societies—for it is through their influence and co-operation alone, that any reform in medical education can be effected. Let the members of the profession throughout the Union unite in the demand for the requisite qualifications in those who enter into its ranks. Let them carefully point out to their pupils the nature and extent of those qualifications, and conscientiously aid them in the means for their attainments, by personal instruction, and by affording them every opportunity for the study of disease within their reach, and finally, when prepared to profit by a course of medical lectures, preparatory to graduation, to indicate to them the institution best adapted to complete the thorough course of instruction commenced under the private preceptor's guidance. Let this be done, and the desired reform in medical education will be speedily accomplished; but not until then.

Dr. Holmes presents some pertinent remarks on the deficiencies of the clinical instruction afforded to the medical student in this country.

4. At the Session of 1854, of the *Medical Society of the State of North Carolina*, the subject of the annual address by the President, Dr. J. B. Jones, was, “The manner of man's relation to some of the many destructive agents by which his existence is often terminated.” His views on this subject will be understood from the following paragraph, which occurs at an early part of the address:—

“But for his nervous system, and the connate objects of its control, man would escape the injurious or fatal action of all that class of agents from which the vegetable world suffers no inconvenience. This fact plainly declares that nerve is the medium through which he is placed in relation with by far the majority of morbid causes. We read accounts of primary lesions of the blood, secretion, &c., in which accurate descriptions of these changes are given. But if the blood is a vital fluid, as dependent upon innervation for its normal properties, as nerve matter is upon blood for stimulus and supply, a lesion of innervation is a necessary element in the definition of lesions of the blood.”

“Poisonous and morbid agents,” according to Dr. Jones, “produce death or disease by their action primarily upon the nervous system.” Before he closes he admits, however, that the action of all morbid agents cannot be accounted for in this manner. Thus, he says:—

"Any substance entering the blood, and impairing or destroying the integrity of one or more of its constituent elements, impairs, or robs, to a corresponding extent, the organs which are dependent upon such product for their existence and functional capacity; and this is no doubt the mode of relation between individual structures and some poisons and therapeutic agents."

The second article in the volume, is a long address in opposition to the commonly received doctrine of malaria as a cause of disease—chiefly, however, in reply to some strictures of Dr. Satchwell, on a former address on the same subject, by Dr. Charles E. Johnson, the author of the present article.

The address, though in many respects an able one, presents no new facts or arguments bearing upon the subject discussed, and is not, in our opinion, suited to the *Transactions* of a State Medical Society. Positive contributions to medical science, derived from the personal observations and investigations of the members of the several county societies, digested and arranged by competent committees, are what we look for in these *Transactions*; and we are very properly disappointed, when we find, in place of these, theoretical disquisitions leading to no positive results.

A short letter from Dr. James J. Philips follows, On the importance to the physician of the study of the standard medical works that issue from the press, and of the valuable contributions to be found on the pages of our professional journals.

The volume closes with "An account of some of the early proceedings of the Society, and the Code of Ethics of the National Association." D. F. C.

ART. XVI.—*Reports of American Institutions for the Insane.*

1. *Of the Mt. Hope Institution*, for the years 1849, 1850, 1851, 1852, and 1853.
2. *Of the Vermont Asylum*, for the years 1852 and 1853.
3. *Of the Maryland Hospital*, for the year 1852.
4. *Of the South Carolina Asylum*, for the year 1852.
5. *Of the Louisiana State Asylum*, for the years 1849–50, and 1850–51.
6. *Of the Butler Hospital*, for the year 1853.

1. THE Mount Hope Institution, near Baltimore, is the largest and one of the most successful of what can properly be termed the *private* establishments for the insane, in the United States. The principle upon which it is conducted, as represented in the report for 1849, is truly benevolent. It "has never obtained from the city or State the smallest pecuniary assistance, or from individual benevolence the slightest endowment," yet, "whilst burdened with a heavy debt, it appropriates one-fifth of its room to the maintenance of destitute lunatics." Persons of all religious persuasions are admitted, and participate "on an equal footing in all its benefits."

	Men.	Women.	Total.
Insane patients, January 1, 1849	21	38	59
Admitted in course of the year	45	41	86
Whole number	66	79	145
Discharged, including deaths	38	32	70
Remaining, January 1, 1850	26	48	74
Of those discharged, there were cured	19	13	32
Died	8	6	14
Cases of <i>mania-à-potu</i> , January 1, 1849	3	0	3
Admitted in course of the year	77	11	88
Discharged, recovered	75	11	86
Died	3	0	3
Remaining, January 1, 1850	2	0	2

The special condition resulting in death, in the cases of insanity, is not mentioned.

¹ There are some discrepancies in these figures which a reviewer cannot correct.

Three cases of what is termed "febrile delirium" were received, and all terminated fatally. We are bound, of course, to assume that these were what the term imports under which they are recorded, but since many cases of a disease which is now generally believed to be distinct from ordinary febrile delirium have been reported, at various institutions, under the different terms "typhomania," "phrenitis," "exhaustive mania," "Bell's disease," &c., and, particularly, since Dr. Stokes gives the following description of his cases, we cannot forbear to presume that some of them belonged to the form of disease mentioned. "In these cases," says he, "there appeared to be a kind of inflammation of the membranes of the brain, accompanied by a general and intense impairment of all the faculties of the mind. This is a condition, often, we think, mistaken for insanity. It usually assumes the form of maniacal excitement, sometimes preceded by a short period of melancholy, or depression of spirits. Patients affected by it are remarkable for the rapidity of their movements, incessant talking, and sleeplessness. The pulse is frequent, but not hard or full, the tongue is coated, and the eyes often slightly suffused. Such cases often prove fatal very suddenly, and after death the pia mater and the arachnoid membrane will be found considerably more vascular than natural, and occasionally some effusion of serum will be observed. Death does not seem to result from inflammation, but from the exhaustion and sinking of the vital powers after long-continued excitement, agitation, and sleeplessness." Yes; and hence it has been found, in most of the northern asylums, that the pretty free administration of stimulus, as wine or brandy, is the only method by which death, in these cases, can be averted, and the patient restored to health. The Italics in the quotation are ours. Some of them denote symptoms, which, to us, do not appear fully to correspond with those of ordinary febrile delirium.

Of the one hundred and forty-five insane patients, ten had the suicidal propensity. "For a long time during the spring and summer, almost every patient brought to us was represented by the friends to be strongly suicidal. None succeeded in carrying their plans into execution.

"The mode of self-destruction in asylums is, for the most part, by hanging. The particular way, however, generally appears to be a matter of much thought and consideration. But, after the plan is once settled, they seem to neglect all other means which may offer themselves, until they have an opportunity of perpetrating the deed in that particular way.

"There is no subject connected with the history of insanity, in which more crude, ignorant, and mistaken notions are entertained, than in what is termed *religious madness*. * * * * The best authorities on the subject declare that there is reason to believe that the number of persons who become insane through the influence of religious hopes and fears, is much less than is generally supposed. As far as our own observation goes, we have never yet succeeded in tracing any one case unequivocally and directly to this cause. * * * * The circumstance that the mind of the lunatic is occupied, during the period of his disease, with ideas and feelings connected with an invisible world, is no proof whatever that the derangement of his understanding was produced, in the first instance, by impressions related to the same subject."

From the report for 1850:—

	Men.	Women.	Total.
Insane patients, January 1, 1850	26	48	74
Admitted in the course of the year	32	40	72
Whole number	58	88	146
Discharged, including deaths	33	32	65
Remaining, Dec. 31, 1851	25	55 ¹	80 ¹
Of those discharged, there were cured	16	15	31
Died	3	5	8
Cases of <i>mania-a-potu</i> at beginning of year	2	0	2
Admitted	59	11	70
Discharged cured			65
Remaining at end of year		2	2
Deaths (inferred from the above—not stated in report)			5

¹ Deducted from the preceding figures, these would be 56 and 81.

Several pages of this report are devoted to the purpose of the prevention of insanity, by pointing out the symptoms, and suggesting the proper treatment of the incipient stage. The psychic symptoms are included under the general terms of *changes in the disposition, temper, and affections*. The writer then proceeds as follows:—

“There are certain physical symptoms which will serve to point to the nature of the threatened mischief, and guide the treatment. The patient complains of a sense of tightness or constriction across the forehead, sometimes attended by noise in the ears, flashes of light, flushing of the face, &c.; by a state of watchfulness by night, and restlessness by day; by costiveness, by gastric and hepatic derangement. The inability to sleep is a symptom which ought never to be neglected. It may be considered one of the most valuable indications we possess of approaching insanity. It seldom deceives. Whenever this state of watchfulness by night and restlessness by day occurs in connection with some of the foregoing manifestations of an altered state of the feelings and affections, not another moment is to be lost. Now it is that much may be done by the ordinary family physician. He ought carefully to scrutinize the state of the general functions, and immediately set about rectifying morbid action wherever located.

“Accumulations in the alimentary canal must be removed by purgatives; the secretory and excretory organs must be duly regulated by the warm bath, and by diuretics. Cerebral irritation, and turgescence or morbid excitement of the capillaries of the part, must be subdued by local depletion, leeches to the temple, cups on the nape of the neck, &c. Sleep must be restored by the cautious administration of Dover’s powders, or morphia, at night. But on no account should they be resorted to until preliminary steps are taken to subdue the lurking mischief in the brain.

“As soon as possible after these points have been attended to, the patient should be sent on a journey, in order to divert the mind, and to change the whole current of thought.”

Theatrical entertainments, or “exhibitions,” have been introduced as a part of the amusements of the patients at Mt. Hope. “Many who take no part in these performances, are amused by witnessing them, and imperceptibly get their feelings enlisted in the success of others. By such recreations as these, we have known, in a considerable number of cases, melancholy feelings and insane delusions to be dispelled, and recovery to take place.”

In view of the prevalence of delirium tremens, and of the influence of intemperance in the production of other forms of mental derangement, Dr. Stokes urges the importance of legislative action for the reformation and control of inebriates. He would have them removed to houses of refuge, where they should be subjected to medical treatment and moral discipline, the term of their detention to be limited only by the decision, in each individual case, of a proper medical or judicial tribunal.

Report for 1851:—

	Men.	Women.	Total.
Patients at the commencement of the year	25	55	80
Admitted in course of the year	50	52	102
Whole number	75	107	182
Discharged, including deaths, and one escape	38	46	84
Remaining at the end of the year	34 ¹	64 ¹	98
Of those discharged, there were cured	12	17	29
Died, including one suicide	8	6	14
Cases of <i>mania-à-potu</i> , January 1, 1851		2	2
Admitted	53	1	54
Discharged cured			52
Died			2
Remaining			2

¹ According to the preceding data, these should be 37 and 62. In one place the deaths are reported 8 males, 6 females; in another, 9 males, 5 females.

Of the insane patients, two died of acute meningitis, and two of febrile delirium.

In the table of causes, seven cases are attributed to "defective education;" and from the remarks upon this subject we make the subjoined extract:—

"In most of these cases, we are constrained to say, the too fond parents have unconsciously sown the seeds of this most bitter fruit. Too great indulgence in childhood, and previously to, as well as during puberty, and a want of moral discipline then, and up to maturity, with neglect of that education which inculcates, and indeed imposes proper principles of feeling and action, are undoubtedly among the most deeply-laid foundations of insanity. Persons thus brought up have their temper, emotions, and moral affections so little under command—are so subject to ebullitions of passion, to capricious or violent and fugitive emotions—are so liable to act from momentary feeling and impulse—as to acquire a disposition of mind not only most unamiable in itself, but also most prone to marked disorder when subjected to its more immediate productive causes. * * * * In young girls, even though there be no predisposition from a constitutional taint, faulty education will itself originate and develop that exalted degree of nervous susceptibility and irritability which, in later life, may eventuate in *moral insanity*."

The doctor thus gives his opinion in regard to depletion:—

"To bleed and otherwise deplete copiously for insanity alone, even in its early stages, is a highly injurious practice. Without having the least influence over the delusions of the patient, it lowers the grade of his vital forces, and renders him less able to support the more or less wearing influence of the mental disease on the bodily health. * * * * It greatly disposes to a more rapid decline of mania or monomania into dementia. * * * * It diminishes the chances of subsequent cure, and very generally serves to protract the duration of the disease. In our estimation, depletion is the great error in the primary treatment of insanity. * * * * We could point out more than one case now in this house, on whom the detraction of blood, injudiciously resorted to, has inflicted irreparable mischief."

Report for 1852:—

	Men.	Women.	Total.
Insane patients, January 1	34	64	98
Admitted in course of the year	41	42	83
Whole number	75	106	181
Discharged, including deaths	44	42	86
Remaining, December 31	33 ¹	62 ¹	95
Of those discharged, there were cured	19	23	42
Died	3	5	8
Cases of <i>mania-à-potu</i> , including two at beginning of the year	64	8	72
Discharged recovered	58	7	65
Died	5	1	6
Remaining, December 31	1	0	1

"During the last year," says the report, "a larger number than any previous year have been admitted in a state of raving madness, attended with excessive excitement and violence. During the continuance of the maniacal paroxysm, the object aimed at in the medical treatment has been to calm undue nervous susceptibility and irritability, without impairing the vital energy, or exhausting, by reducing remedies, the vital powers. No case of acute mania has occurred in which any depletory measures have been adopted, either by bleeding or other antiphlogistic remedies, save aperients and diuretics, with moderate local depletion, which are generally required in order to remove vitiated secretions, to clear the eliminatory organs of obstructions, and to lessen local congestion. Indeed, it is now an admitted axiom by physicians of insane asylums in general, that in this malady *inflammatory action* in the brain is of exceeding rare occurrence. It would seem as if all the energies of the constitution were directed to

¹ Thirty-one and sixty-four, if the antecedent numbers are correct.

the full development of the mental disorder, and diverted from the processes whereby inflammatory action is established and maintained. * * * * We have invariably endeavoured to support and succor the constitution, and thereby to fortify it against the debilitating influence of long-continued excitement, agitation, and raving. * * * * Cases continue to be admitted in which general depletion has been lavishly resorted to."

Dr. Stokes gives us a table of the ages of the patients, by which it appears that there were 39 between 20 and 30 years, and 69 between 30 and 40. He refers to the assertion that in Europe the predisposition to insanity is greatest in the decennium from 30 to 40 years, and remarks that "in France, it appears certain that persons are more liable to insanity between 30 and 40 years of age than at any other period of life." He quotes from the statistics of the Pennsylvania Hospital for the insane, where 44.87 per cent. of the patients were between 20 and 30, and only 18.9 per cent. between 30 and 40; those of the Ohio Asylum, where there were 43.97 per cent. between 20 and 30, and but 24.52 per cent. between 30 and 40; and of the Bloomingdale Asylum, where the number between 20 and 30 was "much larger" than in any other decade. He then remarks that "the inference to be deduced from our table is *more in accordance with the results of the asylums in France*, inasmuch as the largest number appear to have been attacked between 30 and 40 years of age." Let us ask Dr. Stokes this question: Does your table show the ages of your patients at the time of the first attack of insanity? If it does, your institution presents a most remarkable contrast, in this respect, to all others in the country. It is a perfect anomaly. Your table has this caption: "*Showing the Ages of Insane Patients from January 1, 1852, to January 1, 1853.*" The reader would hence infer that it exhibits the *present age* of all your patients (for they are all included) irrespective of the number of years they had been in the asylum—regardless of the prior duration of the disease. In France, and generally in other parts of Europe, the ages upon which the calculation is based are those at the time of admission of the patients. Supposing that your table represents the ages in 1853, it is not remarkable that the results should coincide with those of the French asylums, rather than with those of the three American institutions mentioned, where the computation was based upon the *ages at first attack*. Have you not attempted to compare things which, from their very nature, are not susceptible of comparison?

Report for 1853:—

	Men.	Women.	Total.
Insane patients, January 1	33	62	95
Admitted in course of the year	48	72	120
Whole number	81	134	215
Discharged, including deaths	36	47	83
Remaining, December 31	45	87	132
Of those discharged, there were cured	15	15	30
Died	8	10	18
<i>Mania-à-potu</i> cases, including 1 from 1852	20	1	21
Discharged cured	18	1	19
Died	2	0	2

From the tables of the "civil condition" of the *mania-à-potu* patients in the five reports before us, we deduce the following results. Of the men, there were single, 138; married, 135; widowers, 4. Of the women, single, NONE; married, 27; widows, 8. There is a slight error which we cannot correct, in these aggregates, arising from the fact that, in the course of the five years, seven patients remained in the asylum over the 1st of January, and are consequently reckoned twice. But this does not materially affect the results.

"During the past year, the demand for accommodations for the insane has been so pressing as to exclude *mania-à-potu* cases almost entirely. For the last six months we have been unable to admit a single case of it."

In this report, the first of the five in which it is introduced, we find a record of the causes of death of the insane patients. "Three died of recurrent apoplexy, four of exhaustive mania, two of inflammation of the small intestines, two of epilepsy, one of marasmus, one of phthisis, and two of dysentery."

In regard to insanity as coincident with age, the table in this report "corroborates the inference" which Dr. Stokes thought himself "warranted in deducing from the statistics of last year," because 57 patients were between 30 and 40 years old, and only 47 between 20 and 30. Now let us understand ourselves in regard to this subject. If Dr. Stokes would merely assert that at any given time, to-day, for instance, the number of insane persons in our asylums, or in the whole population, between the ages of 30 and 40 years, is greater than the number between the ages of 20 and 30 years, we grant that his assertion is correct. No one doubts it who has examined the subject. If he would assert, or infer, that either at the time of *first attack* of insanity, or of *admission* into the asylums, the number of patients between 30 and 40 years of age exceeds that of those between 20 and 30, we would answer that we have abundant proof that such is not the fact.

In an article published in the *New York Medical Journal*, some six or seven years ago, this subject is discussed probably more minutely and analytically than by any author whose writings have been examined by Dr. Stokes. It includes the statistics of most of these existing American Asylums, and exhibits the ages as taken in four different methods.

1. *Ages of the patients in the asylum on a given day.*—The recovered of but one asylum is given. Result: Between 20 and 30 years, 18; between 30 and 40 years, 32—similar to that of the Mt. Hope statistics.

2. *Ages of all the patients during a given year, including the old residents and those admitted.*—Statistics from four institutions. Whole number of patients 848. Between 20 and 30 years of age, 214; between 30 and 40 years, 245. Corroborative of the Mt. Hope table.

3. *Ages at the time of admission into the asylums.*—Eight institutions; aggregate of patients 7,516. Between the ages of 20 and 30 years, 2,337; between 30 and 40 years, 1,884. No longer sustaining the result in Dr. Stokes's tables.

4. *Ages at the time of first attack of insanity.*—Nine institutions; aggregate of patients recorded upon this principle, 4,742. Between the ages of 20 and 30 years, 1,712; between 30 and 40 years, 1,106. *The number between 20 and 30 years exceeds that between 30 and 40, by a fraction more than 54 per cent.* The number of patients forming the basis of this estimate is so large, that the result may be taken as a solution of the question in regard to which decade of life furnishes, absolutely, the most cases of the primary invasion of insanity. But if the decennium of *greatest liability* to the disease be sought, or that in which the greatest *relative* number of cases occur, it can only be found by comparing the number of the occurring cases in each decennial period, with the number of persons of corresponding age in the general population. Dr. Bates has done this for Maine, and Dr. Kirkbride for Pennsylvania, and have shown that, in those States, the *greatest liability* to the disease is between the ages of 20 and 30 years. In the aforementioned article in the *New York Medical Journal*, the cases therefore admitted into the Utica Asylum are compared with the inhabitants of the State, and the result shows that the liability is slightly greater between 30 and 40, than between 20 and 30 years, being as 1000 to 991. We anticipate a different result when the calculation shall be based upon greater numbers, and that in New York, as in other States mentioned, the true period of greatest liability will be found to be in the decennium from 20 to 30 years.

In 1853, twelve patients with suicidal propensity were in the Mt. Hope Asylum; one of homicidal mania, in which the patient killed his wife, and one in which the mother, labouring under puerperal mania, took the life of her infant. "The intellect appeared to be unclouded, and the closest scrutiny could detect no aberration of the mind. The moral feelings appeared benumbed, deadened, perverted.

"General depletion has in no case been resorted to. In many instances it has been found necessary to resort to the local detraction of blood, in order to reduce undue cerebral excitement. * * * * We reiterate the assertion, that if large quantities of blood are abstracted previous to admission, the consequence is that the recovery of the patient is much retarded, if not rendered altogether hopeless. A case of puerperal mania was sent to us, in both of whose arms recent incisions were apparent, and who had been freely cupped on the

neck and temples. The most complete incoherency, after three months' continuance under treatment, exists. In another case of acute mania, in a female aged 65, the lancet had been freely resorted to before admission, without the slightest diminution, but rather with an aggravation of the frenzied excitement. In a few days complete prostration and exhaustion ensued, and she rapidly sank."

Many topics to which we have not alluded are discussed with ability in these reports of Dr. Stokes. We have found it difficult to restrict our extracts within prescribed limits, and have therefore selected only such parts as are of most importance, or the most novel to our readers.

2. Dr. Rockwell, of the Vermont Asylum, never makes voluminous reports. Each of the two now at hand is limited to four pages.

	Men.	Women.	Total.
Patients in the Asylum, August 1, 1851	169	166	335
Admitted in course of the year	79	82	161
Whole number	248	248	496
Discharged, including deaths	73	72	145
Remaining, August 1, 1852	175	176	351
Of those discharged, there were recovered			78
Died			31

Two of the four pages are occupied by extracts of letters from patients discharged.

	Men.	Women.	Total.
Patients, August 1, 1852	175	176	351
Admitted in course of the year	70	89	159
Whole number	245	265	510
Discharged, including deaths	62	76	138
Remaining, August 1, 1853	183	189	372
Of those discharged, there were recovered			72
Died			43
Aggregate admitted since the opening of the Asylum			2,066
Recovered			968
Number of deaths not given.			

In March, 1852, variola made its appearance among the inmates of the Asylum. "A short time before the first case occurred, a patient was admitted from a section of country where this disease prevailed, although neither herself nor any of her family had been exposed or suffered from it. We know of no other way in which it could have been introduced. The disease continued with us about ten weeks. One of our nurses and twenty-seven female patients were attacked by it, and only one, a female of seventy-two years of age, died. None of our male patients were affected with this disease."

Two of the wings of the Asylum have recently been enlarged, making additional provision for seventy persons.

3. From the very brief report by Dr. Fonerden, of the Maryland Hospital, we collect the following items:—

	Men.	Women.	Total.
Patients at the beginning of the year 1852	63	67	130
Admitted in the course of the year	26	8	34
Whole number	89	75	164
Discharged, including deaths	21	13	34
Remaining, December 31, 1852	68	62	130
Of those discharged, there were cured	5	5	10
Died	4	2	6

Besides the foregoing, seven cases of *mania-à-potu* were received, none of which terminated fatally.

Many applications for admission were refused, and owing to the crowded condition of the wards, it has become necessary to remove the patients belong-

ing to the District of Columbia, a considerable number of whom have heretofore found an asylum at this institution.

It is grateful to learn that measures have been taken towards furnishing the insane of Maryland with accommodations consistent with the present idea of their proper treatment. "The General Assembly, at its session in the year 1852, the first under the new Constitution, passed a law for the erection of a new hospital for the insane; and the commissioners appointed to execute it have purchased a very eligible site, on which they are now proceeding very actively with the work."

4. We have, as usual, two reports from the Lunatic Asylum of South Carolina; one by Dr. Trezevant, the Physician, and the other by Dr. Parker, the Superintendent. The former principally consists of a strong and urgent appeal, in which the inconveniences and defects of the present buildings are described, for a new establishment, located in the country. It is to be hoped that it has not been made in vain.

Patients in the Asylum, November 5, 1851	127
Admitted in course of the year	46
Whole number	173
Discharged, including deaths	38
Remaining, November 5, 1852	135
Of those discharged, there were cured	19
Died, including one suicide	8

From the two reports, we infer that the system of treatment pursued at the institution corresponds, as nearly as circumstances will permit, with that of the hospitals of more recent and more nearly perfect construction.

5. The State Asylum for the Insane, at Jackson, Louisiana, was opened on the 21st of November, 1848. On the morning of that day, eighty-five insane persons were removed from the Charity Hospital in New Orleans, and taken, "in a body" the distance of one hundred and sixty miles, to the Asylum, where they arrived, without accident, on the following day. The statistics, December 31, 1849, as given by the Physician, Dr. Preston Pond, are as follows: "Patients admitted, 130; recovered, 29; discharged, 18; removed, 6; eloped, 7; died, 24." It appears from this, that some are reported as recovered, who have not been discharged.

"The deaths have been, one accidental, two of fits, one of dropsy, one of cholera morbus, and the remainder of chronic diarrhoea."

"The number of deaths is large in proportion to the whole number of patients. The class of persons received here is different from that of almost any other institution of the kind in the United States. A great proportion came from New Orleans, where are congregated a multitude of foreigners from all nations. Many of them are in destitute circumstances, suffer much from improvidence, disappointment, and disease, until their bodily health is destroyed, and when they arrive here life, in many cases, can only be prolonged for a short period."

By the report for 1852 and 1853, it appears that this institution is under the general direction of a president and board of administrators; the executive officers being a physician, a superintendent—who is not a member of the medical profession—and a matron; that it will now accommodate one hundred and forty patients, and when finished will be able to receive two hundred and fifty.

	Men.	Women.	Total.
Patients in the Asylum, Dec. 31, 1851	33	45	78
Admitted in two years, to Dec. 31, 1853	75	48	123
Whole number	108	93	201
Discharged, eloped, and died	40	29	69
Remaining, Dec. 31, 1853	68	64	132
Of those discharged, there were cured	4	9	13
Died	23	17	40

Causes of death.—Chronic diarrhœa 5, diarrhœa 8, phthisis pulmonalis 3, cholera 4, yellow fever 2, marasmus 3, “fits” 3, cholera morbus 3, old age, chlorosis, “disease of the lungs,” chronic dysentery, lead colic, dropsy, general palsy, “flux,” 1 each; one not mentioned.

The patients who died with yellow fever, and of cholera, had those diseases when admitted. Neither of the epidemics spread among the other inmates of the institution.

“Diarrhœa is the most troublesome disease we have to contend with. It has been treated here with blue mass and Dover’s powder, sub-nit. of bismuth, sulph. copper and opium, sulphuric acid, sulphate of iron and sulph. alumen and potassa, acetate of lead and opium, and by cod-liver oil. Some cases have recovered, some have been improved and life prolonged. But generally, when a disposition to chronic diarrhœa is manifested, the disease, though checked, recurs again and again, until it finally terminates in death.”

	Men.	Women.	Total.
Whole number of patients admitted	191	134	325
Discharged recovered	30	23	53
Died	60	35	95

6. At the Butler Hospital, the number of patients on the 31st of December, 1852, was	64	78	142
Admitted in 1853	45	47	92
Whole number	109	125	234
Discharged, including deaths	46	52	98
Remaining, Dec. 31, 1853	63	73	136
Of those discharged, there were cured			44
Died			22

Died of chronic mania 10, “acute maniacal affection” 5, paralysis 2, “meningitis produced by transference of disease” 1, epilepsy 2, uterine disease 1, uncertain 1.

Of twenty-seven discharged improved, it was believed that “many would probably have recovered had they remained a few weeks longer.”

Patients admitted since the institution was opened, in 1848	583
Discharged recovered	171
Died	102

It will be recollected that in our notice of the last two preceding reports by Dr. Ray, we gave a synopsis of the discussion upon which he entered in regard to the causes of insanity at the present day. The subject is resumed in the report before us, and what is here written, together with that which preceded it, is almost enough to destroy one’s confidence in the advantages of civilization.

In resuming the discussion, Dr. Ray informs the reader that mental alienation rarely springs from an individual source, and that consequently, when he treats of a specific cause, it is only as one which occupies a “prominent place in a combination of incidents more or less directly followed by insanity.”

Intemperance in the use of alcoholic beverages is first considered; and at the close of this part is the following remark in reference to a certain class of drinkers with whom almost every lunatic asylum is troubled: “Let the legislature enact that habitual drunkenness shall be subjected to all the disabilities of insanity, and then we may engage in a work of humanity without infringing upon the rights of individuals.” * * * “Vicious indulgence” furnishes the Hospital at Worcester with about six per cent. of its patients, and to the Butler Hospital a somewhat larger proportion. “There is reason to believe that this form of disease has been increasing with all the contrivances of luxury incident to the progress of civilization. Abundance of stimulating food, allurements of dress, refinement of manners, frequent and intimate social intercourse between the sexes, and, above all, a species of literature especially designed to inflame the passions and fill the mind with seductive images—these are incidents which, whether regarded individually or collectively, have nowhere, and at no time, been so widely prevalent as in this country during the last fifty

years. They constitute the origin of much of the kind of insanity we are considering, and sufficiently explain why it should be more common in American than European communities."

The consumption of vital energy required by the exigencies of civilized life is believed to be a prolific cause of insanity. "There never was a time when brains were more rapidly used up, in one way or another. In the struggle for wealth, power, or distinction, or even for the privilege of living at all, success requires continuous attention, intense application, and a strain of all the faculties to an extent that was once comparatively rare. The disastrous result is usually precipitated by habits of living not calculated to restore the energies thus prodigally expended." Here the author proceeds to condemn in detail the warming of apartments with air heated by red-hot iron, ill-ventilated rooms, neglect of physical exercise, improper food—"bread just from the oven, charged with some deleterious drug and saturated with butter, with meat swimming in gravy, and swallowed with little mastication—not to mention a host of other articles equally improper." The immediate and secondary effects of these agencies are minutely described, and the more remote results, reached by gradation of abnormal phenomena, exhibited.

"No careful observer can help coming to the conclusion that, of all the incidents which tend to develop insanity in the female sex, none is so prolific as what is called 'ill health;' and there can be but little doubt that nowhere is this condition more common than it is with us. This form of disease arises, in a great degree, from excessive domestic labour in conjunction with bad diet, bad air, insufficient recreation, and, in married women, frequent childbearing." Reasons for its greater prevalence here than in similar classes in other countries. "The latter unquestionably work hard and fare hard, but *they start with a stronger constitution; they are much in the open air; they live on plain food, and move in a social sphere that bounds their wishes and aspirations.* Here, the woman enters upon married life with a constitution somewhat delicate. Ambitious that her house and family should be distinguished among her neighbours by all the indications of good management, but unable to indulge in hired service, she labours beyond her strength, and does nothing towards restoring it by suitable relaxation. The cares of an increasing family, without increasing pecuniary means, seems to forbid the slightest rest from the daily routine of toil. The duties are all within doors, in over-heated apartments; while a certain regard for appearances, and a perpetual straining after a higher social sphere, give rise to an uneasy, if not a repining state of mind. At last, the appetite fails, less and less food is taken into the stomach, the nervous system becomes irritable under the slightest impression, the sleep is diminished, the flesh reduced, and the mind is depressed by unaccountable gloom and apprehension. The end is now at hand in the shape of unequivocal insanity, from which recovery is tedious, at best, and often hopeless."

The subject of hereditary transmission is next discussed, but without adducing any new feature or effect of importance. Its operation in increasing the prevalence of insanity is thus alluded to: "I think we are warranted in saying that insanity in persons who subsequently become parents, is, upon the average, duplicated in the next generation. We must, therefore, shut our eyes to this trait of insanity—its self-multiplying power—before we can doubt the fact of its rapid increase. A larger knowledge of the organic laws, and of the penalties that follow their violation, will, undoubtedly, check the evil, but this power will too often be overborne by the other agencies that are concerned in the case. When reason, prudence, foresight, inculcate one lesson; and passion, sentiment, and taste another, it is obvious enough which will generally prevail.

"The operation of the physical causes here mentioned is aided, in this country, there is much reason to believe, by peculiar atmospheric conditions. * * * That our people are distinguished by restlessness, impulsiveness, impetuous and boisterous movement, may be regarded as a fixed fact. That this trait is to be attributed to atmospherical influences, is rendered probable both by the absence of any other adequate cause, and by the greater excitability which accompanies insanity in this country, as compared with others;" and "this excitability is but little less apparent in the Germans, Irish, and English, who abound in our hospitals, than in the native Americans." P. E.

ART. XVII.—*On the Structure and Use of the Spleen.* By HENRY GRAY, F. R. S., Demonstrator of Anatomy, and Surgical Curator of the Pathological Museum of St. George's Hospital. London, 1854. 8vo. pp. 380.

THIS admirable work, for which the physicians and surgeons of Guy's Hospital awarded to its author the munificent prize endowed by the late Sir Astley Cooper, is an honour to British science. For elaborate research and experiment, it is not excelled by the famous labours of Lyonnnet in the anatomy of the *Cossus ligniperda*. The investigations of previous writers have given us correct notions of the anatomical structure of the spleen—and, indeed, most of the results of the researches of Mr. Gray and his coadjutors; but none have so systematically, patiently, and accurately unravelled its structure and functions, as the latter.

After an historical introduction, the development of the spleen and its various tissues are first considered; and results of the author's observations clearly prove that the organ does not attain its maximum of development during foetal life, and that its conditions of utility need not be sought for during that period. He further adds: "If it be admitted that the spleen, like other organs, is performing its function to the fullest possible extent when it attains its largest size, and when it arrives at its maximum of development and growth, then it is proper that we should observe at what period this occurs." A table is then presented of one hundred and sixty observations by which, and others of a similar character, the author arrives at the following conclusions:—

"1. That the size, and consequently the activity of the spleen, are called into action from the period of birth to the termination of adult life.

"2. That its size, and consequently its function, are periodically manifested, and increased during and after the completion of the digestive process.

"3. Its size varies considerably according to the state of nutrition of the body, being increased in highly fed, and being diminished in starved animals.

"4. Independent of these extremes of size, consequent upon the nutrition of the body, it increases considerably after the completion of the digestive process in the highly fed animals; whilst in the opposite case, no difference in its size is observed."

The structure of the spleen is next minutely investigated, the results of which very generally accord with the researches of Kolliker, Ecker, Gerlach, &c.

In the chapter on the bloodvessels of the spleen, the author confirms the fact that each of the main trunks of the splenic artery supplies simply that region of the organ in which that branch ramifies, no anastomoses existing between it and the majority of the other branches.¹ The capillaries terminate by becoming continuous with the smaller venous trunks; but, as previously mentioned by Ecker, some of them cannot be traced directly continuous with the veins, and these communicate with interspaces of the pulp parenchyma which are finally connected with the veins.

From a number of experiments on horses it is shown that the quantity of splenic blood varies under different circumstances. The greatest amount exists in the organ just after the completion of digestion, and the new material has been received into the circulation. The smallest amount is found long after the digestive process has ceased, and the new material has been expended in nutrition, secretion, &c. The quantity also varies with the character of the nutrition of the animals; in ill-fed animals being small, and in those which have been starved, it is reduced to its minimum. The introduction of fluids, also, especially such as are quickly absorbed, considerably influences the increase of the blood; and, of course, while the direct transfusion of blood into the circulation increases that of the spleen, its withdrawal in a corresponding degree decreases it.

¹ Several preparations by Dr. Horner, in the Museum of the University of Pennsylvania, are beautifully illustrative of this fact.

The spleen is likewise found to act as a reservoir for blood when the circulation is obstructed as a consequence of impeded respiration. Experiment alone does not exhibit this result, for it is observed that diseases of certain organs, as the heart and liver, attended with obstruction to the circulation, are in all cases accompanied with enlargement of the spleen, and distension of its vessels.

From numerous analyses it was ascertained that the emerging blood of the spleen, as compared with the arterial blood entering the organ, and with the blood of the general venous and portal systems, presents the following peculiarities. It contains less solid matter than arterial or other venous blood, far less blood-globules, a considerable increase in the amount of albumen and fibrin, more fat, a variable amount of iron; and lastly, its serum presents in all cases a dark-reddish tinge.

An attentive study of the splenic pulp confirms the view of Kolliker, that the blood-corpuscles undergo disintegration in the spleen; and it is to this cause we must attribute the peculiarities of the splenic venous blood when compared with that which enters the organ.

After investigating the structure of the Malpighian corpuscles, and determining the laws which regulate their variation in size, the author considers that his results afford sufficient evidence of the glandular nature of these bodies, and of their close similarity in structure and function with the corpuscles of other ductless glands.

The last chapter of the work is on the physiology of the spleen; and in this the author enters into a consideration of the facts resulting from his labours, and concludes that the function of the spleen is to regulate the quantity and quality of the blood.

J. L.

ART. XVIII.—*Epilepsy and other Affections of the Nervous System which are marked by Tremor, Convulsion, or Spasm—their Pathology and Treatment.* By CHARLES BLAND RADCLIFFE, M. D., Licentiate of the Royal College of Physicians, Assistant Physician to the Westminster Hospital, Lecturer on Materia Medica and Therapeutics at the Westminster Hospital School of Medicine, etc. etc. London, 1854. 8vo. pp. 144.

THOSE of our readers who have had an opportunity of perusing Dr. Radcliffe's treatise on *The Philosophy of Vital Motion*, are aware that he has advanced a new and somewhat startling theory of muscular motion. That muscular contraction is a purely physical phenomenon dependent on ordinary molecular attraction when the muscle is *not* stimulated. That the real operation of nervous and other vital agencies, and of electricity and other physical forces, is not, as usually taught, to excite or stimulate contraction in muscle and other organic tissues, but to counteract this state, and induce relaxation or expansion. In other words, that all stimulants, vital and physical, so far from producing, on the contrary antagonize, muscular contraction, which occurs only upon the suspension or withdrawal of the action of the stimulants, in a manner perfectly analogous to that which takes place in a bar of metal when heat is withdrawn.

This physiological heresy, an exposition of which prefaces the proper subject of the essay before us, is enforced and illustrated by a series of arguments and illustrations of a most acute and plausible character, which cannot but arrest attention, even though they fail to carry conviction of the truth of Dr. Radcliffe's theory of vital motion.

Upon that theory is based, in the volume before us, the pathology and treatment of epilepsy, and other nervous affections marked by tremor, convulsion and spasm, as, the tremors of delicate and aged persons, chorea, paralysis agitans, delirium tremens, the rigor and subsultus of fever, the tremor of mercurial poisoning, the convulsions of fever, of hydrophobia, of saturnine and hydrocyanic acid poisoning, and those dependent upon the retention of urea

in the blood; the convulsions of cerebral disorder,* of uterine disorder, and of intestinal disorder, and the convulsions of death; laryngeal spasm, cholera cramps, tetanus, ergotism, catalepsy, and rigor mortis.

Whatever estimate may be formed of Dr. Radcliffe's views of the nature and cause of muscular contraction and relaxation, and the correctness of the application of these views to explain the pathology of epilepsy and cognate affections, there can be little doubt that his theory of these diseases has a surer basis in truth—in many, at least, of its features—than that commonly received.

On an analysis of the leading phenomena of epilepsy, Dr. R. shows that neither the vascular, nervous, nor muscular systems present any indications of morbid excitation, but the reverse.

The condition of the skin, the state of the pulse, and the general diminution of the temperature of the surface, but especially of the extremities, indicate a very decided depression of the circulation. During the height of the fit, the state of the circulation is one of prostration, verging upon actual extinction.

The entire condition of the epileptic indicates the same want of activity in the nervous as in the vascular system. "The intellectual and memorial faculties fail day by day under the blight which eventually obliterates them. On the eve of the fit, the patient is rarely otherwise than silent, sad, moody, and still; in the fit, he is bereft of all sensibility, consciousness, and volition, and for some time afterwards he is stupid, confused, and exhausted."

"Arguing from the state of the pulse and the respiration in epilepsy, it would appear," Dr. R. remarks, "that the *medulla oblongata*, the *spinal cord*, and the *sympathetic ganglia* are in the same condition of inactivity as the brain. Nay, it cannot be otherwise, for the functional energy of these organs, as of the system generally, must be in direct relation to the activity of the circulatory and respiratory changes. It is to be observed, also, that that remarkable want of tone, which is a very marked peculiarity of the epileptic, is indicative of the want of energy in the spinal cord, if it be a function of this organ to supply this tone."

The depressed condition of the muscular system is evinced in the epileptic by the remarkable inadequacy to exertion; in the slowness with which the system rallies after fatigue, as well as in the pallid and soft condition of the muscles on dissection, which condition contrasts very strongly with the normal redness and consistency of these organs.

"Viewed in this manner," says Dr. R., "the vascular and nervous systems of the epileptic, as well as the mobile structures in which the convulsive phenomena are manifested, are seen to present unequivocal evidences of inactivity, and this inactivity—so far at least as the vascular and nervous systems are concerned—is found to be most marked in the fit itself."

The causes, or supposed causes, which operate in the induction of epilepsy are, according to Dr. R., all of a depressing nature.

"Thus," he remarks, "the seizure is referable, not to joy, but to fright and fear—not to any natural excitement, but to the exhaustion consequent upon excess and abuse—not to good cheer, but to hunger and privation. It happens at night, when the vivifying influence of the sun is withdrawn, rather than the day. Much obscurity hangs over these matters, from the careless manner in which the most incongruous agencies have been grouped together as exercising the same influence upon the system, and much obscurity is inevitable from the difficulty of untying the complex knot which holds together the several influences acting upon the body; but there is no reason to doubt, and every reason to believe, that these several causes of the fit are of an exhausting and not of an exciting character."

On a review of the foregoing facts and considerations, Dr. R. considers it to be sufficiently evident that—

"Epilepsy cannot be caused by any excitement of the muscles consequent upon the excessive supply of nervous or any other stimulus. On the contrary, everything is in harmony with the physiological premises, and, as might be anticipated from these premises, the convulsion would seem to depend upon want of vital stimulation, which want had allowed the molecular attraction of the muscles to come into play and gain the ascendancy."

Although we are not prepared to give our assent to the position which Dr. R. attempts to establish, namely, that all stimulants, vital and physical, in place of exciting or promoting, on the contrary antagonize muscular contraction, we nevertheless admit the correctness of his views in relation to the condition of the circulatory, nervous, and muscular systems in the epileptic—everything connected with the phenomena and progress of his morbid condition, evinces a diminution of vigor—of vital energy—a depression of their normal functions; each of these systems is in a condition of *under*, and not of *over* activity.

Dr. R. endeavours to show that the same depressed activity in the vital functions obtains in all the affections allied to epilepsy—those, namely, of which tremor, convulsion, or spasm constitute a prominent system. The evidence of depressed vital energy are in many of these affections sufficiently apparent, and even in those where the evidence is of a less striking character, it will not be found wanting, upon a careful analysis of the circumstances under which the convulsive or spasmodic phenomena present themselves, and the condition of the circulatory, nervous, and muscular systems preceding and attending them.

“The subjects of nervous trembling,” Dr. R. remarks, “have a certain delicacy of constitution which cannot be overlooked, and if not women, they have the feminine habit of body in a very marked degree. Those who tremble from old age or from shaking palsy, present unequivocal marks of decrepitude and decay—the listless wish, the snowy or hairless head, the fireless countenance, the wasted limb, the feeble pulse. Chorea is almost peculiar to females, and to females whose parents were infirm or aged, or who themselves have become enfeebled by improper or injudicious habits. It often originates during some severe and exhausting disease, and is always accompanied by signs of debility. The atony of the circulation is usually indicated by paleness of the lips, face, and tongue, by pastiness of the skin, by effusion into the serous cavities, and by rheumatic deposits.”

Convulsion happens most frequently in women and children, and but rarely in men, and this fact is an argument that other convulsive disorders besides epilepsy are connected with a state which is characterized by weakness rather than by strength.

“Spasm, again, occurs more frequently in women than in men, and most frequently in the more irritable and weakly of women. Cramp is the constant companion of tremulousness. It increases with the advances of age, and is almost permanent when nervous tremors become intensified into palsied tremors. The subjects of catalepsy, in like manner, are more delicate and impressible than they ought to be. Their skin is usually pale or dingy, their pulse readily disturbed, their general appearance either hysterical or apathetic. A cataleptic boy, a patient of my own, was as irritable, uncertain, and fretful as an infant. His apprehension was slow, his memory weak, his head large, his eyes staring, his pupils dilated and sluggish, his complexion sallow and venous, his hand cold and clammy, his pulse slow and feeble; and lastly, the state in *rigor mortis* is expressed by the name.”

The consideration of the pathological condition of the vascular, nervous, and muscular systems in the several diseases and stages of disease marked by tremors, convulsions, and spasm, is followed by a chapter on periodicity. The views of the author in reference to this interesting subject are far from satisfactory, and cannot be received in their full extent as explanatory of the periodicity of disease until established by more convincing facts than have as yet been adduced in their support.

After noticing the indications of periodicity in the physiological phenomena of plants and the lower order of animals, Dr. R. observes:—

“In spite of every cause of perturbation, there are many evidences of periodicity even in man. It is something more than accident which so often causes man to be stunted, stolid, and passionless in countries where cold reigns without a rival. It is something more than accident which times the periodical changes of women by the lunar orbit. There are, indeed, frequent irregularities in all these cases, and particularly in the last, and these may be supposed to be the natural results of the changing periods of food and passion, but the law is apparent above every irregularity.

"It would appear, then, that there are certain periodical changes in vital phenomena which reflect more or less distinctly the movements of the sun and moon, some of them corresponding to the day, others to the month, and others to the year; and that these changes are more and more conspicuous the lower the grade of organization in which they are displayed—more so in woman than in man, more in animals at the foot of the scale of being than those at the summit, and most of all in the plant.

"Returning now to the bedside, it may be expected that the signs of periodicity will always be masked and obscure in man, but that they will be manifested most distinctly in him who is most deprived of that active inherent life which constitutes the badge of distinction between man and the plant, and not in the person who is acted upon by inflammation, or who is excited in any other way. And so it is.

"There can be no doubt as to the obscurity of the evidences of periodicity even where that obscurity is least, as in epilepsy and in affections allied to epilepsy; but there can also be no doubt as to the existence of these evidences. Thus, on looking at a number of cases, it is found that convulsion and spasm occur more frequently at night than in the day, more frequently about the time of new moon, than about the time of full moon, and more frequently in the winter months than in the summer months. Of these evidences of diurnal, monthly, and annual periodicity, the diurnal are the most frequent, and the best established; but all are sufficiently frequent and obvious to convince any one who will take the trouble to seek after them for himself, or to consult the admirable little treatise of Dr. Mead: *De imperio solis ac Lunæ in corpora Humana et morbis inde oriundis*. They are not, perhaps, sufficiently frequent and obvious to allow any theory to be based upon them, but it is impossible to omit noticing that the greater frequency of convulsion and spasm in the night, at the times of the new moon, and during the winter months, is in accordance with the preceding pathological doctrines, and that, being thus in accordance, it is an additional confirmation of the correctness of those doctrines.

"It appears, therefore, that the signs of morbid periodicity are manifested most distinctly in the person who is most deprived of that inherent life which constitutes the badge of distinction between man and the plant, and not in the person who is acted upon by inflammation, or who is excited in any other way; and being so they furnish an important confirmation of doctrine, and a new rule of treatment. The confirmation of doctrine is obvious, for in this point of view the signs of periodicity become only so many additional evidences of that constitutional want of innate strength which appears to be the prominent fact in the pathology of epilepsy and of the cognate disorders. A new plan of treatment is equally obvious, for if the signs of periodicity depend upon a simple want of innate strength, then it becomes necessary to abandon all those leeching and starving plans of treatment which have originated in the supposition that they depended upon internal inflammation, or fever, or some other state of excitement, and to adopt in their stead all the means which are calculated to arouse and invigorate the downcast and flagging powers of the system."

The general plan of treatment for epilepsy and the affections allied to it, laid down by Dr. R., is unquestionably the correct one. At the same time we think he depends too exclusively upon a tonic and stimulating course, to the exclusion of all other remedial measures, however strongly they may seem to be indicated by the morbid condition of various organs which is so often associated with epilepsy and the cognate diseases.

A full diet of nutritive and easily digestible animal food, without the usual quantities of farinaceous and green vegetable matters, is insisted upon by Dr. R., as essential in all cases of epilepsy.

The propriety of such a diet in all cases in which the patient can be induced to take it is admitted. In many cases, however, the appetite and digestive powers of the stomach are so impaired in the epileptic, that the patient is either averse to taking food of a proper kind, or if he can be induced to do so even to a very moderate extent, it produces considerable distress and other unpleasant symptoms. In such cases, the digestive powers of the stomach must be restored by an appropriate course of treatment before the full animal diet, recommended

by Dr. R., can be carried into effect, with any hopes of deriving from it the desired restorative and invigorating effects.

Dr. R. is in the habit of recommending a very liberal allowance of alcoholic stimulants to his epileptic patients; and he states that, from the result of this practice, he is fully satisfied that there is no disease in which they are more needed. He believes epileptics to be benefited by the use of coffee, and injured by the use of tea.

That there may occur many cases of epilepsy, in which the use of alcoholic stimulants are required, we admit; but we fear that, to allow their "very liberal use" in all cases, indiscriminately, would be apt to do far more injury than good. We can see no objection to the substitution of coffee for tea.

The epileptic's habits should be so ordered as to save the strength as much as possible. Hence Dr. R. very properly advises celibacy, abstinence from any severe study, and from bodily exercise.

"Often," Dr. R. remarks, "I have found a patient to improve in a marked and unmistakable manner as soon as he had had the resolution to conquer the fidgetiness which is invariably connected with debility, and to force himself to rest; and often I have known a patient begin to retrograde if he had begun to try his strength too soon. Only the other day, I had a note from a medical gentleman, in which he told me that a patient, about whom he had consulted me, had gone on very well so long as he had made a point of riding to his place of business, and that the fits had returned as soon as he had begun to disregard this direction."

As tonics in epilepsy, Dr. R. prefers quinia and iron.

Of turpentine he speaks in the highest praise, as "the remedy which stands foremost among those which have rendered unequivocal service in epilepsy." Its nauseous taste, however, and the irritation which it excites in the urinary and generative organs, have always been a serious objection to it, and the result has been that comparatively few patients have the resolution to persevere in its use as long as is necessary to insure permanent benefit. Dr. R. was, in consequence, induced to substitute naphtha for turpentine, and found that in doses of half a drachm to a drachm it produced the same decided relief as the turpentine; but it was scarcely less disagreeable, and patients could not be induced to take it for any length of time.

"After this," says Dr. R., "it occurred to me to try camphor, and this I did in doses of two or three grains, either alone or in combination with quinia or iron, one or both, according to circumstances. Being given in the form of pills, it was free from the principal objection applying to the two former stimulants, and it had this peculiar advantage, that instead of irritating the urinary and generative organs like turpentine, it exercised, or seemed to exercise, a direct quieting influence upon them. In other respects, as tried in several cases, the result was not less satisfactory.

"Next in order of time I gave a fair trial to chloric ether, and still with very decided benefit. Under ordinary circumstances, I gave half-drachm doses of this preparation, either alone or in combination with the ammonio-citrate of iron, or quinia, or naphtha, and in all cases it proved to be a very favourite and effective remedy, particularly with children. Sometimes I substituted Hoffman's anodyne in place of the chloric ether; and sometimes, when the need of a stimulant has seemed to be very urgent, I have associated the two, but it has always seemed that this form of ether is far less efficacious than chloric ether.

"In cases where, and at times when, an occasional stimulant effect was necessary, I have recommended the aromatic spirit of ammonia, either alone or in combination with ether, and the result has usually been certain and satisfactory."

In addition to internal stimulants, Dr. R. employs, externally, counter-irritants, and the hot bath.

Dr. R. recommends a hot bath daily, and an additional one whenever any unusual depression tends to the apprehension of a fit. From the adoption of this practice he has seen many instances of marked and decided benefit.

"This plan," he remarks, "seems to be equally desirable in long-standing cases, where there is much cerebral congestion, and theoretically and practi-

cally there is much reason to believe that this is what might be expected, especially if a towel dipped in cold water be wrapped around the head of the patient while he remains in the hot bath.

"In actual practice," says Dr. R., "I have rung changes upon these different stimulants, either giving them alone or combining them with iron and quinia, substituting one for another according to the changing circumstances of the case, and always allowing at the same time a liberal supply of dietetic stimulants, upon which, indeed, hope is mainly to be based, and I have every reason to be satisfied with the results. I have never met with a patient who has not been benefited; for even where the case has been of long standing, and the fits have kept their ground, there has been a manifest diminution of intellectual torpor, the face has lost a good deal of the brutalized expression which had been creeping over it, and the distressing nervous headache has disappeared, if that symptom had been present—and I have met with many patients who have been completely cured.

"If there is one time more than another when stimulants are necessary, it is on the eve of the fit. Then, vigorously administered, they will often prevent the paroxysm. Nor are they contra-indicated in the fit itself. At this time all that is usually required is to raise the head as much as possible, so that the blood may not gravitate into it, and to unloose the neckerchief and shirt-band; but if more is required, it is still upon stimulants that dependence must be placed, and this equally, whether the circulation be in a syncopal or asphyxial state. Indeed, under these circumstances, the proper course is to dip a door-key or hammer-head into boiling water, and apply it to the pit of the stomach, or to put the patient into a hot bath, or to take advantage of a moment of quiet and inject a turpentine enema into the rectum. Nor are stimulants contra-indicated after the fit, except perhaps during the first few moments of the reaction which follows upon the collapse, and this only in some instances, for often this reaction is not up to the normal standard. Nay, they are not necessarily contra-indicated by the mental excitement which occasionally supervenes upon the fit, for this excitement is usually, if not always, of an asthenic character.

"All these considerations are in harmony with what might be gathered from a simple inspection of the state of the pulse (the true key to practice), and the conclusion is that stimulants will be found to be the proper remedies for epilepsy, if they are given with discrimination, and regulated in quantity according to the heat or coldness of the season."

With regard to tracheotomy as a remedy in epilepsy, Dr. R. remarks that it is not easy to come to a decision, inasmuch, especially, as there is an insufficiency of evidence on the subject.

"Still," he observes, "it is clear that it does not fulfil all the original expectations of Dr. Marshall Hall concerning it. It does not prevent convulsion. It does not always, or even usually, make the convulsions lighter. It does not prevent danger, for of the few patients upon whom the operation has been performed, three have died either in the fit or in connection with the fit, and of these three, the opening in the windpipe was free from obstruction—at least in one. Under these circumstances, it becomes a question whether the benefits of the operation are sufficient to counterbalance the associated inconveniences and dangers, even where (which rarely happens) the asphyxial symptoms are consequent upon spasmodic closure of the larynx—and this question must remain in abeyance for the present.

"—— As to the rest, it only remains to be said that the accustomed rules of treatment must be applied to the correction of any special source of exhaustion, and particularly of those which are peculiar to female epileptics."

In reference to the treatment of affections allied to epilepsy, Dr. R. recommends a similar invigorating, tonic, and stimulating course. The general conclusion is—

"That epilepsy and the cognate disorders must be treated upon the same principles, and that upon these principles every cause of depression and exhaustion must be avoided, every means of increasing and establishing the strength must be sought after, and stimulants must be trusted to as the grand

agents in recovery. In a word, physiology, pathology, and therapeutics concur in showing the necessity of a complete revolution in everything relating to the theory and practice of the maladies which have been under consideration, and they also justify the hope that in future the theory will not be a subject of mystery, or the practice a source of conjecture, perplexity, and failure."

The essay of Dr. Radcliffe is well deserving of an attentive perusal on the part of the medical practitioner. The views of the author in reference to the nature of muscular contraction, and the influence exercised upon it by nervous and other vital agencies and by physical forces may, it is true, receive but little favour; nevertheless, his general conclusions as to the pathological conditions under which tremor, convulsion, and spasm ordinarily occur, which are by no means necessarily dependent upon the truth of his physiological doctrines, as well as his general directions for the treatment of those affections of which tremor, convulsion or spasm is the prominent characteristic, are worthy of serious consideration.

D. F. C.

ART. XIX.—*A Clinical Introduction to the Practice of Auscultation, and other Modes of Physical Diagnosis, in Diseases of the Lungs and Heart.* By H. M. HUGHES, M. D., Fellow of the Royal College of Physicians, &c. Second American from the second and revised English edition. Philadelphia, Blanchard & Lea, 1854. 12mo. pp. 304.

THIS little work is of that kind which will surely meet with many readers. The author has contrived to present to the student, in a comparatively small compass, a comprehensive, accurate, and well-arranged exposition of the leading principles and facts in the physical diagnosis of diseases of the chest. His language is plain, and the book is free from all that unnecessary discussion of the laws of sound, and of doubtful questions which are so apt to embarrass the learner in his efforts to discern and comprehend the essential points in this important branch of medicine. As its merits consist mainly in the manner in which the subject is presented, we shall be excused from offering an analysis of the work; it is only necessary to state that the author expresses his dissent from the views of Skoda, especially in the very general application of the "*laws of consonance*" to diseases of the chest. We append, however, the following explanation of a curious fact: "In some cases, which are not so rare as it has been by some authors supposed, it happens that though the pleura is actually full of fluid, and may be proved to be so by inspection after death, not only is tubular breathing general, but a shrill resonance of the voice is distinctly audible over the whole side. Though the lung be compressed into the smallest space, and the bronchial tubes be therefore far removed from the solid parietes, in all parts excepting in the neighbourhood of the spinal column, still, their vibrations are communicated to those parietes, and from them to the ear. How does this happen? It is, indeed, difficult positively to assert how; and because difficult to explain, the fact itself may be questioned. The same thing, however, in regard to the voice, as well as to the tubular breathing, has occurred in persons from whose chests, immediately after the examination and observation of these signs, several pints of fluid have been drawn off by paracentesis. How, then, are these circumstances to be explained? It is acknowledgedly one of those cases in which ordinary acoustic principles appear (most probably from our not being aware of the whole or the exact circumstances of the case) to be insufficient for the satisfactory explanation of what is occasionally heard in the practice of physical diagnosis. If a conjecture might be ventured upon the subject, it would be this: so long as there exists any portion of the lung which is capable of further compression; so long, therefore, as the parietes are not tense, and the fluid contained in them, although in large quantity, is movable, so long is bronchial respiration inaudible over the general surface of the side. But when the compression is great enough to render the

parietes and the fluid contained within them tense, but not so great as to flatten or absolutely to close the larger bronchial tubes, then the vibration of those tubes is communicated to the spine, or to the bony parietes, against which they rest, and thence, as in a well-braced drum, to the walls of the entire cavity. The fluid is so tightly pressed that it moves as a solid, and conveys the vibrations as a solid to, and with the solid walls. * * * In the former edition of this work, I spoke doubtfully and hesitatingly in reference to this curious subject, and as if it were of very rare occurrence—as it has, indeed, been represented by most authors—seeing that my attention had been directed to it only in a few instances. From many cases, however, which I have since examined previously to the operation of *paracentesis thoracis*, and from many others in which almost equal evidence existed of the presence and accumulation of fluid in the pleura, I am quite certain that it is not uncommon; I feel assured that it exists as the rule; and I very much question whether, if some accidental circumstance does not exist to prevent its development, it is not generally present when one side of the chest is fully distended with fluid.”

For soundness of doctrine, and practical good sense, there are few manuals of physical diagnosis which can compare with this; it therefore recommends itself to the attention of all who desire to possess accurate knowledge on this subject.

M. S.

ART. XX.—*Auscultation and Percussion.* By Dr. JOSEPH SKODA. Translated from the fourth edition by W. O. MARKHAM, M.D., Assistant Physician to St. Mary's Hospital. Philadelphia, Lindsay & Blakiston, 1854. 12mo. pp. 380.

To those who are not familiar with the modifications which Prof. Skoda has endeavoured to make in the theory and nomenclature of the sounds heard in the lungs and heart, this translation of the last edition of the Viennese professor's work will be highly acceptable. The following may be taken as a hasty sketch of his peculiar views:—

Rejecting the pectoriloquy and œgophony of Laennec as being merely modifications of the thoracic voice, uncertain in their diagnostic value, he recognizes only the following:—

1. The voice, accompanied by a concussion in the ear, completely traverses the stethoscope—loud bronchophony, which may be either clear or dull.

2. The voice, unaccompanied by concussion in the ear, passes incompletely through the stethoscope—weak bronchophony.

3. An indistinct humming (*sümmen*), with or without a barely appreciable concussion in the ear.

4. Amphoric resonance, and the metallic echo of the voice. Considering, moreover, Laennec's bronchial and cavernous respiration to be identical, and that there are respiratory murmurs to be heard over the chest, which cannot be classed under the head either of bronchial or vesicular murmurs, he institutes the following division:—

1. The pulmonary respiratory murmur, or vesicular breathing of Andral.

2. Bronchial breathing.

3. Amphoric echo and metallic tinkling heard during breathing.

4. Indeterminate (*unbestimmte*) respiratory murmurs.

Furthermore, he divides the râles as he does the voice and respiration, “so far only as the division has a practical value.”

1. Vesicular râle.

2. Consonating râle.

3. Dry crepitating râle, with large bubbles, or *craquement*.

4. Indeterminate râles.

5. Râles accompanied by an amphoric resonance and metallic tinkling.

From the above, it will be seen that Dr. Skoda aims not only at a simplification of the nomenclature of Laennec, but denies the accuracy of the facts upon

which it is based. Some of the fundamental doctrines which Laennec proposed, to account for the transmission of sounds within the chest, and upon which rests the diagnosis of the most frequent diseases of the lung, are completely revolutionized by our author's system. With many acknowledged imperfections, but with daily universal testimony to its general accuracy, as well as its occasional defects, Laennec's immortal work is still destined, we believe, to occupy the same place it has hitherto held. The deceptive character of the experiments upon which Skoda's system is founded, and the many discrepancies which are palpable in its application, deprive his innovations of that success which the wonderful diagnostic skill of their author would gain for them. In order, however, that the reader may acquire a correct idea of the new theory of the production and transmission of sound in the lung, we transcribe the following summary of it from Dr. Markham's preface:—

“ * * * The voice passes into the parenchyma of the lungs through the medium of the air in the trachea and the bronchial tubes, and is not propagated along their walls; it traverses healthy as readily as it does hepatized lung, and even somewhat more readily; consequently, bronchophony does not depend upon an increase of the sound-conducting power of consolidated pulmonary tissue; moreover, when the lung is consolidated, the thoracic voice increases and diminishes in force, without any concurrent change taking place in the condition of the lung. This variation in its strength evidently results from the circumstance of the bronchial tubes being at one moment blocked up by mucus, &c., and at another freed therefrom by the cough and expectoration, &c. If the bronchophony depended upon the conduction of sound, it would be a matter of indifference whether the tubes contained air or fluids. It must not be forgotten that, according to the ordinary laws of reflection of sound, the more solid the parenchyma, the more difficult does the passage of the sound from the air into it become. That the air in the mouth and nasal cavities consonates with sound formed in the larynx, is proved by the fact of the changes which the voice undergoes through opening and closing the mouth and nose, whilst the condition of the larynx remains unaltered; just in the same way does the air in the trachea and bronchial tubes consonate with the laryngeal sound. Now, air consonates only in a confined space, and the force of the consonance depends upon the form and size of the space, and upon the nature of the walls forming it; the more solid the walls, the more completely will the sound be reflected, and the more forcible the consonance. The cause of the loud voice produced by a speaking-trumpet is well known. But the air will consonate with certain sounds only; in the trachea and bronchial tubes, it becomes consonant with the laryngeal voice, in so far as their walls have a like or analogous character to the walls of the larynx, of the mouth, and of the nose. Within the cartilaginous walls of the trachea and the bronchial trunks, the voice consonates nearly as forcibly as in the larynx; but as the bronchial tubes divide in the lungs, they lose their cartilaginous character, becoming at last merely membranous in structure, and therefore very ill adapted for consonance; when, therefore, the consonance is increased in these latter tubes, we may be sure, either that the membrane forming them has become very dense or cartilaginous, or that the tissue around them is condensed and deprived of air, whereby the sound-reflecting power of the tubes is increased. Of course, the communication between the air in the tubes and the air in the larynx must be uninterrupted. The walls of a confined space frequently vibrate in unison with sounds excited within it, as do those of an organ-pipe or of a speaking-trumpet. The larynx vibrates with every sound, and its vibrations are perceptible at a considerable distance from their point of origin; so, also, must the walls of the bronchial tubes, which are distributed through the parenchyma of the lungs, vibrate when the voice consonates within them; and the vibrations thus excited will extend to the surface of the thorax, passing through several inches of thick fleshy parts, or of fluids, and manifest themselves there as the consonating sounds of the bronchial tubes.”

The author explains the respiratory murmurs and râles upon the same principle. His account of the cause of the impulse, sounds, and murmurs of the heart offers much that is interesting. The second part of the treatise is, however, that which will command the most attention, since in it are presented the

results of close and long-continued clinical observation of the phenomena elicited by physical diagnosis. Even without assenting to the correctness of the author's explanation of acoustic phenomena, or approving of his nomenclature, the reader can still not fail to derive much instruction from consulting this portion of the work. The translation is an excellent one, and its republication will, no doubt, be acceptable to many.

M. S.

ART. XXI.—*The Pathology and Treatment of Pulmonary Tuberculosis, and on the Local Medication of Pharyngeal and Laryngeal Diseases frequently mistaken for, or associated with Phthisis.* By JOHN HUGHES BENNETT, M. D., F. R. S. E., Professor of the Institutes of Medicine in the University of Edinburgh, etc. etc. Philadelphia, Blanchard & Lea, 1854. 8vo. pp. 130.

MOST of the readers of this *Journal* are familiar with this work, from its having been already issued with the *Medical News* during the first half of the year. To them we need not point out its value, and on those who have not yet studied the important facts contained in it, we would respectfully urge its perusal. In addition to a most lucid and rational account of the pathology of the disease, the author furnishes indications for treatment (illustrated by cases of his own), which are worthy not only of close study, but of general imitation. His labours are directed to the end of impressing upon the mind the fact that the treatment of tuberculosis should be a curative, and not merely a palliative one. His own success warrants him in this recommendation, and in this case, at least, there can be no doubt of the entire accuracy of diagnosis and trustworthiness of detail.

M. S.

ART. XXII.—*Clinical Lectures on Pulmonary Consumption.* By THEOPHILUS THOMPSON, M. D., F. R. S., Fellow of the Royal College of Physicians, London; Physician to the Hospital for Consumption and Diseases of the Chest, etc. Philadelphia, Lindsay & Blakiston, 1854. 8vo. pp. 259.

THESE lectures were originally delivered at the Brompton Hospital for Consumption, during the spring of 1851, and were published in the *Lancet* of that year. Without being at all systematic in their arrangement, they will be found to contain a good deal of valuable information relative to the statistics and treatment of consumption. The experience of the author with the cod-liver oil is highly favourable, and he has also found some of the animal and vegetable oils of great service. In some cases, indeed, the neat's-foot oil was used with signal advantage after the cod-liver oil had failed to effect any good. Dr. Thomson has been led to rely with a good deal of confidence upon a mark at the reflected edges of the gums of a deeper colour than the adjoining surface—as strongly indicative of a consumptive diathesis. He gives the following results as applicable to diagnosis:—

“1. That the absence of the streak in men affected with inconclusive symptoms of consumption, may incline you to a favourable interpretation of any such suspicious indications, but that in women rather less weight is to be attributed to this negative sign.

“2. That the presence of the sign in women is almost conclusive evidence of the existence of the tubercular element in the blood. When in either sex it coincides with a pulse not materially altered in frequency, by change from the sitting to the standing posture, the presence of phthisis may with high probability be assumed, even before having recourse to auscultation.”

With this brief notice, we beg leave to refer the reader to the “Lectures” for an explanation of this phenomenon, and for many valuable suggestions relative to the treatment of the disease.

M. S.

ART. XXIII.—*On Gout; its History, its Causes, and its Cure.* By WILLIAM GAIRDNER, M. D. Third edition. London, 1854. 12mo. pp. 400.

WE are pleased to find that the very favourable estimate we formed of this treatise on its first appearance, has been sustained by the suffrage of the medical profession generally, as is shown by the demand for a third edition within the short period that has elapsed since the first was issued.

This work is unquestionably a highly interesting and instructive one, and deserving of an attentive perusal on the part of every physician whose sphere of practice renders him liable to be repeatedly consulted by subjects predisposed to, or by patients already labouring under the attack of gout in its regular or irregular forms, or in those cases where its strange and confusing association with other known forms of disease, cause a decided change in the character and progress of the latter, and demand, to a certain extent, a modification in their treatment.

Having, in our notices of the former editions of Dr. Gairdner's treatise, adverted to the views inculcated by the author in regard to the pathology of gout and its hygienical and therapeutical management, we need only say, in reference to the present edition, that the practical observations on the particular application of remedies in the treatment of gout are very considerably extended; by which the instructions of the author are rendered more clear and explicit, and, consequently, more easy to be carried out by those who desire to test, by clinical experience, their true value.

An entirely new chapter is added, on what Dr. Gairdner very correctly terms "a subject of the keenest interest;" the existence of sugar as a healthy ingredient in urine, and of minute saccharine impregnations of this fluid as an attendant on various phases of gout.

Dr. Gairdner says that "it is with regret" he observes "the growth of this book." The additions that have been made to it, by which this growth has been occasioned, are of a character that will prevent his readers, we are convinced, from joining in this regret, inasmuch as they have increased its value in a greater ratio than they have its bulk.

D. F. C.

ART. XXIV.—*The Diseases of the Fœtus in Utero, not including Malformations, with an Outline of Fœtal Development.* By HENRY MADGE, M. D., Member of the Royal College of Surgeons, Licentiate of the Apothecaries' Society, etc. etc. London, 1854. 12mo. pp. 200.

THE pathology of foetal life is at once a curious and interesting subject of inquiry. Nor is it one altogether devoid of important results, affecting not merely the health and life of the foetus, but also the well-being and comfort of the mother. That the diseases so frequently met with in the new-born infant—excluding, of course, certain forms of abnormal development—are the result of the diseased condition of one or other of the parents, but especially of the mother, or of morbid causes acting upon the latter, but the effects of which, in the development of actual disease, are experienced only by the foetus in utero, cannot be doubted. Thus, children born of mothers labouring under syphilis, smallpox, tuberculosis, scrofula, intermittent fever, etc., are very apt to labour at birth under these same diseases; in other instances, however, infants are born with diseases, as acute and chronic inflammation of the peritoneum, of the lungs, or of the heart; purulent ophthalmia, or erysipelas, of which the mother, at no period during pregnancy, exhibited the slightest symptom. The pathology of these latter affections is a subject well worthy of investigation. Are they the result of morbid actions originating primarily in the structure of the foetus,

or are they produced by morbid causes which act upon the fœtus through the system of the mother?

We possess few treatises devoted exclusively to a consideration of the diseases incident to the period of intra-uterine life, and these few are, for the most part, imperfect and unsatisfactory. Incidentally, the subject is noticed by numerous medical writers, and our professional journals contain several able and valuable communications on some parts of the subject, among which we would especially notice the two elaborate papers of Dr. J. Y. Simpson, of Edinburgh, on peritonitis occurring during fœtal life, and on some of the presumed consequences of intra-uterine inflammatory action.

In 1840, Dr. Wm. C. Roberts, of New York, presented a very interesting and fair exposition of the various facts recorded in reference to the diseases of the fœtus, as "A Contribution towards a better Acquaintance with Intra-uterine Pathology." (See the numbers of this Journal for August, 1840, and October, 1841.)

Dr. Madge must have entirely overlooked this contribution of Dr. Roberts, as well as the very elaborate memoir of M. Andry, on the diseases of the fœtus and its appendices, which occupies some eighty-two pages of the first volume of the *Journal des Progrès* (2^{me} series, tom. i. 1830, pp. 126-205), when he penned the declaration that "nothing has yet appeared, having any pretensions to an elucidation" of the whole subject, "or to an arrangement of what is already known" in relation to it. "That as yet, we have had no clear and concise résumé of the important researches made by modern pathologists, and particularly by our continental brethren, on the diseases of the fœtus in utero."

The essay of Dr. Madge is certainly a most interesting one. He has collected, with commendable industry, the leading facts bearing directly upon the subject, and has endeavoured, as far as possible, to give to them a systematic arrangement, so as to indicate their pathological relations.

The information communicated will amply repay the time spent in the perusal of the work. The author has by no means exhausted the subject; his labours may, however, prompt to the investigation of the causes, character, indications, prevention, and cure of a class of diseases by which human life is impaired or destroyed even within the mother's womb.

D. F. C.

ART. XXV.—*Principles of Comparative Physiology.* By WILLIAM B. CARPENTER, M.D., F.R.S., Examiner in Physiology and Comparative Anatomy in the University of London; Professor of Medical Jurisprudence in University College, etc. etc. With three hundred and nine wood engravings. A new American, from the fourth and revised London edition. Philadelphia, Blanchard & Lea, 1854. 8vo. pp. 752.

In the last edition, the principles of general as well as of comparative physiology were embraced in the same volume. The increased amount of matter not only deserving but demanding notice that had been accumulated, by the diligence of observers in various departments of the science, since the appearance of that edition, and the desire of the author to present a full exposition of the present state of the entire subjects embraced in the original work, has induced him to divide into separate treatises the principles of general and those of comparative physiology. For various reasons he has issued first the comparative physiology, which we have in the volume before us; it being his intention, as he informs us, to reproduce the general physiology, as a companion volume to this, so soon as his numerous engagements will permit him to bestow upon that portion of his revision the careful attention which it requires.

The treatise before us may be viewed in some measure as a new work. Every portion presenting not merely the indications of a careful revision, but copious additions and modifications, so as to render it a full as well as an accurate exponent of the actual condition of the science of comparative physiology.

The student of this highly interesting and instructive branch of natural history cannot but return his most grateful thanks to Dr. Carpenter for the preparation of a work so well adapted to facilitate his labours—in which, with so much skill and evident care, will be found orderly and methodically digested and arranged for his use, the contributions of many investigators, industriously accumulated from the numerous volumes in which they are found recorded—some of these volumes of difficult access, excepting to such as have at their command a large and well-supplied scientific library, while many of them are sealed books to the merely English reader.

With the work of Dr. Carpenter, now placed within his reach by the cheap, though splendid reprint of Messrs. Blanchard and Lea, no one need be deterred from the study of one of the most pleasing, as well as the most useful branches of natural science. By means of its clear and accurate teachings, all, whether professional or amateur students, may trace the wisdom of the Creator, in the structure and vital endowments of the various organized beings that people the earth; and in “that combination of unity of plan with variety of purpose, by which is produced the endless diversity united with harmony of forms, so remarkable in the animated world.”

D. F. C.

ART. XXVI.—*The Principles of Animal and Vegetable Physiology: A Popular Treatise on the Functions and Phenomena of Organic Life; to which is prefixed a General View of the Great Departments of Human Knowledge.* By J. STEVENSON BUSHNAN, M.D., Physician to the Metropolitan Free School, etc. etc. With one hundred and two illustrations on wood. Philadelphia, Blanchard & Lea, 1854. 12mo. pp. 234.

THIS is certainly a very pleasing book to read, and withal highly instructive. To such as desire to acquire a general idea of the principles of animal and vegetable physiology, but who have neither the time, nor perhaps the inclination, to derive it from the study of either of those extended systems of special or comparative physiology, in which the subject is fully treated in all its various relations and bearings, this little treatise of Dr. Bushnan is specially recommended. Concise as is the sketch which it presents, it is, nevertheless, sufficiently clear and accurate, throughout, to convey a definite and correct conception of the respective functions of the several organs of plants and animals, with the principal modifications of the organs and functions in the different classes of the latter;—while the leading phenomena of the development of the germ and embryo, in both divisions of the organic world, are pointed out. The results, also, of a complete analysis of the constituents of organic structure, are presented, whether as reduced to their component textures by the anatomist, or, by the chemist, resolved into their proximate and ultimate elements. In the introductory chapter “an attempt has been made to expound, in brief and lucid terms, the general nature, relations, and applications of all the chief departments of human knowledge, in order to give the reader, not specially trained in science, a general view of the vast field of inquiry which the Creator has laid open to the lawful exercise of the human intellect.” And from this rapid survey of the various departments of knowledge, to point the student to the principles which ought to guide him in the pursuit and application of the several sciences.

We are far from recommending the treatise before us, as even a suitable manual for the use of those who would study thoroughly the science of physiology, either human or comparative. It is for the use of the general reader—of the student to whose future pursuits a profound knowledge of the subject is not essential, that the treatise has been written. For the purposes of such it is admirably adapted, and may be the means of leading many to a closer inquiry, through the aid of other guides, into the wonderful structure of all living organisms, and the nature and laws of their vital properties, actions, and relations. An

inquiry in which every one is interested, inasmuch as, from a correct knowledge of that structure and of those properties, actions, and relations, is derived our acquaintance with the means by which health and vigour may be promoted and maintained, and disease and premature decay avoided, not only in our own bodies, but in the animals and vegetables that furnish us with food and raiment—with the essentials, as well as with the comforts and luxuries of life.

D. F. C.

ART. XXVII.—*Healthy Skin: A Popular Treatise on the Skin and Hair, their Preservation and Management*. By ERASMUS WILSON, F.R.S., author of a "Treatise on Diseases of the Skin," "A System of Human Anatomy," &c. &c. Second American from the Fourth and Revised London Edition. With Illustrations. Philadelphia, Blanchard & Lea, 1854. 12mo. pp. 291.

THIS is, at once, a most interesting and instructive treatise, on a subject in reference to which no individual can be indifferent, inasmuch as upon a proper understanding of the structure and functions of the skin and its appendages, and of the means by which their proper and uninterrupted action can alone be maintained, the comfort and health of the body, as well as its exterior beauty, will in a great measure depend.

It has been well remarked by Hufeland, in his work on the art of prolonging life, that the skin "is not to be considered merely as a common covering to defend us from the sun and the rain, but as one of the most important organs of our body, without the incessant activity and agency of which there can be neither health nor long life, and in the neglect of which, in modern times, lies the secret source of numberless diseases and evils that tend to shorten our existence." How proper, therefore, is it that every one should make himself acquainted with the structure and offices of the skin, and the influence exerted upon it by diet, clothing, exercise, ablution, and bathing. In the work before us, these several subjects are discussed with great clearness and accuracy; and so well convinced are we of the importance of the author's teachings to all who would desire to insure and promote their own health and comfort, as well as the health and comfort of those placed under their control, that we should be pleased to find a copy upon the reading-table of every family in our land.

The proper care of the hair, though not, perhaps, attended with results commensurate with those which are derived from a due attention to maintain the healthful condition of the skin, is nevertheless of sufficient importance to claim the attention of every prudent person. The beauty and permanence of the hair, and the freedom of the scalp from troublesome, disgusting, and even dangerous diseases, is to a much greater extent under our control than is generally supposed. The remarks of Mr. Wilson in relation to this subject cannot fail to interest every one. The same remark may be made in reference to his concise but pertinent directions for the management of the nails.

D. F. C.

ART. XXVIII.—*The Science and Art of Surgery: being a Treatise on Surgical Injuries, Diseases, and Operations*. By JOHN ERICHSON, Professor of Surgery in University College, and Surgeon to University College Hospital. Edited by JOHN H. BRINTON, M.D. Illustrated by three hundred and eleven engravings on wood. Philadelphia, Blanchard & Lea, 1854. 8vo. pp. 908.

THE high character of this excellent work is very fitly and forcibly expressed by the motto and title adopted by its author. He proposes to treat of the science as well as art of his favourite branch; and his motto is in the words of the philosopher of Verulam: "They be the best Chirurgeons which, being

learned, incline to the traditions of experience, or being empirics, incline to the methods of learning." We have here, in very few words, our author's idea of a true surgeon, and we are led at once to look for, what we soon begin to find in the pages before us, the production of an expert equally accomplished in the teaching and the practice of his profession. This book has been for some time before the public, and is already so firmly established as a most desirable companion to the analogous volumes of Fergusson, Druitt, Miller, Pirrie, and others of similar date, as to require no further introduction at this late hour. In fulness of practical detail, precision and perspicuity of style, convenience of arrangement, and soundness of discrimination, as well as fairness and completeness of discussion, it is better suited to the wants of both student and practitioner than either of its predecessors. In one material respect, moreover, it decidedly excels them all; for without sacrifice of essential matter, or needless display of novelty or useless lore, it affords a clear, concise, and faithful picture of the latest progress of its subject in most questions of really practical or scientific moment. As a compend of the existing state of British surgery, from one of the best schools and ablest expositors in the English metropolis, it is especially valuable in our view; and we the more regret, on that account, our entire inability, within the restricted limits of this notice, and in the short interval of time allowed us, to make any use of the numerous passages which have suggested themselves as worthy of especial note, in passing, for the benefit of those of our readers who may not have met with the original. We must content ourselves, therefore, with the strong recommendation of this original, as the most reliable text-book for the student and work of reference for the practitioner, among the English manuals of the present day. A brief outline of its plan of arrangement is all that we have room to add. According to the preface, Mr. Erichson's treatise contains the substance of the lectures on surgery delivered by him, as professor of that branch, at University College. Omitting all consideration of affections of the eye, and almost entirely that of skin and ear diseases, as neither of them come properly or practicably within the province of a systematic lecturer, he begins his course under the head of "First Principles," with a brief examination of the nature and treatment of inflammation, some appropriate and useful remarks on operations in general, and a pretty full account of amputations in particular. He then passes on, in a second division, to the study of surgical injuries, including among others, wounds, fractures, dislocations, and their operative and otherwise remedial treatment, under which head he occupies some two hundred and twenty-four pages, in twenty-two extremely interesting and instructive chapters. The third and last division is devoted to the history and treatment of surgical diseases, and presents in thirty-eight chapters and five hundred and seventy-three pages, an imposing array of the various ills that flesh is heir to, which would be vastly more formidable were it not accompanied by such an admirably effective display of the wonderful resources of the healing art. The notes of the editor, although not very full, are generally to the purpose, and carefully written. They supply some occasional omissions of the author, or relate to a few improvements and peculiarities of practice in this country, or for the most part in his native city, and appear to be generally suggested by the precept or example of his Philadelphia preceptors. The publishers display their usual liberality and taste in the getting up of the edition, and, in presenting it in such a handsome and convenient form, have conferred upon American readers an obligation which we trust will meet with a suitable response.

E. H.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *On the Spleen, and some Peculiarities of its Capillary System.*—FÜHRER considers the spleen to be at the same time an organ of *retrogressive metamorphosis*, as also a *laboratory* of the *blood-globules*. Concerning the first function, the spleen acts like the other secreting glands, only with this exception, that the products of the metamorphosis are not excreted by a proper duct, but are carried off with the blood by the *vena lienalis*, and conducted to other organs of secretion, to undergo there further change. The blood-globules are formed, according to Führer's view, within a peculiar adventitious system of capillaries. These adventitious transitory vessels originate from the normal capillaries themselves, analogous to the new vessels in pseudo-membranes. They commence as small excrescences, which elongate into thin processes, forming on some points of their course various swellings, cellular cavities, in which the blood-globules are developed. New ramifications proceed from these, forming anastomoses with others, and communicating through these with veins, into which they pour out their contents. As soon as the circulation and formation of blood becomes sluggish, these adventitious vessels collapse, and a part of them perishes altogether; on the other side, their development becomes very active, connected with swelling of the whole spleen, and increase principally of the Malpighian bodies during and soon after the process of digestion—*i. e.* during the period of a more energetic tendency of the blood to the spleen, with increased activity in the formation of blood. The peculiar fibrous cells (*Faserzellen*) described by other authors as forming a part of the parenchyma of the spleen, are cells or cellular bodies of the just described adventitious vessels; they are, therefore, fragments of the apparatus for the formation of blood, the nuclei seen within them represent the commencement of new blood-globules. As corroborating his view, Führer refers to the phenomena consequent on the extirpation of the spleen—*i. e.* vicarious swelling of the lymphatic, principally mesenteric, glands, which exhibit an unusual development of the normal capillary vessels, and besides this, the adventitious vascular system just described in the spleen.—*Brit. and For. Med.-Chir. Rev.*, from *Vierordt's Archiv*. Band xiii.

2. *Functions of the Liver and Spleen.*—According to Liebig, the bile is an intermediate product, by means of which certain elements of the body are transformed into carbonic acid and water. If this doctrine be true, it ought to follow that animals, from which the liver has been removed, should give off, in a given time, and in proportion to their weight, less carbonic acid than animals in which the organ has been left intact. Dr. MOLESCHOTT removed the liver from a number of frogs, and succeeded in getting a few of them to survive the operation. The most of the surviving frogs lived only three days, but some of them

even for fifteen days. Moleschott then placed the frogs thus mutilated into an ingenious apparatus, which only admitted sufficient air to maintain their respiration, and in which the carbonic acid expired was absorbed by a contrivance of Liebig's, and transformed into carbonate of potash.

He began his experiments by placing in the apparatus as many sound and uninjured frogs as weighed, collectively, 100 grammes.

The carbonic acid exhaled by these 100 grammes of frogs, in 24 hours, averaged 430 millegrammes. He next experimented with 100 grammes of frogs deprived of their livers, and found that, in the same space of time, they only excreted, on an average, 192 millegrammes of carbonic acid. The author chose for his experiments, as far as possible, animals similar in their age, sex, condition, etc. He also extended his researches, in a similar manner, to animals deprived of their spleen, and found that the quantity of carbonic acid expired by them, in a given time, was to the amount of the gas given off by uninjured healthy frogs, as 1 to $1\frac{2}{3}$, thus proving that the spleen has less influence than the liver.

Moleschott also observed, that the blood of those frogs from which he had removed the livers contained *no sugar*; and this fact he considers confirmatory of Bernard's opinion, that, in the production of sugar, the liver does not act as a simple secreting organ, separating the sugar from the blood, as urea is thrown off by the kidneys. Indeed, we know that, after the removal of the kidneys, the blood becomes surcharged with urea.—*Monthly Journ.* July, 1854, from *Müller's Archives*, 1853.

3. *The Influence of Dilution of the Blood on the Urinary Secretion.*—Dr. KIERULF made a section of the left ureter in various dogs, and he collected the urine secreted by means of a glass tube. This operation was performed on the first dog between 8.55 and 9.40 A.M., with very trifling loss of blood. Shortly before the operation, the dog passed a considerable quantity of normal straw-coloured urine. At 9.45 a small quantity of blood was abstracted from a vein; and at 11.40, 495 grammes of distilled water, at the temperature of 32° R. (104° Fahr.) were injected into the jugular vein. Five minutes thereafter, the urine was mixed with a considerable quantity of blood, and its amount was also increased. A second bleeding was performed at 11.55, and the urine was collected till 4.9 P.M., when it contained blood, but in a very small quantity. At 4.50 the dog was bled for the third time, and at 5 o'clock some sanguinolent urine was passed by the intact ureter. Next day the urine was of the normal clear straw-colour.

Other experiments of a like nature gave analogous results. He instituted other researches to ascertain whether the mixture of blood in the urine was due to an increase of the pressure of the circulating fluid, or to some change in its composition. He found that the urine passed after an injection of defibrinated blood into the veins, was perfectly clear. Dr. Kierulf makes the following deductions from his experiments:—

1. That considerable dilution of the blood causes at first the excretion of albumen by the kidneys, and thereafter an admixture of blood in the urine. This latter effect he regards as due neither to increased pressure of the blood, nor to rupture of the capillaries in the kidneys.

2. That the injection of water into the circulation increases the salts in the blood, although it diminishes the solids in the urine.

3. That the quantity of urine secreted in a certain time does not correspond to the proportion of water in the blood.

The author considers that these experiments, or others analogous to them, may yet cast light upon the obscure pathology of Albuminuria.—*Ibid.*, from *Zeitsch. für Rationelle Medicin, von Henle und Pfeuffer*. Bd. iii. 1853.

4. *Blood-Crystallization.*—L. TEICHMANN has succeeded in obtaining crystals from blood without any preparatory evaporation, by the addition of four, five, or more parts of water to one of blood, and allowing the fluid to stand sufficiently long. In this way, and by the insertion of a small piece of cork under one of the angles of the covering glass, he has produced crystallization in the

blood of all the animals he examined, and in all the bloodvessels indiscriminately. His observations were conducted on the blood of man, oxen, swine, rabbits, pigeons, and fish; frogs' blood, for a time, formed the only exception. This observer believes that the crystallizable substance is contained in the blood-corpuscles; he has procured crystals from the filtered washings of the blood-cake, and states that he found them more perfect the more the corpuscles were freed from serum and fibrin. With regard to the influence of temperature, he has observed that the slower the evaporation takes place, the more complete will the crystals be; but if it be required to produce them quickly, the temperature may be slightly raised, but it must not be carried to the point at which the albumen coagulates. In subsequent experiments on frogs' blood, he procured crystals by the addition of a very considerable quantity of water, at a very low temperature; the quantity of the crystals was proportionately smaller than in other specimens of blood, and they are always colourless when thus obtained. From blood four months old, and also from dried blood, he has succeeded in forming crystals.—*Brit. and For. Med.-Chir. Rev.* April, 1854, from *Zeitsch. für Rat. Med.* Bd. iii.

5. *Hämin Crystals*.—By this name TRICHMANN designates crystals obtained by treating the mass of dried blood-corpuscles in hydrochloric, acetic, oxalic, tartaric, citric, or lactic acids, under a glass cover in a temperature of 20° to 50° R. These crystals have a yellow, brick-red, brown, or black colour; they form rhombic prisms, or sometimes stellæ, needles, or granules, like those of black pigment. They are insensible to the action of air, and are insoluble in water, ether, alcohol, acetic, hydrochloric, and nitric acids; in boiling nitric acid, however, they are entirely dissolved; they are soluble in dilute liquor potassæ, but become blackened in this fluid when concentrated; concentrated sulphuric acid, and still more readily ammonia, dissolves them. To the substance which undergoes this form of crystallization, and which he supposes to be derived from the blood-corpuscles, he gives the name of *Hämin*; he considers it to be different from the hæmatin of fresh blood, which is soluble in water, and also from the hæmatoidin forming the crystals described by Zwicky and Virchow. He has obtained *Hämin* crystals in the blood of various animals. Finally, he considers the black pigments as forms of undeveloped crystals in different degrees of oxidation. As a practical result of the knowledge of the properties of *Hämin*, he suggests the employment of the above manipulations to obtain crystals in cases of suspicious spots and stains.—*Ibid.*

6. *Influence of Cod-Liver Oil and Cocoa-Nut Oil on the Blood*.—DR. THEOPHILUS THOMPSON read before the Royal Society (April 27, 1854,) a paper on this subject.

He found, that during the administration of cod-liver oil to phthisical patients their blood grew richer in red corpuscles, and he refers to a previous observation of Dr. Franz Simon to the same effect. The use of almond-oil and of olive-oil was not followed by any remedial effect; but from cocoa-nut oil, results were obtained almost as decided as from the oil of the liver of the cod, and the author believes it may turn out to be a useful substitute. The oil employed was a pure cocoa oleine, obtained by pressure from crude cocoa-nut oil, as expressed in Ceylon and the Malabar coast from the Copperah or dried cocoa-nut kernel, and refined by being treated with an alkali, and then repeatedly washed with distilled water. It burns with a faint blue flame, showing a comparatively small proportion of carbon, and is undrying. The analysis of the blood was conducted by Mr. Dugald Campbell. The whole quantity abstracted having been weighed, the coagulum was drained on bibulous paper for four or five hours, weighed, and divided into two portions. One portion was weighed, and then dried in a water-oven, to determine the water. The other was macerated in cold water until it became colorless, then moderately dried, and digested with ether and alcohol, to remove fat; and, finally, dried completely, and weighed as fibrin. From the respective weights of the fibrin, and the dry clot, that of the corpuscles was calculated. The following were the results observed

in seven different individuals affected with phthisis in different stages of advancement:—

	Red corpuscles.	Fibrin.
First stage, before the use of cod-liver oil ...	{ Female 129.26	4.52
	{ Male 116.53	13.57
First stage, after the use of cod-liver oil ...	{ Female 136.47	5.00
	{ Male 141.53	4.70
Third stage, after the use of cod-liver oil ...	{ Male 138.74	2.23
Third stage, after the use of cocoa-nut oil ...	{ Male 139.95	2.31
	{ Male 144.94	4.61

Med. Times & Gaz. June 10, 1854.

MATERIA MEDICA AND PHARMACY.

7. *Experiments on the Preparations of Squill.*—Dr. CHATEAU has instituted a series of experiments, with the view of replying to a question proposed by the Faculty of Medicine of Paris: “*Determine by clinical observations what are the effects of the preparations of squill.*” In a *résumé* of the memoir presented to the Academy, M. Chateau states the powder, wine, vinegar, and oxymel of squills were employed; but the powder, most frequently, on account of its action not being interfered with by any other constituent of the medicine.

His first experiments were made upon seven dogs, to whom he administered quantities of the squill varying from 40 grammes of the powder to 1 gramme, or their equivalent, of the wine of squill. In one of these experiments, 1 gramme of soft extract of squill was injected into the subcutaneous cellular tissue; in all the other experiments, the drug was introduced into the stomach. The effects produced were the following: The animals became dull, and this was followed by increase of the buccal secretions, nausea, efforts at vomiting, and by semi-liquid stools passed in small quantities; when the dose was sufficiently large, tremblings supervened, and paralysis of the posterior limbs, which soon extended to the anterior. After this, the animals appeared to regain their equilibrium, and then suddenly a convulsive attack occurred; they fell upon their side; there were some movements of deglutition, a little orthotonos, and death in from thirty-five minutes to one hour and fifty-five minutes after administration of the drug. When the dose was small, the same series of phenomena was exhibited, but more tardily; and death was delayed for twelve or fifteen hours. In all the experiments there was a remarkable diminution of the temperature of the body, as taken in the rectum, believed to be due to the hyposthenic action of the squill upon the nervous system. On examination of the bodies after death, the viscera were found congested, the blood black and thick, the bladder empty, or containing but little urine, the ganglia of the great sympathetic reddened, the cerebrum little altered, but the cerebellum and spinal cord softened sometimes even to diffuence.

M. Chateau next relates the results of his observations on the human subject in disease, which had reference principally to the diuretic and laxative operation of the drug. He administered it in 4 cases of pulmonary emphysema, in 3 cases of albuminuria, 3 cases of abdominal dropsy, 1 of which was ovarian, 2 cases of pleurisy, 2 cases of pneumonia, 2 cases of phthisis, and several cases of rheumatism, only 1 of which is related in the original memoir. In these 17 cases, the operation of the squill was as follows: in 7, it proved *diuretic and purgative*; in 2, simply *diuretic*; in 2, simply *purgative*; in 4, *expectorant*; in 1, *diuretic and expectorant*; and in 1 no effect was observed.

He noticed that when either the purgative or diuretic operation became excessive, the other immediately ceased.

The number of cases of each disease in which the squill was administered was too small to warrant the general therapeutical conclusions drawn by Dr. Chateau; we shall therefore content ourselves with noticing two points of in-

terest connected with the operation in the cases described. Of the cases of *albuminuria*, 2 were purged by the powdered squill without any increase in the secretion of urine; in the third case, where the wine of squill was employed, a diuretic effect followed; but in all three cases, the albumen in the urine was unaffected. In one of the cases of *abdominal dropsy*, where the powder proved diuretic and laxative, the dropsy was removed in less than two months.

The reply of Dr. Chateau to the question of the Academy is: "*That squill has a direct hyposthenic action upon the ganglionic and cerebro-spinal systems, and that this action is exhibited at first by an increase of the urinary and intestinal secretions, and at last, if the dose of the drug be large, by paralysis and death.*"

Dr. Chateau prefers the powder for internal administration, next to this the wine of squill, and as an expectorant, the *oxymel*. He considers 10 to 15 centigrammes (from $1\frac{1}{2}$ to $2\frac{1}{2}$ grains) a good dose to commence with, and that it may be increased, after some days, to 35 or 40 centigrammes ($5\frac{1}{2}$ to 6 grains).—*B. and F. Med.-Chirurg. Rev.* July, 1854, from *Archives Générales*, Jan. 1854.

8. *On Delphinin*.—The deficiency of our knowledge respecting the properties and operation of the alkaloid delphinin, has led Dr. LEONIDES VAN PRAAG to their new and complete investigation. The delphinin which was employed in the experiments whose results we are about to detail, was obtained from Messrs. Frommsdorf, of Erfurt.

Dr. J. L. Van Praag administered the alkaloid to animals from each class of the vertebrata, and in the paper under our notice, gives the results of his observations on fishes, frogs, birds, and mammalia, and relates at length the experiments which he instituted. The mammals which he selected for experiment were dogs, cats, and rabbits. In these, asphyxia, as a result of paralysis of the heart (*Herzlähmung*), was among the principal phenomena; whilst paralysis (*Lähmung*) of the nerves of motion, and, at a later period, of those of secretion and the special senses followed. Dogs were least of all affected when the poison was given to them in a piece of meat, since the two were soon vomited together. It operated more powerfully when it was administered by the anus, or introduced into a wound of the integument. But death followed almost suddenly, so that scarcely a minute intervened, upon injection into a vein, from arrest of the heart's action; the animals, in consequence of discontinuance of respiration, gasp for breath with open mouth, and in a short time death follows, in the midst of severe tetanus. On injection of the alcoholic solution into the anus in dogs, repeated evacuation of feces at first occurred, and then salivation. The walk became staggering and uncertain. There arose great adynamia, in consequence of which they leaned against anything for support, until at last they fell down. The sensibility of the skin diminishes simultaneously with the lessening power of the motor nerves, until anaesthesia is complete. The respiration is at first increased, panting and accompanied by howling; in addition to this, tenacious, clear mucus accumulates in the glottis and larynx, which renders respiration still more difficult. At a later period, the frequency of respiration diminishes, until at last the act occurs very rarely, or ceases altogether. The functions of the brain and of the organs of the senses are little affected, until the disturbance of the circulation and respiration increases. In the instance of cats, into whose anus the alcoholic solution was injected, the evacuation of feces and flow of saliva were also increased, and there were observed, at first, frantic and wild leaps, the symptoms of adynamia occurring at a later period. The phenomena of the circulation and respiration are the same as in dogs, and both cats and dogs show signs of a tickling sensation in the mouth. In rabbits, also, after the injection into the anus, active and strong symptoms of reaction appear, as in cats, terminating, however, in adynamic symptoms. When the delphinin, in a fatal dose, was introduced, in cats, into the subcutaneous cellular tissue of the back, the poison operated first of all on the sensory nerves at this spot, and induced symptoms resembling those arising from a powerful irritant. The animals exhibited great disquiet, bristled up their hair, set up their back, rolled on their back, and finally lay down. Gradually, however, in this case also, the action of the poison upon the

heart and lungs appeared. When, in the instance of dogs, the poison was given dry with the food, the operation of the poison soon became apparent upon the mucous membrane; there were observed vomiting, and an excessive itching in the mouth, so that the animals rubbed their lips and mouth with their feet, and rubbed their nose upon the ground. With this, also, arose profuse salivation. The principal points observed on dissection of the poisoned animals were, fulness with blood of the membranes of the brain, of the heart, of the larger venous trunks, and of the liver; fulness of the gall-bladder, and a collection of mucus in the air-passages. Nothing was noticed respecting the condition of the kidneys. In one case, air was found in the veins of the membranes of the brain.—*B. and For. Med.-Chirurg. Rev.* July, 1854, from *Archiv. für Pathol. Anat. und Physiol. und für Klinische Medicin*, Bd. vi. Heft 3.

9. *Comparison between Chloroform and Cold as Anæsthetics.*—By JAMES ARNOTT, M. D. The last number of the *Medical Times and Gazette* records another death from chloroform. Within the last three months, three deaths have happened from the same cause in the London hospitals alone, and in the hands of careful men, experienced in the use of this agent. Yet, as the fatality is small in comparison with the number of cases in which chloroform is used, every one *hopes* that such an event will not happen in *his* hands. Is it proper, however, to incur any amount of risk when the same object—the prevention of pain—can be attained by other means perfectly devoid of danger, and possessing many other advantages? Does not the surgeon err who continues to use chloroform unnecessarily, even should he himself never so cause death, by countenancing the employment of a measure that every month proves fatal in the hands of some other surgeon? Does he not, in fact, so participate in causing these deaths? It is merely a question of time. The moment some eminent patient dies from chloroform, unnecessarily administered, that moment will it be deemed imperative to substitute a safer anæsthetic. Why should not this time be anticipated, and such an event prevented? Why should not a stop be at once made to the afflicting mortality from this cause? For what can be more distressing to the relatives of a person so killed, or to the surgeon who administers the fatal poison, than the consciousness that its administration was altogether unnecessary.

Benumbing cold has been proved to be a safe and perfect anæsthetic in the immense majority of surgical operations. It would have prevented death in more than three-fourths of the fifty or sixty cases that have proved fatal from the use of chloroform. Among others who have afforded evidence of these powers of intense cold, I may mention M. Velpeau, in France, and Mr. Paget, in this country. The French journals have at various times reported M. Velpeau's use of cold for this purpose in the *Hôpital La Charité*; and the *Medical Times and Gazette* of the 1st inst. reports an operation in which it was successfully employed by Mr. Paget. In a paper, in the *Medical Times and Gazette* of the 25th of March, I have mentioned how cold is applied to produce anæsthesia; the design of the present communication is to make a comparison between it and chloroform.

It is commonly supposed that the application of benumbing cold must be a difficult and troublesome proceeding; much more so, in both respects, than the administration of chloroform. The very contrary is the truth. Whether the cold is applied by keeping in contact with the part, for a few seconds, a refrigerating mixture of ice and salt, contained in a gauze bag or a thin metallic vessel, or by touching it with a thick piece of copper that has been dipped in such a mixture, nothing can be easier; and it is impossible to fail. Different from chloroform, the anæsthetic effect is complete within a minute; and, as it has no unpleasant consequences, the surgeon is released from those protracted attentions which he is so often called upon to give in allaying the nervous symptoms that frequently follow the administration of chloroform. He requires no assistant; and, as the anæsthetic brings no new danger of its own, his mind is undisturbed, during the operation, from the anxiety which he would suffer from chloroform on this account.

The expense of either plan is so trifling that it does not deserve mention with

respect to private practice; but, with reference to hospitals, where the strictest economy is required, it may be worth while to state that cold does not cost a twentieth part of the price of chloroform. In using a frigorific mixture for remedial purposes in dispensary practice, I have made two pennyworth of the materials answer for several cases in succession. Mr. Ferguson, of Giltspur Street, has had benumbing vessels elegantly made of silver; but, however well suited for private practice these may be, a rougher apparatus will answer. On one occasion, in employing congelation for phlebitis, I borrowed for the purpose the net which confined the hair of the attendant nurse; and the principal ingredient cost as little as the instrument which contained it, for, there being a snow-storm at the time, it was gathered from the door-step.

The perfect safety from cold, and the anæsthesia from chloroform in the deepest operations, are the great respective advantages of these agents. Of the thousands of times intense cold has been used, not once has it been followed by any more untoward event than a slight cutaneous irritation. If the skin is merely benumbed, no redness follows the application; if congelation of the adipose matter under the skin is caused, a redness comes on, which may continue for a day or two. But, as explained in the paper already referred to, in the *Medical Times and Gazette*, this is the very contrary of inflammation. Instead of being a symptom of inflammation, the redness shows that a condition of the part exists rendering inflammation impossible. And in this safety produced by congelation, there is an advantage not inferior in importance to the insensibility. For, to the erysipelas and phlebitis following surgical operations, the greater number of deaths occasioned by them is to be attributed.

The anæsthesia from chloroform in deep operations can only be called perfect under the supposition, still contested, that the unconsciousness of the patient afterwards, that he has submitted to an operation, proceeds from having felt no pain, and not merely from having forgotten it. To judge from his struggles and cries, the latter would be the conclusion.

The anæsthesia produced by chloroform is by no means so certain as the anæsthesia produced by cold, because, in the latter case, there is no unconsciousness. But, in deep operations, it is only the incision of the skin which is very painful. The most eminent orthopædic practitioner of the day states, in a letter to the writer, that in the operations he is conversant with, the only source of pain is the incision of the skin; and perhaps no surgeon has had so good an opportunity of forming an opinion on this point. But all will agree that, if the insensibility of the skin were suspended, there would be very little suffering from the cutting of the deeper parts. So little, indeed, that it becomes a question whether life should be endangered by suspending it. The pain attendant on tightening the ligatures of arteries could be easily obviated by the momentary previous application of a congealing copper ball.

Chloroform, by causing unconsciousness, prevents the patient's assisting the surgeon in his operation, and apprising him of mistakes that may happen in its performance. The public has just been reading, with horror, the account of attempts made to drag a stone from an unopened bladder, by a forceps introduced through the wound, and grasping both stone and bladder. But for the insensibility induced by chloroform, the screams of the unfortunate child would at once have indicated the error; and the system, perhaps, is more to be blamed than the surgeon.

In the act of administration, and afterwards, certain inconveniences attend both measures. Chloroform, besides producing unconsciousness, causes a sensation of choking, and is often succeeded by headache, sickness, and prostration. Cold, applied only to the degree of benumbing (which may often be sufficient), causes no unpleasant sensation; but when congelation is produced, there is a sense of pricking, like that caused by mustard, both at the time, and after the return of the circulation. This subsequent smarting may be entirely prevented by a moderate application of cold; and that which first takes place may be lessened, if thought worth while, by a little management.

In recapitulating the subject, we may say, that although, in deep operations, the insensibility produced by chloroform may be greater than that produced by cold (unless this were applied in the successive stages of the incision), in all

superficial operations, which constitute the immense majority, cold is superior to chloroform in the circumstances of safety, ease of application, or the saving of time and trouble, certainly of producing anæsthesia, and, lastly, in the power it possesses of preventing subsequent inflammation. Surely, a conscientious and humane surgeon will not allow the prejudice against novelty or innovation to outweigh so decided a superiority. Anæsthesia will, no doubt, henceforth be a required element of every surgical operation; but chloroform, fortunately, is not the only mode of producing it.—*Med. Times and Gaz.* Aug. 5, 1854.

10. *Charcoal Coverlet for the Prevention of Smell from Gangrenous Sores.*—In some cases of hospital phagedæna recently under his care, in St. Bartholemew's Hospital, Mr. Wormald made an ingenious and very useful application of the disinfecting powers of charcoal. It is well known that dry charcoal will effectually absorb any noxious or offensive gas which can be made to pass through it. On this power, Dr. Stenhouse's disinfecting respirators depend for their efficiency. The difficulty in applying it in hospital practice has, however, arisen from the difficulty of keeping it at the same time dry and in a uniform layer around the part giving rise to effluvia. Dr. Wormald's plan consists in sprinkling freely between two sheets of cotton wool a tolerably thick layer of powdered charcoal, and then "quilting" them together in small segments, so that the powder is retained securely in its place. The pads thus prepared may be of any size, according as required to wrap round the end of a stump, or to cover a superficial ulcer. The sloughing sore having been dressed in the ordinary manner, and a little lint or wool so placed as to absorb any discharge which may flow, over all is laid the charcoal quilt, which is then lightly confined by a bandage. It forms, in addition to its disinfectant properties, a very soft and comfortable envelop, more especially if the sore be in such a part that the patient is obliged to lie on it.—*Med. Times and Gaz.* July 1, 1854.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

11. *Treatment of Fever by large Doses of Quinia.*—Dr. F. CORBYN states that from his long experience of the treatment of fever by large doses of quinia, when this disease broke out with such violence in its typhoid form at Anarkallie and the Citadel, he urged on the attention of medical men, when he went through their hospitals in August, the immediate necessity of prescribing quinia in large doses, as twenty grains, for instance, in each. Their apprehension was that the expenditure by such a practice would be so great that they could never obtain an adequate supply; but he soon allayed their fears, by demonstrating the fact that the expenditure would be 50 per cent. less than in the ordinary practice of administering frequent and, after all, useless small doses of that invaluable medicine. One or two doses, he asserts, when large, will eradicate the fever at once, if combined with good effective purgatives. He found that in H. M.'s 90th, small doses were being prescribed; but that in the Horse Artillery, on the contrary, the very opposite treatment was pursued.

Dr. Mackinnon thus remarks on the success of this method; his success was wonderful:—

"The cases of intermittent fever were so numerous, that I had full opportunity of trying every mode of administering quinia, and I have come to the conclusion that the most effective and the most economical mode of administering it, is to give it in a single large dose, at or towards the termination of the sweating stage. It is now my practice to give 3ss at that period, and I have never seen it fail to put a stop to the disease at once. I have given the same quantity six or eight hours before the accession of the paroxysm; but I found its effect uncertain; it often checked the paroxysm, but it sometimes failed to do so; in the latter case, however, though one paroxysm succeeded the dose, I

never saw a second follow. It was this circumstance which led me to conceive that the period of six or eight hours was too short for the full development of the antiperiodic effects of the medicine, and I was thus induced to prescribe it at the termination of the sweating stage. I have found it equally effective in tertian as in quotidian intermittents.

"Lesser doses of from twenty to twenty-five grains, I have found so often to fail, that I adhere now, in every instance, to the large dose.

"I have not found it to produce vertigo, tinnitus, or other cerebral disturbance in a greater degree than small doses frequently repeated do; in fact, as the period of administration of the medicine usually occurs late in the evening, the patient gets his dose at bedtime, falls asleep, and if any sensations of vertigo, or tinnitus, or of other cerebral disturbance, are experienced in the morning, they are usually slight. I prescribe the medicine without much reference to the state of the bowels; if they are confined, a purgative is either given along with the dose, or deferred till the succeeding morning, as the case may seem to require. If they are easy, I often give no medicine but the quinia.

"The great recommendations of this mode of administering quinia in hospital practice are: 1. That it economizes the expenditure of the drug: in the usual plan of giving quinia in repeated doses, from forty to fifty grains and upwards are often consumed in the treatment of a single case; rarely, in the intermittents of hot climates, so little as thirty grains. 2. That the soldier returns to his ranks more rapidly. If the case is recent, and uncomplicated with local derangement, the patient is sometimes discharged on the second day after admission, but usually on the third day, an earlier period than was attainable on the old plan of treatment. Occasionally, admission into the hospital is not desired; but the patient comes in the evening, swallows his dose of quinia, and returns to the ranks. 3. That it makes fewer demands on the attention of the hospital subordinates, and diminishes greatly the amount of their labour, instead of having repeated doses at specific hours to administer, a matter, in an hospital full of cases of ague, involving no small labour, and requiring considerable attention as to time. There is only one dose to be given.

"In remittent fever, I have also, during the remission, prescribed a single large dose of quinia with the best effects; but the cases in which I have so prescribed it have been too few to admit of my giving an unqualified opinion as to the superiority of this over the common mode of administering the remedy."

Dr. Mackinnon did not conceal his treatment under a bushel, but communicated his success to his assistant, Dr. Mactier, in medical charge of the 3d troop of his brigade, which was then stationed at Loodianah, and where fever was prevailing to a fearful extent. Dr. Mactier thus describes the disease, and his success also:—

"The only disease which has prevailed to any remarkable extent in the troop, during the past year, has been intermittent fever; but from this they suffered so much as to attract the notice of government, and cause their removal from Loodianah to Jullundur. Occasional cases of fever occurred during the early part of the year; but it was not until August that the number became in any degree remarkable. From this time, however, they rapidly increased, till about the 20th of October, when the disease reached its maximum. On one occasion, twelve cases out of one hundred men were admitted into hospital in one day, and it was no unusual thing to have forty sick under treatment, besides ten to twenty convalescent; with only two exceptions, every individual in the troop, including women and children, has suffered from one or more attacks of fever. It is a subject of congratulation, that out of 356 cases of fever, treated during the season, only one proved fatal. The symptoms of the disease were the same as have generally been remarked. On its appearance, the cold stage was frequently absent; but this became more and more marked, and, latterly, it was the one from which the patient suffered most severely. In a few instances, the prostration of strength during the ague fit was so great that stimulants required to be liberally administered. The most remarkable feature in the disease was the obstinacy with which it returned, after the patient had suffered from one or two attacks. Affections of the spleen were, as might be supposed, exceedingly common, although, in general, they were only temporary. Still, in not

a few instances, the organ became chronically enlarged. Seven of the worst cases of the sort have been sent to Landour for the ensuing hot season.

"As regards treatment, the ordinary method of administering divided doses of quinia, was for some time had recourse to, and with the usual result; the disease was cured, but not until the medicine had been taken for several days. In October, I began to adopt the plan of giving one large dose of quinia, twenty to thirty grains, a few hours before the expected paroxysm, and the results were in the highest degree satisfactory. Latterly, however, I have tried another method, which, in my practice at least, has proved decidedly more successful, viz: instead of giving the large dose of quinia before the febrile paroxysm, I now administer it just after the sweating stage has subsided. Since this treatment was suggested to me, by Dr. Mackinnon, of the 3d Brigade H. A., in November, I have never thought of employing any other, as, out of upwards of fifty cases, in only two has it failed immediately to cut short the attack. I have never seen disagreeable symptoms result from the large doses of quinia; headache and giddiness have certainly occasionally been complained of, but not more frequently than when the small repeated doses were given. The simplicity of the treatment, the saving of trouble to the attendants, and the reduced expenditure of the medicine, are all recommendations in favour of this practice, which, though by no means a new one, has not, I believe, been generally adopted. In the treatment of natives, large doses proved alike economical and successful."

Phillour, like Loodianah, suffered unprecedentedly from this severe fever during the year 1850; admissions during July, August, September, October, and November, of fever cases were 1,751; died, 10. But, during 1851, Assistant Surgeon Frederick Corbyn, M. D., took medical charge. It was to be supposed, from the repeated attacks of almost every man in the regiment this year, that they would be more weakly and predisposed, and that greater mortality would ensue. Dr. Corbyn prescribed twenty-grain doses of quinia, accompanied with a brisk purgative; hence, though during July, August, September, October, and up to the 8th November, there were 1,037 fever cases admitted, not one died; but, beside the sick of the 49th Regiment, there had been a considerable number of fever cases among officers, conductors, and sergeants and their families, as well as among the arsenal establishment, in all of which recovery took place under the same treatment.—*Indian Annals of Med. Sci.* Oct. 1854.

12. *Treatment of Epilepsy.*—Dr. R. B. Todd, in an interesting clinical lecture delivered at King's College Hospital, offered the following valuable practical observations on the treatment of epilepsy. In reference to the drugs which have been generally employed, he observes: "Most of these have been used in consequence of their exhibiting a certain amount of physiological influence on the nervous system. As to others, it is difficult to trace the circumstances which led to their admission among the *juvantia* in this disease. First in the list I would place those drugs which belong to the narcotic class—opium and its various preparations, belladonna, henbane, conium, camphor. These certainly exercise a very powerful influence in calming the excitable state of the nervous system which accompanies epilepsy. They are applicable chiefly to the more acute cases, and especially when epileptic delirium is threatened or present. You should never administer them except when you have full opportunity of watching your patient, or of confiding him to the care of another. This remark, I need hardly add, applies chiefly to belladonna and the preparations of opium.

Belladonna has been greatly extolled by some French writers, far more than it deserves, as far as I can judge from my own experience. It is a remedy which leaves no permanent ill effect, and which I think, is worthy of a more extensive trial in this country than it has yet received. That it exercises a physiological influence on the nervous system, no one can doubt, who has administered it for forty-eight hours; but as it induces a state of pupil very like that which is so common in epilepsy, one might fear that it rather favoured the epileptic state. A good series of clinical observations are yet wanting upon the effects of this drug. The preparation chiefly used in France is the powdered leaves of the plant.

Opium is often useful in cases in which the fit is apt to occur in the night or early morning. A full dose given at bedtime will prevent the development of the fit.

The valerian was used formerly in epilepsy more extensively than now. It is a medicine, however, which should not be despised or discarded. Its use promises most in the complications of hysteria and epilepsy. Its virtue depends upon the presence of an oil or acid which is capable of entering into combination with metallic bases, as zinc and iron, and which also combines with quinia and other alkaloids. You may give it either in scruple doses of the powder two or three times a day, or in infusion, or as an ammoniated tincture, or in combination with quinia, or the metals.

Other nervine remedies may be added to your list, and you should keep them in reserve, to employ as occasion may require, such as musk, castor, assafetida, stramonium, sagapenum, garlic, remembering the moral influence of a change of drug. I cannot say that the evidence of any special power of an anti-epileptic nature in any of these drugs is at all satisfactory.

In this class of drugs you may place the Sumbul, of which a tincture is prepared by Mr. Savory, of Bond Street. One of the earliest cases in which I employed it, seemed to benefit very decidedly by it; and I have since used it many times with unequivocal good, so as to lead me to look upon it as a useful stimulant, and antispasmodic remedy.

Digitalis has been greatly lauded by some. It may act favourably partly by its diuretic properties, and partly by its influence on the heart's action. There is quite sufficient evidence of its utility to warrant its being retained among the list of remedies applicable to this disease, and to justify its occasional cautious use.

The cotyledon-umbilicus may rank with digitalis as a remedy for epilepsy. I cannot say that I have met with a case distinctly benefited by its use; but the cases published by Mr. Salter, and those recorded by the late Dr. Graves, indicate that it possesses a certain anti-epileptic power. I incline to think it acts by a diuretic influence.

There are other drugs obtained from the vegetable kingdom, which, I think, deserve a trial in epilepsy. One of these is the *Achillaea millefolium*, which, in the form of a decoction, exerts a very decided diuretic influence. On the same principle, taraxacum may do good in certain cases, as in those in which the epilepsy has relation to gout in the system.

The alkaline salts, especially the bicarbonate and the nitrate of potass, appear to me often to be very useful on a similar principle, namely, by increasing the activity of the kidneys.

The cardamine pratensis was greatly extolled by Sir George Baker. Turpentine has been used chiefly from its anthelmintic properties. It is a remedy that should be used very carefully, and never, if there be suspicion of organic disease of the brain.

The *selinum palustre*, or *peucedanum montanum*, is highly commended by Herpin, on, as I think, insufficient grounds. The plant is very difficult to procure, and I have not yet been able to try it.

The various metallic tonics have been and are extensively used in epilepsy. Of these, the safest is zinc, the sulphate, or the oxide. Either of these preparations may be given without any disadvantage that I know of, for a considerable time, and in large doses. Some time ago I gave to a patient in this hospital as much as half a drachm of sulphate of zinc, thrice a day, without any sensible effect. The dose had reached that amount by gradual increase. Whether these drugs exercise any special favourable influence, I am unable to say; all I know is, that, under their long-continued use, patients sometimes cease to be troubled with fits.

The various salts of iron are also applicable to the treatment of this disease, and especially to cases in which the blood is poor in hæmatin. But they are not so harmless as the salts of zinc. Their tendency in some persons to excite headache precludes their use with them; and they often disturb digestion, and on this account cannot be persevered with. The saccharine carbonate, the sulphate, the citrate, and the tincture of the sesquichloride, as well as Griffith's

mixture, are the preparations of iron which you will find most suitable to the generality of cases.

Salts of copper and of silver are also used for epilepsy, with, I think, at best, but very doubtful efficacy. To the cautious use of the ammonio-sulphate of copper, I see no material objection; in too large doses it may irritate the gastrointestinal mucous membrane, but such irritation is easily removed.

There is, however, a much more serious objection to the use of the salts of silver. The nitrate and the oxide are the salts employed. Now, there is no doubt that the nitrate discolours the skin. We see repeated examples of this in the streets; scarcely a day passes that I do not recognize persons in the streets, as epileptics, who had been treated with nitrate of silver. If the nitrate of silver were a certain, or even a very frequent cure for this formidable disease, well and good; but, seeing that it is very far from being entitled to the credit of being such a remedy, I say, that we have no right to make blackamoors of our patients, and to stigmatize them forever as epileptics, and too often as *epileptics uncured*, on the mere chance of doing good by nitrate of silver. No doubt few would hesitate long between choosing a discoloured skin, and being the subject of a disease so fearful as epilepsy. But it seems to me, that the remedy ought to be a specific, to justify the practitioner in administering it, with the strong probability there is of producing such an effect. Remember, too, that the nitrate of silver stain communicated in this way, is probably indelible, and that, by an experienced eye, it is not to be mistaken. I have seen it after ten and fifteen years from the administration of the drug.

Finding, then, no special virtue in the nitrate of silver as against epilepsy, and that it is very apt to produce the serious result to which I have referred, with whatever care it may be administered, I have long since abandoned its use. I fear the oxide of silver is not more promising, either as to its influence on the disease, or its freedom from the discolouring power.

Indigo has likewise been given in epilepsy, but with no other result, as far as I can learn, than what Penil observed, that it turned the nails of his patients blue.

The time has, I think, now arrived, when we must look in another direction for an anti-epileptic remedy. We must turn our attention more particularly to those substances which are capable of being exhibited by inhalation, of being brought to bear upon the nervous system, without being subjected to the action of the gastric fluid.

Ether and chloroform, it is now well known, are capable of being taken into the blood in this way. Some years ago, I tried experiments as to their power of controlling the convulsions induced by strychnine, and with the result of finding that they possessed that power very completely. As long as an animal is fully under the influence of ether or chloroform, no convulsion can be produced. I was thus led to try their inhalation in epilepsy. I have chiefly employed for this purpose chloroform, because it is less disagreeable to patients to take. The results of my observations so far, are these—that in the more acute forms of epilepsy, the inhalation of chloroform has considerable influence in controlling and modifying the attacks; it has also a very decided power over epileptic delirium; and over some of the concomitants of epilepsy, as the violent convulsive jerks of the muscles of both upper and lower extremities, which are always most distressing, and often very dangerous. It has a marked and immediate power over puerperal convulsions; and I am glad to see that my friend, Professor Simpson, of Edinburgh, sanctions, by his high authority, its use in such cases. It is also applicable, and with advantage, to the convulsions of infants, and to laryngismus stridulus.

There is no use in employing chloroform in epilepsy unattended by other symptoms, when the attacks are at uncertain times and at long intervals; nor do I advise you to attempt its use, unless the patient is fully under your control, or will heartily co-operate with you. And in cases where the heart is affected, either functionally or organically, it should never be used. Its inhalation at two or three stated times of the day, exercises a very calming effect on the nervous system, diminishing its excitability very materially. But this must be car-

ried on over a long period of time—many months, and even years; and the dose may vary from twenty to sixty minims, according to the effect produced, and you should aim at obtaining a gradual, not a rapid effect. The patient should always remain in the horizontal posture during the inhalation, and for at least half an hour after it.

You will expect me to say something on a proposal made within the last few years to open the trachea of patients suffering under severe epilepsy, with the twofold object of preventing the fits, or of obviating their effect, in creating undue distension of the cerebral bloodvessels, if they should occur.

Before a proposition of this kind can meet with general favour from practical men, it must be satisfactorily proved that the hypothesis, or theory, if you will, from which the proposal emanates, is essentially sound; and next, it must be shown that the operation is in itself one that is not seriously dangerous to life; and lastly, it ought to be in the highest degree probable that the attacks will not recur after the operation has been performed. It is, as you know, contrary to all principles of sound surgery to perform an operation, unless it be for the complete removal of the disease, or, at worst, to stave off the reappearance of the malady to the latest period.

The hypothesis from which this proposal emanates assumes that the exciting cause of all the evil in severe epileptic paroxysms, is the contraction of the uncles of the neck and glottis, which induces and maintains a congested state of brain, which, in its turn, excites and keeps up the convulsions. I have elsewhere examined this congestion theory in detail, and I am not aware that the arguments which I then advanced against it have ever been fairly met. I have also shown, by experiment, that epileptic convulsions may be excited in dogs which had been freely tracheotomized previously, full provision having been made for the free ingress and egress of the air; and also in a dog in which the muscles of the glottis were paralyzed by section of the recurrent nerve. These experiments, the advocates of tracheotomy in epilepsy have found it convenient to ignore.

Again, I would ask, is tracheotomy either a very simple or a very safe operation? I do not think that the results of the operation either for other diseases, or for epilepsy, are very satisfactory. Patients often die from the effects of the operation; exhaustion, erysipelas, diffuse inflammation of the areolar tissue spreading into the mediastinum.

And, lastly, what are the prospects that, the operation having been done, the evil will not recur? This can only be ascertained by that which I cannot recommend, frequent experiments. I would only make this remark; that those who have the courage to try these experiments, and feel themselves justified in so doing, must carefully watch the results, and endeavour to draw a proper distinction between the actual physical effects of the operation, and that mental influence which, as I have shown you in a former part of this lecture, undoubtedly tends to stay the disease, when any new treatment is adopted, or any considerable change takes place in the patient's position and circumstances.

But, gentlemen, let us look on, and watch the result of these operations in the hands of others. No one will be more ready to acknowledge himself in the wrong than I shall be, should it turn out that, in this proposal, an important remedy has been found for a most terrible malady.

In conclusion, I have to express my fears that I have drawn but a sorry picture of the power of our art to deal with this formidable disease. But let not this discourage you; and do not fall into the notion, nor countenance it in any way, that epilepsy is an incurable malady, and that epileptic patients may as well be abandoned to their fate. There is no doubt that some may be completely cured, many very greatly alleviated, and that all should, as far as possible, be the objects of medical scrutiny and care, with at least the object of finding out more and more of the natural history of the malady, and of the *jurantia* as well as the *lædencia*; and with a hope that, at some time or other, a remedy may be vouchsafed to us, or at least that we may gain some insight into the intrinsic nature of this formidable scourge of mankind.—*Med. Times and Gaz.* Aug. 12, 1854.

13. *Chloroform in Hypochondriasis*.—Professor OSBORNE stated at a meeting of the College of Physicians, in Ireland (June 1, 1853), that he had lately, in three cases, opportunities of observing a peculiar effect of chloroform taken into the stomach, in controlling the depressing and saddening feelings belonging to hypochondriasis. Considering that state to be produced by a depraved sensibility of the stomach or colon, and frequently of both, he was led to the internal employment of chloroform, which, being promptly volatilized at the temperature of the stomach, and before being decomposed by the process of digestion, ought to be expected to act as a local anæsthetic, even though the dose should not be sufficient to produce any change in the function of the brain.

The first patient who presented the conditions requisite for this experiment was a married woman, and a mother, aged thirty-three, of a querulous disposition, as was well marked in her countenance, and who had been on a former occasion under his care and that of another practitioner, complaining of a variety of pains in the abdominal region; and she, although relieved, yet persevered in the belief that she still had some internal disease. She now appeared to labour under spinal neuralgia. After this had yielded to the application of nitrate of silver to the spine, and some other remedies, she still continued to feel an indescribable sensation of depression, and of internal annoyance, no longer to be referred to the spinal nerves; no cause for it could be detected. The appetite was good, and the action of the bowels regular. In two days, after taking ten drops of chloroform thrice daily, she *began, for the first time, to acknowledge that she was better*, and in a few days afterwards was free from complaint. The second case was that of a caretaker in the Linenhall, aged twenty-nine. He complained of the deepest dejection of spirits, and of an uncontrollable aversion to make any exertion. His countenance expressed sadness and moroseness. All the functions were in a healthy state, except that the heart's action became tumultuous when excited either by emotion or exercise; but no organic disease could be detected. He stated that he had not been addicted to excesses of any kind, and that there was no cause for his lowness of spirits. He got valerianate of zinc, and also pills to regulate his bowels; but although the heart's action became steadier, yet the depression and inward sensation continued the same. After taking twenty drops of chloroform thrice daily, for two days, he *began to confess*, what he never did before, *that he was better*. His sleep being still unsatisfactory, and disturbed by disagreeable dreams, he was ordered to take forty drops at bedtime. He now stated that he slept with a pleasing dream of seeing his brother, who had gone to America. During the two following nights he took the same dose; and, although his sleep was interrupted by the disturbance attendant on a man in a dying state in the same ward, yet when he did sleep, his dreams were pleasant, being usually that he was enjoying the company of the most agreeable of his friends. He was dismissed with a marked improvement in his countenance, and *acknowledging that he was better*.

The third case was that of a farmer, from the county Wicklow, twenty-eight years of age. In him, the poet's exclamation, *Beati agricola*! appeared to be completely reversed. The angles of his mouth were drawn down, his brow generally contracted, and he appeared sometimes as if contemplating suicide; yet he stated that he was in comfortable circumstances, married, and with a family. He complained of an inward sinking and sense of depression, so constant and overpowering, that for some months he could not command himself to make any exertion, and had become unable any longer to attend to his business. His bowels were usually torpid, but although he had repeatedly taken purgative medicines with effect, yet he had obtained no further benefit. A careful examination having been made without detecting any disease, he was ordered to take ten drops of chloroform thrice daily, and two assafetida pills every alternate night. At the end of about four days of gradual improvement, his countenance had become placid. He *confessed that he felt much better*; and in a few days afterwards, feeling a strong desire, and also a capability, of resuming his ordinary avocations, he went home.

These cases are selected as being nearly free from complication. It must, however, be recollected, that there are several other uses to which chloroform may be applied in affections of the stomach and intestinal tube; but this appears

to be one of the greatest value, inasmuch as no other medicine can be named which in this respect seems to come into competition with it. How far the effect is permanent, and capable of completely removing the sensation of hypochondriasis, or in what degree it may require to be resumed or repeated, Dr. Osborne as yet has not been able to determine; neither did he think it necessary, before this Association, to clear himself from the absurdity of bringing it forward as a universal *nepenthes*. Like all other agents affecting the nerves of sensation, its potency must vary much in different individuals, requiring various doses, while in some cases it may fail altogether.

With regard to the mode of administering chloroform internally: as its specific gravity is nearly 1.5, and it is insoluble in water, it must, when swallowed, soon settle at the bottom of the fluids in the stomach; and although it is volatilized, yet being covered, and under pressure, it may remain in contact sufficiently long to irritate the stomach at the part of contact, as was proved to take place in the case of camphor by Orfila. Hence, then, it is desirable that it should be diffused or diluted before it is taken. In aqueous mixtures, even when shaken up, it soon falls so that it cannot be equally measured out, and its pungency is annoying even to the mouth. In gum Arabic mucilage, it soon collects in large globules at the bottom of the bottle, covered with a white powder of arabine which it has precipitated. To obviate this inconvenience it has been proposed to give it suspended in syrup, but to make a syrup of the same specific gravity 1.006 grains of sugar to the ounce of water would be required, while that of the Pharmacopœia contains only 874 grains; besides, chloroform has a heavy sweet taste which renders the addition of syrup peculiarly objectionable.

The menstruum which Dr. Osborne used in the above and other cases, was the decoction of Irish moss (*carrageen*). With this, chloroform forms a uniform mixture, and in the proportion of ten drops to the ounce, they remain for an indefinite time without separation taking place. The taste of the mixture is sweet, like that of a heavy syrup, to relieve which it may be well to add a few drops of some aromatic or bitter tincture.

Another mode of avoiding the pungency of chloroform, is by giving it in combination with tinctures, as it is soluble in alcohol, and remains dissolved even in proof spirits. The following is a specimen of this kind of formula, and is peculiarly grateful to the taste, and susceptible of various additions and alterations, according to the requirements of individual cases: Chloroform, and tincture of ginger, of each half an ounce; aromatic spirits of ammonia, two drachms. Mix. Twenty-five drops to be taken thrice daily in a wineglassful of milk.—*Dublin Quarterly Journ. Med. Science*, Nov. 1853.

14. *On the Use of Vegetable and Mineral Acids in the Treatment, Prophylactic and Remedial, of Epidemic Disorders of the Bowels.*—An interesting paper on this subject was read before the Epidemiological Society, July 3, 1854, by J. H. TUCKER, Esq.—The author commenced by alluding to the remarkable, but well-established fact, that in 1849 the cider districts of Herefordshire, Somersetshire, and part of Devonshire, were, to a great extent, exempt from the epidemic ravages of cholera, while the disease was raging around. Upon further inquiry, it was ascertained that this exemption was confined a good deal to those individuals who drank cider as a common beverage, and that those who partook of malt liquor occasionally suffered. He also remarked that, in some parts of France and in Normandy, more particularly where cider is the common beverage, cholera is seldom known to exist; and further, that Switzerland was reported to have been free from its visitation.

Having adduced these and other facts in proof of the prophylactic power of cider, the author expressed his opinion that other vegetable acids would be found of service, such as lemon-juice, orange-juice, and sour wines made from grapes, or even from gooseberries. And as it would be found impossible to supply the whole of London with a sufficient quantity of pure cider, Mr. Tucker suggested that *vinegar* might be found a useful substitute in case of another outbreak of cholera, provided that it could be obtained in a state of purity. In confirmation of his view of the sanative and medicinal virtues of vinegar, the author quoted Hippocrates, who (*de natura muliebri*) “employed

white vinegar medicinally"—Plutarch and Livy, who refer to the use of vinegar by Hannibal, in his passage over the Alps, when he is said to have "softened the rocks with fire and vinegar," an operation which the author facetiously regarded as rather metaphorical than chemical, as the vinegar, swallowed by the troops, probably sustained their strength, and thus in effect softened the asperities of their rough way. The author also quoted from Roman history the story that "Scipio Africanus is said to have gained a great battle with a few skins of vinegar," the troops refusing to march until the general had obtained a supply. Cæsar was also reported to mention in his Commentaries the supply of vinegar to the troops; and Mr. Tucker remarked that the drink of the Romans in all their campaigns was vinegar and water, and, sustained by that beverage, they conquered the world. Modern authors (Sir John Pringle, Sir Gilbert Blane, and others) were also quoted in proof of the antiseptic and medicinal qualities of vinegar. The author then proceeded to show that acid drinks were not only preventive, but remedial in epidemic disorders of the bowels. Cases were related, in which not only persons were exempt from attacks of cholera raging around them, who drank large draughts of cider, but a case of severe cholera was also related, which yielded to the diluted juice of sour apples. The efficacy of the *Mineral Acids*, especially the sulphuric, in diarrhœa, and especially in choleraic diarrhœa, was also advocated by reference to numerous facts and authorities. He also referred to some established facts connected with the spread of epidemic dysentery in the army, showing the efficacy of vegetable acids in that disease.

In conclusion, Mr. Tucker suggested a necessary caution relative to the use of the wretched and unwholesome substitute for vinegar commonly sold in the London shops.

The discussion which followed the reading of the paper, elicited many facts in confirmation of the author's views; and, as to the efficacy of sulphuric acid largely diluted with water, in choleraic diarrhœa, there was not a dissentient voice.—*Lancet*, July 15.

15. *Sulphuric Acid in the Treatment of Cholera and Choleraic Diarrhœa*.—Dr. H. W. FULLER, Assistant Physician to St. George's Hospital gives (*Med. Times and Gaz.* Aug. 12, 1854) the following statement relative to the efficiency of sulphuric acid in the treatment of cholera and choleraic diarrhœa, deduced not only from his own experience, but also from the concurrent testimony of a number of practitioners.

"Firstly. As to the form in which the medicine should be administered. One ounce of the dilute sulphuric acid of the Pharmacopœia should be added to eleven ounces of water, and of this mixture three table-spoonsful should be given as a dose. The acid is very grateful to the palate, and to the stomach; so I rarely mix it with syrup or any flavourings, which are apt to nauseate, if not to interfere with the action of the remedy. Sometimes, however, I add a drachm or half a drachm of chloric ether to every alternate dose of the medicine, and occasionally, at the outset of the attack, administer two grains of opium in a pill, combined, it may be, with five grains of calomel. If the first stage of the disease is passed, I never administer opium.

"Secondly. In ordinary cases of choleraic diarrhœa, implying by that term cases in which there is thin watery purging, and possibly vomiting, with faintness and coldness, and clamminess of the surface, a pale moist tongue, crampy pains in the belly, and possibly also cramp in the extremities, three or four doses of the acid mixture, taken at intervals of half an hour, will generally suffice to effect a cure. The vomiting and purging will be stayed, the cramps will subside, and heat will return to the extremities. Little or no after treatment is usually required.

"Thirdly. In confirmed cases of cholera, characterized by the symptoms already enumerated, but accompanied by a feeble or imperceptible pulse, collapse and blueness, or lividity of the countenance and extremities, a dose of the acid mixture should be administered every twenty minutes, until warmth returns to the extremities, and colour to the lips. The addition of chloric ether to each dose of the mixture is extremely serviceable, if tolerated by the stomach. As the

symptoms subside, the medicine should be exhibited at longer and longer intervals. In all cases, whether of confirmed cholera or of choleraic diarrhœa, a mustard poultice should be applied to the pit of the stomach, and frictions to the extremities, and a dose of the mixture should be given *immediately* after each act of vomiting.

"Fourthly. In cases of collapse, it is advisable to commence the treatment by a brisk mustard emetic. Through its agency, not only is the stomach emptied, and thus put in a state to be acted on by the medicine, but the dormant energies of the system are aroused.

"Fifthly. In no single instance, either in my own practice or in that of others, have I known the slightest ill effect from these full doses of the acid; while experience has proved that in very severe cases the medicine administered in smaller doses is inadequate to excite reaction and save life. Within my own experience, six doses have always proved sufficient to effect a cure, and I should not feel disposed to persevere in administering the medicine beyond the eighth dose.

"Sixthly. The cases in which the acid proves most efficacious are cases of true cholera and choleraic diarrhœa, marked by the symptoms already alluded to. In the majority of such cases it acts like a charm. In ordinary bilious diarrhœa, accompanied by a foul tongue and not by coldness of the surface, the ordinary remedies for diarrhœa have appeared to me more efficient. Even in these cases, however, no harm results from the administration of the acid, and much good is often effected; so that, during the continuance of the choleraic influence, every person may be safely recommended to keep a bottle of the acid mixture in his house for immediate use, until the arrival of a medical man.

"If the public were made aware of the fact, and were to act upon it, I am satisfied that the mortality from cholera would soon be reduced within very moderate limits. This I state as the result of actual experience in above a thousand cases, many of which have occurred in my own practice."

16. *Strychnine in Cholera*.—Prof. W. FRASER, of McGill College, Montreal, has been led, on physiological grounds, to try the effect, in cholera, of minute doses of strychnine as a *general stimulant*, for sustaining the vital powers on the eve of their failure, and for restoring them to functional action when that is all but suspended by the supervention of collapse.

The indications which have guided him in the administration of strychnine, are the following:—

"In all cases of real Asiatic cholera (tested by the evacuation of rice-water stools), even before the supervention of collapse, the coming failure of the circulation is indicated by less or more irregularity or fluttering in the rhythmic action of the pulse. When the case is seen at this stage, the strychnine should be at once commenced simultaneously with means for arresting and correcting the discharges. Given at this early period, it will rarely fail to arrest the patient's descent into collapse. From six to twelve doses will usually cause the pulse to become firmer and more regular, and the anemic appearance due to the serous discharges from the alimentary canal, to be replaced by an active capillary circulation. In those cases, on the other hand, that are in a state of collapse when first seen, the principal indications are to bring on reaction, and to arrest the discharges, when these still continue. All who have seen much of cholera, must be but too familiar with the frequent failure of ordinary stimulants in bringing on reaction; hence the necessity of employing such an extraordinary and powerful agent as strychnine, which, according to my experience, will often, but not always, succeed in doing so, in cases where all other stimulants would fail. In some desperate cases, other stimulants, as tea, camphor, brandy, or champagne, may be advantageously given, when the stomach will bear them concurrently with strychnine; they will, however, be very commonly rejected, while strychnine alone is retained."

The dose he has found "most suitable is the one-forty-eighth part of a grain, dissolved in acetic acid and alcohol, and repeated every quarter of an hour, or every five or ten minutes, in very severe cases, till the pulse, if it has been fluttering, as in incipient collapse, becomes steady, or till reaction is established

in cases of collapse—when these objects are accomplished, the interval between the doses ought to be lengthened—and should the specific effect of the medicine on the nervous system be produced, then its exhibition should be suspended till these disappear, when it may be again administered, if deemed requisite, at longer intervals. The medicine should be thus continued till the circulation is fully and firmly established, and the patient is fairly beyond the risk of collapse. During the administration of this dangerous remedy, the patient should be frequently seen by the physician, in order to watch its effects, and direct its discontinuance, should its effects on the nervous system manifest themselves.”

The number of cases in which Dr. F. administered the quinia is twenty-two. A tabular view of these are given.

This table shows “that the number of deaths in the twenty-two cases was five, or about twenty-two and a half per cent. It will also be noticed in regard to the five deaths, that in four of them reaction came on, and they died, not of cholera, or at least, not in the collapsed state of cholera, but of the secondary fever which so often follows it. The only one that did die in collapse was William Anderson, and he (though several hours in the hospital) was but one hour under the strychnine treatment, during which Mr. Loverin, the student who watched him, tells me he took four doses only, so that in his case it can hardly be said to have had a fair trial. I conceive it is, therefore, deserving of special notice, that in all the cases (and some of them were of the very worst description), except in Anderson’s, the strychnine was successful in accomplishing the purpose for which it was prescribed, namely, bringing on reaction, which tends to prove its superiority over all other stimulants hitherto employed in this disease.”—*Medical Chronicle*, Sept. 1854.

17. *Subnitrate of Bismuth administered in Enemata for the Cure of Diarrhoea and Inflammation of the Colon.*—Diarrhoea, attended with glairy and bloody evacuations, has been prevalent of late in France. Two infants, both at the breast, have been successfully treated by M. TROUSSEAU, by the subnitrate of bismuth, given in enema. One of them had had cholera, characterized by vomiting, diarrhoea, coldness, and well-marked emaciation. Opium, nitrate of silver, and other remedies were given, by which the sickness was arrested, but the diarrhoea continued. The other child had not had vomiting, but there was a glairy diarrhoea, showing that the disease was limited to the large intestine. M. Trousseau prescribed a lavement according to the following formula:—

Subnitrate of bismuth two scruples. Thick linseed-tea sufficient. The mixture to have the consistence of soup.

The diarrhoea ceased immediately upon the administration of the lavement.

It is known that M. Monneret gives in similar cases to infants as much as two or three ounces of the subnitrate of bismuth daily. M. Trousseau rarely goes beyond four scruples; very often he is content with half that quantity administered in water, or in the form of pastilles. Dr. Lassegue, chief of the clinique of the Hôtel-Dieu, first entertained the idea of employing it as a lavement in cases of inflammation of the colon, from having witnessed its beneficial effects in certain diseases of the skin. Some of the subnitrate mixed with albumen or thick linseed-tea, spread upon a poultice, rendered humid by the application of glycerin, was brought into contact with the affected parts. M. Lassegue carried out a somewhat similar plan in the large intestine (?), and obtained results which deserve to be recorded: the more so, because the remedy, being harmless, may be applied without fear to all similar cases.—*Journ. de Méd. et de Chirurg. Prat.*

18. *Tapeworm unsuccessfully treated by the Extract of Male Fern and Kousso.*—Dr. GEORGE PATTERSON relates (*Monthly Journ. Med. Sci.* July, 1854) three cases of tapeworm recently treated by him, and derives the following conclusions from them:—

1. It would appear from the above cases that the kousso and ethereal extract of male fern are, like all other remedies that have hitherto been prepared for tapeworm, liable to, at least, occasional failure. At the same time it is certain, from the effects that followed upon their administration in these instances, that

they are remedies of great power, very obnoxious to the parasite, and, therefore, valuable additions to our means of treatment.

2. I should be disposed to believe, from what was observed in these cases, that the same remedy is not suited alike to every case; but that in one subject the kouso, in another the male fern, and in another the turpentine, will be found to be the most appropriate remedy. Thus, in the second case, neither the male fern nor the kouso brought off any of the worm, while turpentine invariably did so. In the first, on the contrary, turpentine and castor-oil were repeatedly given before the male fern, without having the least effect. Again, we sometimes find the worm not only detached and dislodged, but carried downwards and expelled by the male fern and kouso, unaided by any purgative; while at other times they require to be followed by the administration of a purgative dose. As to the comparative energy of the kouso and male fern, it would appear from the above cases (in which both were successively given to the same person) to be nearly equal, and in the phenomena attendant upon their operation both remedies are very much alike.

3. The first of the cases I have narrated is in many respects a remarkable one. It furnishes a striking portraiture of the symptoms of the disease in a very aggravated form; and the immense number of joints that were discharged at various times, both singly and in pieces of several feet in length, would seem to indicate either that there must have been more than one of the parasites in the man's body, or else that it is capable of reproduction, and of attaining its former growth in an incredibly short space of time after large portions of it have been detached.

4. The same case affords an instance of the worm finding its way into the stomach, portions of it being stated to have been brought up by vomiting in round balls. This is a more common occurrence with other kinds of intestinal worms. Thus, I have met with several instances, both in children and adults, where lumbrici have escaped by the mouth or nose. In one case of an hysterical and hypochondriacal female, the circumstance of one of these worms having escaped by the mouth, produced an impression that she still had one in the wind-pipe. The sensation, which was partly hysterical, and partly perhaps due to an enlargement of the thyroid body, led to continual efforts to bring up the worm and dislodge it with the finger, which necessarily produced increased local irritation, and the failure of which aggravated her despondency and low spirits. She vainly resorted to several medical men, including myself, for relief—neither reasoning nor such remedial measures as the nature of the case suggested having succeeded in overcoming the delusion.

5. In the second case, we have an instance of the coexistence of another kind of worm with the tapeworm. This is worthy of notice, principally as showing (what has been pointed out by Dr. Watson and others) that the term "*solitaire*," which some French writers have applied to this worm as a distinguishing characteristic, is not correct.

6. Besides the above three cases, falling under my own notice in the course of a few months, I have heard of another instance, under the care of one of the medical gentlemen of this town, in which the kouso brought off several yards of a tapeworm, but equally failed to eradicate the disease.

19. *New Remedy for Tapeworm.*—C. MACKINNON, Esq. gives (*Indian Annals of Med. Sci.* Oct. 1853) the result of his administration of a remedy in tapeworm, new, probably, in European practice. This is the "*Kameyla*," a drug procurable in all the bazaars of Northern India, used as a dye by the natives, and in common use as a vermifuge for dogs. "My attention," he says, "was first called to it by a gunner of the brigade, affected with tapeworm, in whom both turpentine and kouso had failed to expel the worm. He stated that a companion of his, affected with tapeworm, had taken the remedy with success. I immediately sent for some, and without any previous preparation of the patient gave him three drachms. He was a large, powerful man, and this producing no effect, in four hours afterwards the same dose was repeated. It now operated very freely and frequently, and with the fourth stool a large tapeworm six yards long was passed.

This result was so satisfactory that I have continued to employ the remedy whenever a case presented itself; and I have now given it in sixteen different cases, and in all without a failure. As far as my experience goes, I have found it a better and more certain remedy than either turpentine or kousso, and much less disagreeable to take than either of these remedies.

In none of my cases, subsequent to the first, did I ever exceed, for a single dose, three drachms. This usually purges from five to seven times, and the worm is usually expelled dead in the fourth or fifth stool.

In two of the latter cases, in which I administered it in hospital, both patients recently recovered from fever and still weak, the dose of three drachms purged very violently from a dozen to fourteen times. In three subsequent cases, I reduced the dose to ζ iss, and no action on the bowels succeeding it, I gave, in six hours afterwards, ζ ss of castor-oil. This acted four or five times, and in each case the worm was passed dead. In one case, in which ζ ijj were given, though the medicine acted as usual, the worm was not passed until next day; and in another, in which the same dose was administered, followed by the usual operation, only a portion of the worm two feet long was passed. In this case, on the succeeding day, a dose of ζ iss was given, followed in six hours by ζ ss of castor-oil. No portion of the worm being passed in the evacuation following the castor-oil, on the succeeding day, ζ i given in the morning, again in the middle of the day, and again at night. Four or five motions followed the medicine given in this manner, and a worm eight feet long was passed dead.

In almost every case, the long slender neck of the worm appeared in the motion.

To a native child of five years of age, I gave a dose of forty grains, and a tapeworm as duly expelled. The drug usually purges speedily. In about half the cases, some degree of nausea and slight griping were experienced; in the remaining half no inconvenience whatever was sustained, some of the patients declaring it to be the easiest purge they had ever taken in their lives.

I have not thought it necessary to subject the patients to any previous preparation; but it is not unlikely that if a laxative were taken the day before, so as to sweep out the intestines, and the patient directed to fast, that a lesser dose might then act on the worm, deprived as it would then be of a protecting medium of feculent matter. I have usually given the remedy in ζ i of cinnamon water.

I do not advance the remedy as a specific against the disease; all remedies fail occasionally, and I suppose this one will also; but it has hitherto proved in my hands the most successful and most certain remedy in tænia that I have yet used. In at least three cases, in which turpentine and kousso both failed, I have succeeded with the "kameyla."

The following I should sum up as the results of my experience:—

1. That "kameyla" is a safe and efficient remedy for tapeworm, and more certain than either turpentine or kousso.

2. That to a strong European ζ ijj may be safely given as a dose.

3. To a person of feeble habit, or to a female, ζ iss, followed, if necessary, by ζ ss of castor-oil, is a sufficient dose.

The "kameyla," as sold in the bazaars, is a powder of a brownish-red colour. It is gathered from a shrub or tree, the *rotlera tinctoria* of the natural order *euphorbiaceæ*, which grows abundantly along the lower ranges of the Himalayas. It is a powder which is attached to the fruit-capsule, and when the fruit is ripe, it is brushed off and collected. It is used as a dye, imparting a bright orange colour to silk, and is sold in the drug mart of Umritsur, at twelve rs., a maund for the first quality, and ten rupees for the second.¹

The drug is used by native practitioners made into an ointment, as an external application in itch and other skin disorders, and its use as a vermifuge is also known to them. It is very probable that the attention of European practitioners has not been previously called to it as a vermifuge, from the smaller doses in which native practitioners administer it, and which must have rendered its use often unsuccessful."

¹ This is at the rate of 2 lbs. for sixpence.

20. *Cases of Gangrene of the Lung successfully treated by Inhalations of Turpentine.*—By Prof. SKODA, of Vienna. These cases are three in number, and Dr. Skoda has made use, in each of them, of the vapours of turpentine, along with sulphate of quinia. The first case is that of a domestic servant, who was affected with gangrene limited to the upper lobe of the right lung. After six weeks of this treatment, all the physical signs had disappeared, and vesicular respiration free from râles was audible. The patient was seen three months later, and remained perfectly well. The subject of the second case was an innkeeper, of strong constitution, somewhat advanced in life, in whom a gangrenous cavity existed in the lower lobe of the right lung. The disease dated from the 11th of March, 1852. On the 21st of March, inhalations of turpentine were commenced. The patient employed them without any repugnance for five or ten minutes every two hours, and in addition took every two hours a grain of sulphate of quinia. In three weeks the expectoration, which had been very abundant, was reduced to a sixth. In six weeks the patient could leave his bed, but the expectoration continued sanious and of a fetid odour. His strength returned, his appetite improved, and in the month of June his general appearance was satisfactory, although cough and expectoration, occasionally fetid, continued. The right side of the thorax was painful, the breathing difficult, but dulness on percussion had entirely disappeared; the respiration was indeterminate, and accompanied by feeble sibilant râles. The patient went to the country and continued the turpentine inhalations twice a day till the month of July, by which time the cough and expectoration had entirely disappeared. In December, he had perfectly regained his flesh and strength, was free from cough or pain, no retraction of the thorax had taken place, and vesicular breathing was everywhere audible. The third case was that of a butcher, suffering from gangrene of the lower lobe of the left lung. He turned ill towards the end of May, 1852. On the 4th of June, he began to employ turpentine inhalations. Within eight days the fetid expectoration had ceased, and the patient believing himself recovered, as the pain and oppression had diminished, ceased the use of the turpentine, as it was disagreeable to him. On the 19th of June, a violent shivering came on, accompanied by cough, dyspnoea, and fetid expectoration. The inhalations were resumed, and gradual improvement took place. They were, however, again suspended, but a fresh exacerbation occurred. His appetite was gone, and his emaciation and weakness were such that, about the end of July, he could not leave his bed. Fowler's solution was then had recourse to, and under its influence, as well as that of the inhalations, the patient recovered so far that he could leave his bed and go to the country. By the middle of October, he had regained flesh and a considerable degree of strength; but a feeling of pain and oppression remained below the scapula and in the left side of the thorax, while there was a little cough with slight fetid expectoration from time to time. Vesicular respiration was audible to the base of the lung, but dulness continued to occupy an extent equal to the size of a pleximeter, in the neighbourhood of the apex of the heart; at this level faint râles were audible. The turpentine inhalations were ordered to be continued. At the end of January the patient believed himself cured; for ten days he had had no chest symptom, when, on the 10th of February, after a slight fit of coughing, he expectorated two ounces of blood, and after it some fetid pus. There were no signs of a new infiltration. The blood probably came from the gangrenous cavity, the walls of which had become callous.—*Zeitschrift für K.K. Gesellschaft der Aertze zu Wien, in L'Union Médicale*, Feb. 16, 1854.

[The inhalations are practised by pouring a little oil of turpentine upon boiling water, and respiring the vapours.]—*Monthly Journ. Med. Sci.* July, 1854.

21. *Prurigo Formicans of the Vulva and Anus.*—M. RICHARD recommends (*Revue de Thérapeutique Médico Chirurgicale*) the following preparation as extremely useful in the cure of prurigo formicans and various kinds of eruption: Take of sulphate of zinc and sulphate of alumine equal parts; reduce these salts to a coarse powder, and put them upon an earthen dish coated with oxide of lead; place the dish on a gentle fire, and leave it until the mixture no longer emits air-bubbles and is turned into a stony consistence. Take the dish from

the fire, pound the petrous substance into a fine powder, and throw about half an ounce of it, in divided parts, at about a minute's interval (to avoid too powerful an effervescence), into a quart of boiling water; then filter through paper, and keep the lotion for use. Every morning apply some of this solution with a fine sponge on the affected part; then, if it be the anal orifice which is suffering from prurigo, soak a piece of linen folded square (a little better than an inch to each side), and introduce one of the angles into the orifice. The mode of dressing is the same for the vulva. This lotion is also extremely useful for various kinds of eruption, but in the latter cases internal medicines should also be taken; these should principally consist of alkalies, bitters, and purgatives.

22. *Successful Mode of Treating Mercurial Salivation.*—Dr. NORMAN CHEVERS states (*Indian Annals of Medical Science*, April 1854), that he has met with uniform and apparently certain success from the use of iodine gargles in the worst cases of mercurial ptyalism. He has found that a gargle containing from two to five drachms of the compound tincture of iodine to eight ounces of water, exerts an absolutely prophylactic or curative influence. He states also, that Mr. Burgess has applied the pure tincture to the whole interior of the mouth, in cases of severe mercurial salivation, with speedy and perfect success.

Dr. Chevers, among other cases, quotes the following, in illustration of the benefit of this treatment:—

Cure of Ordinary Cases of Salivation.—In February 1852, I attended an officer, ætat 48, in an attack of cholera, which was then raging epidemically in Chittagong. The disease was generally attended with extreme danger, and this case was one of remarkable severity. During the first sixteen hours, I administered seventy-five grains of calomel. On the third day the sputa became tinged with blood, the gums were swollen and tender, and the spaces between the teeth were filled with coagula. A gargle, containing two drachms of compound tincture of iodine, to eight ounces of water, removed all traces of salivation so effectually within about two days, that my patient, although a very intelligent man, and a rather active dabbler in physis, never appeared to be aware that he had been subjected to mercurial treatment.

“Early in last year, I was called to attend a lady, about thirty-four years of age, who had been suddenly attacked with an excruciating pain across the umbilical region, which appeared to be associated with a sudden check to the catamenial function, resulting from exposure to a draught while very thinly clad. The symptoms were extremely urgent, and a dose of ten grains of calomel was among the first remedies employed. Relief was obtained almost at once; but on the second day, the tongue was found swollen, and clots occupied the interspaces of the teeth, but little uneasiness was complained of. The iodine gargle was employed with such rapid success, that the patient scarcely referred a second time to the condition of her mouth.

Employment of Iodine as a Prophylactic.—Of late, I have been in the habit of beginning to employ the gargle in all cases where the quantity of mercury given has been such as to render the occurrence of salivation probable. Judging from a confessedly very limited experience of this measure, I apprehend that its early employment will anticipate the occurrence of salivation in all cases where the constitution is good, and there is little or no visceral disease; that, even under the worst circumstances, it will greatly limit the severity of the action; and that, for the most part, the original disease, on account of which mercury was administered, will have its decline rather favoured than otherwise by the absorption of iodine from the mucous membrane of the mouth. A certain degree of doubt will, of course, attend nearly all details of prophylactic treatment, but I think that the following cases may be regarded as encouraging.

“Early in the last rains, I was requested to visit a medical officer suffering extremely from an attack of ileus, which I attributed to the sudden outflow of a quantity of a highly vitiated bile, acting as an almost corrosive irritant upon the mucous membrane of the small intestines. When called to him, I found him greatly weakened by intense pain and obstinate vomiting, and by oozing from the bites of several leeches which he had himself applied to his abdomen.

He had already taken three or more five-grain doses of calomel, which, however, had not acted upon the bowels. I administered a ten-grain dose almost immediately; and, the disease remaining obstinate, a scruple dose was recommended by Dr. Miller, who met me in consultation, and was given on the following day. The iodine gargle was employed early; and, although it was nearly certain that a large proportion of from forty-five to fifty grains of calomel must have fully entered the system, ptyalism did not occur.

"In October last, I attended the children of an European for mumps, which was then rather prevalent in Howrah and its vicinity. All these children had suffered from hooping-cough during the preceding winter. The elder boys did well, but the two youngest, aged respectively about two and four years, were suddenly attacked with laryngitis as the swelling of the parotids began to decrease. Although very actively treated, the younger infant died in little more than twenty-four hours from the onset of the laryngeal symptoms. I found the larynx and trachea absolutely occluded by an exceedingly tenacious croupy deposit. The other child's symptoms were equally violent, but he recovered under very severe treatment, a part of which consisted in the administration of fifteen grains of calomel within as many hours. In a day or two, one or two aphthæ appeared on the tongue, yet it could scarcely be said that salivation was present. The gargle was used freely and no further annoyance was experienced, although the tongue has ever since presented that patchy appearance not unfrequently noticed among delicate children in India."

23. *Diabetes Mellitus*.—Probably one of the most elaborate papers ever written on this disease has just been published by Dr. TH. VON DURSCH, of Manheim. It is founded upon two very interesting cases of diabetes, of which he has given very careful clinical reports, and also accounts of the pathological lesions found after death. With indefatigable perseverance and most praiseworthy zeal, he carefully ascertained, every day for several months, the nature and amount of the food and drink taken by his patients; the amount of fluid contained in the aliment; the quantity of urine excreted, its specific gravity, and the amount of sugar it contained; the number of the stools, the proportion of their watery constituents; the amount of water exhaled by the lungs and transpired by the skin, &c. The results of these most laborious and minute investigations he has condensed into two large synoptical tables, which are appended to the memoir. In one of these, full particulars are given of the effects of different kinds of diet upon the total amount, specific gravity, and saccharine constituents of the urine. As far as our limited space permits, we shall now glance at the general results thus tabulated. *First*, when the patient was put upon a mixed diet for forty days; *second*, when a farinaceous diet alone was allowed during eight days; and, *thirdly*, when animal food was given during a period of five days.

1. *Effects of mixed diet on*

(a). *The specific gravity*.—The average density of the urine, while this regimen was adhered to, was 1037.8; it was higher in the mornings and evenings (1038) than during the day (1036).

(b). *(The amount of the urine)*.—The daily average was 523.4 cubic centimetres. The quantity voided was greater in the morning (197.1 c.c.) and evening (183.1 c.c.) than during the day (143.0).

(c). *The percentage of sugar*.—The average amount of saccharine matter was 9.134 in the 100 grammes. The percentage was lower in the morning and mid-day urine (8.9) than in that passed at night (9.4).

(d). *The total amount of sugar*.—The average daily amount of sugar excreted during the whole period was 477.7 grammes; the lowest was 350, and the highest, 615 grammes.

2. *Effects of farinaceous diet on*

(a). *The specific gravity*.—This continued nearly the same as with mixed diet; the average was 1037.6. With this regimen, also, it was lower during the day than at night or morning.

(b). *The amount of the urine*.—This was increased to 560.4 cubic centimetres

as its daily average. It was greater in the morning (2165 c.c.) than during the day (1737 c.c.) or at night (1701).

(c). *The percentage of sugar.*—This continued nearly unchanged, being on an average 9.39 in 100 grammes.

(d). *The total amount of sugar.*—In this a considerable increase was visible, while the farinaceous diet was continued. The average quantity of sugar daily excreted amounted to 526.4 grammes; and the urine in the morning contained more (201 gr.) than at noon (159g.), or at night (165 gr.).

3. *The effect of animal diet on*

(a). *The specific gravity.*—It remained nearly unaltered by this regimen, as happened with both the other diets. Its average was 1037.2; and it was lower in the morning (1036) than at other times (1037).

(b). *The amount of the urine.*—This was considerably diminished, the average quantity *per diem* being 4588 cubic centimetres. The average amount was much greater in the morning (1816 c.c.) than during the day (1324 c.c.) or at night (1448 c.c.).

(c). *The percentage of sugar.*—This was also lessened, being on an average 8.232 in 100 grammes.

(d). *The total amount of sugar.*—Here, likewise, a striking diminution was manifest. The average quantity daily excreted was 379.8 grammes; this was greatest in the morning (139 grs.), and less during the day (114 grs.) than at night (126 grs.).

Dr. Von Dursch discusses several of the questions relative to diabetes, and brings to bear upon them the weight of his experience and careful observations. As regards the disputed point, whether the quantity of the urine voided in this disease surpasses the amount of the fluids absorbed, he thinks that the question has not been properly considered, and that we ought to compare the amount of water in the urine, &c., with that contained in the food and drink taken. During his investigations, he ascertained the amount of the cutaneous transpiration and pulmonary exhalation by frequently weighing his patient; and he also carefully noted the quantity of water contained in the feces. He has succeeded thereby in satisfying himself that the water given off by the patient equals exactly the amount of the water absorbed by him.

In conclusion, the author believes, from all his researches, that diabetes principally depends on the sugar normally existing in the blood being undestroyed and unappropriated; and he is of opinion that all kinds of food are capable of producing sugar.—*Monthly Journ. Med. Sci.*, from *Henle und Pfeuffer's Zeitsch. für Rationelle Medicin.* 1853. Bd. iv. Hft. i.

24. *Betz on the Thyroid Asthma of Young Children.*—Medical men are divided on the subject of thyroid and thymic asthma; some acknowledging, and others altogether denying their existence. Alois Bednar, for example, in his recent work (*Krankheiten der Neugeborenen*, pt. 3, page 77), does not believe in the existence of asthma resulting from hypertrophy of the thyroid gland alone. He relates ten cases of thyroid enlargement, nine of which were attended with asthmatic symptoms; but in all of these nine cases there existed other pathological affections of the air-passages, sufficient to account for the dyspnoea. He alleges that the thyroid gland may be enlarged without the respiration being affected, but confesses that most cases of hypertrophy are attended by a difficulty of breathing; and he considers the dyspnoea so induced as not asthmatic in its character. Friedrich Betz, of Heilbron, has published an interesting paper on the subject, in which he regards the impediment to the entrance of air, in cases of thyroid hypertrophy, as existing neither at the upper part of the trachea nor in the larynx, but higher up, at the entrance to the air-passage, where the enlarged gland projects into the floor of the mouth. The thyroid body has this peculiarity of conformation in new-born infants, so that both its lateral lobes extend backwards, between the pharynx and the vertebral column; and, according to Albers (*Constatt's Jahresbericht*, 1848), the lobes may even become blended together, so as to form a fleshy ring round the larynx and top of the pharynx.

When, therefore, its extremities become swollen, they constrict the parts inclosed within them.

This affection is easily recognized by swelling of the upper part of the neck, and sensible enlargement of the thyroid body. Expiration and inspiration are both stridulous; the sounds continue without intermission, only remitting during sleep. The mouth is filled with frothy saliva; the hands, feet, lips, and face, are cold and bluish; there is no fever, there is an occasional paroxysm of coughing, and the sleep is short and broken. Suckling is impossible, for the attempts to swallow fluids increase the suffocative dyspnœa. In severe cases, death occurs after from one to four days; in those which are milder, the swelling gradually subsides, and with it the dysphagia and dyspnœa. It assists our diagnosis to remember that this disease is limited, the first eight to fourteen days after birth.—*Monthly Journ. Med. Sci.* July, 1854, from *Jour. für Kinderkrankh.* Sept. and Oct. 1853.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

25. *Modern Philosophy of Cancer*.—ROBERT DRUITT, Esq., in an interesting paper read before the Medical Society of London (*Assoc. Med. Journ.* Jan. 13, 1854), calls attention to the necessity of reform of the classification of cancer, and of the diseases allied to it. These diseases have been classified on three principles: 1. The outward resemblance of morbid growths to natural structures, or to common objects in nature; 2. On the vital qualities of morbid growths—their effects on the health, their curability or fatality; and 3. Physical structure. Mr. Drutt advocates the last base of classification. He thus defines cancer:—

“Cancer is a disease of the blood, manifested by the evolution, in some part of the animal economy, of a structure called the cancer nucleus. This is a bright, oval, well-defined plump body, averaging a little less than one-two-thousandth of an inch in its long diameter, and containing one or more large and distinct nucleoli. In soft, *intense*, rapidly growing cancerous tumours, these nuclei may be free, floating in a liquid, or may be imbedded in a soft, transparent amorphous substance; but usually, this amorphous substance is gathered about the nucleus in a more or less definite spherical mass, called a cell. One cell may inclose one or more nuclei. It becomes much more visible by the addition of water, but vanishes on the addition of acetic acid, which renders the nucleus more bright and distinct. Taken together, the characters of the cancer nucleated cell are, the large size of the nucleus, and of its nucleoli; and the fact that the cells, if abundant and adhering, and ill-developed, are heaped together amorphously; that, if few and well-developed, they have no adhesions to the tissues around them, but readily exude from a cut surface in a milky liquid, called the cancerous juice. This juice in general mixes readily with water; and the cancer cells being, like the similar bodies in milk and pus, not adherent to each other, are equally diffused throughout the liquid, and do not usually clot together as some other cells do.

But, since cell-structures, as they are called, have, besides the cells, some intercellular matter, so in cancer, besides the cancer cell, there is usually some intercell, the varieties of which occasion the varieties of the species. Cancer cells abundantly heaped together, with liquid, or with scanty and delicate filamentous tissue, constitute soft cancer, which is the most intense and rapidly growing example of the species, and which is usually described as the medullary or encephaloid. Scantier growth of cells, more closely packed together in the meshes of fibrous tissue, constitutes the hard, better known as the scirrhus cancer. Immense development of loops of bloodvessels, accompanying superficial cancer, causes it to be called *villous*. When, together with the cancer cells, there is development of a peculiar jelly-like matter, or of a dense imper-

fect bony matter, the cancer is called colloid or osteoid respectively. Other varieties depend on other incidental circumstances; thus infiltration with black pigment gives rise to what is usually called melanic or melanotic, but which might as well be called, in plain English, black cancer. Fatty degeneration occasions the appearance called reticular by Müller, and phymatoid by Lebert. Infiltration with blood produces the well-known fungus hæmatodes.

Whilst alluding to varieties of cancer, I may well take the term *melanosis* as an example of the mischief and confusion arising from the want of definite principles of nomenclature. I might defy any person, reading certain books and journals for the last twenty years, to form a clear idea of what is meant by melanosis. By some persons, the term is used as if synonymous with cancer; by others, not. It is a fair instance of a name which is popular because founded on a character which is striking, but erroneous; because founded on a character which is accidental and variable. In man, black pigment is found in those cutaneous growths commonly called moles; it is also found in some specimens of cancer. In the horse (especially in old grays), deposits of pigment are very commonly associated with fibrous or fibro-plastic growths, which become diffused over the body, just as black cancer in man, and which may soften and ulcerate, or may cause death by accumulating and interfering with the functions of various organs; or, on the contrary, may remain harmless, and allow the animal to reach a good old age. Whether any of the black tumours in man, which may be *diffused* over the body, are of this sort, I know not. But, to return to the subject of classification, the presence of black pigment is no proof that a tumour is a cancer, or that it is not; and it is a caricature of pathology to see, on one shelf in a museum, black cancer from man, and black fibrous tumours from the horse, all arranged under the common head 'Melanosis.'

To return to the subject of cancer. When we speak of *varieties*, we imply the existence of a definite form, the presence of which determines the species; and we believe that the presence of cancer is determined by the existence of the cancer nucleus or cell. Where these exist, there is cancer, no matter with what variety of accompanying circumstances. On the contrary, where there is no cancer nucleus or cell, there, we hold, is no cancer, no matter what else there may be as to structure, and no matter whether the morbid structure which does exist be, or be not, quite as destructive as cancer.

I know very well, however, that some of my professional friends raise an objection to the anatomical or structural basis of classification, on the ground that the anatomical element of cancer, the cell, is not a sufficiently positive quality; or that cancers may exist in which no cancer cell can be detected.

To this, the reply is simple and logical. If, in any growth, after thorough examination by competent persons, no cancer cell or nucleus can be detected, that is evidently not a cancer. Let its anatomical elements be figured and described, and let its clinical history be drawn up for comparison with similar specimens which may hereafter be met with, and let some new and positive name be given to it; but if in any disease, no matter what it is, and how it proves fatal, nothing is found which can be identified with the cancer nucleus, that disease ought to be called by some other name than cancer.

Again, in the search for this structure, there are some sources of error and difficulty which require to be well understood, and which I will describe, as I have myself met with them.

In the first place, the observer, who is studying the cancer structure, must be prepared to meet—just as he must in studying any other series of natural objects—with imperfect, immature, and ill-developed specimens. This, as Lebert observes, will most probably be the case in the examination of rapidly growing and exuberant soft cancers—the fungus hæmatodes or medullaris of authors. Here the nuclei in large masses may be small, and the nucleoli ill marked; the cell substance being hardly defined.

Again, in other specimens, there may be difficulty arising from a directly opposite source, namely, degeneration; for the cells constituting portions of a tumour may be dried up and shrivelled, or filled with oil-globules, or broken down and mixed with large granular cells and crystals of fatty matter.

Again, in every cancerous tumour, the whole mass is not cancer; and it is perfectly possible that portions of a specimen may consist of gelatinous stuff, imperfectly organized areolar tissue, exhibiting fibrillary matter, intermixed with the elongated, caudate, fusiform, and bifid nuclei and cells which may be found in almost every growth; or there may be a confusion, arising from the remains of the structures into which the cancer is deposited.

I mention these possible sources of error, with the further observation, that they are met with equally in all other branches of natural science. Ill-developed, or withered, or monstrous specimens, may at times perplex the botanist: but practice, and the art of distinguishing essential characters which it teaches, soon overcome the difficulty. In fact, if any one denies the existence of an anatomical characteristic in cancer, we may ask how he can account for the fact that such men as Hughes Bennett, Paget, Lebert, Hannover, and a host of others, working independently, and far apart, can find, describe, and figure such a structure, and agree with each other in so doing? From my own observations, I believe that any one, who will take the trouble of procuring and examining specimens, and who guards against the possible sources of error which I have pointed out, will soon be convinced of the existence of the cancer cell. Then, to repeat what I have said before, under whatever circumstances this cancer structure may be found, whether it cause hypertrophy or atrophy of surrounding parts; whether it be melanic in colour, or xanthous; whether universally diffused and quickly fatal, or whether it lingers on in one organ, and allows the patient to die of some other disease—there is cancer, with its one specific determining sign.

Now, this mode of treating the subject is simple and definite, and allows statistics a sure basis to rest upon; and it allows morbid products to be accurately defined by their physical forms at the various epochs of their development; and thus the vital qualities and clinical history, the effects of causes in producing, and of remedies in curing them, can be accurately stated. For instance, the epithelioma (or epithelial cancer, as it is often called) differs from cancer in its anatomical structure; for, instead of the cancer cell, the affected parts are infiltrated with abundance of scaly epithelium, larger than cancer cells, flat, folded, often adhering together, with less distinct nuclei, and very seldom a nucleolus. These also exude from a cut surface, mixed with a serous liquid, and constituting a *juice*; but this juice differs from the cancer juice in being not so equally diffusible through water, and in the fact that the cells adhere in clots or pellets. They are also often found rolled together in large concentric masses. This disease usually seems to begin in the rete mucosum, by a destruction somehow of that liminary membrane, which confines epithelium to the surface. As it differs from cancer in its anatomical elements, so it does likewise in its favourite seats, in its etiology and clinical history, and susceptibility of cure. It is apparently much more liable to be produced by local causes. It almost invariably commences on cutaneous or mucous surfaces, such as the lower lip, penis, scrotum, tongue, larynx, &c. It is excessively liable to relapse if cut out, but, so far as is known, always comes back in the part first attacked, or in the nearest lymphatics; and does not, like cancer, become diffused over the body. Lastly, there is reason for believing that, in epithelioma of *certain parts* of the body, excision may probably lead to a perfect and lasting cure. In the same manner, the fibro-plastic tumours may be defined as consisting of small round or oval cells, with small flattened nuclei, intermixed with intercellular fibrillary matter. The cells and nuclei may be in various degrees of elongation, and the fibrillary matter tough as hemp, or soft and flickering like jelly. Glandular tumours are composed of tissue, like that of secreting glands. Many other varieties of growth might be so defined; but my present purpose will be answered if I show that the plan of classifying according to structure is simple and intelligible, and permits the clinical and vital history of each growth to be fairly studied and compared.

On the other hand, the plan of classifying growths, not according to their structure, but according to their vital characteristics, is based upon the doctrine stated by the most illustrious of English pathologists, that we ought to 'choose modes of life rather than structure for determining the affinities of

morbid products, and for arranging them under generic names. As of all tumours, so especially of cancers, the true nature is to be apprehended only by studying them as living things.' Or, as the doctrine is still more broadly stated by some of my professional friends, if any tumour behaves as cancer does, it is so far 'malignant' or 'cancerous.' For instance, a glandular tumour is removed, and never returns; this is a glandular tumour *pure et simple*. Another glandular tumour returns after extirpation, again and again. This is said to be 'malignant,' or to 'partake of malignancy,' or to be 'disposed to take on malignant action.'

Now, after careful study of the subject, the continued use of the word malignant, instead of structural terms, to characterize morbid growths—and this in spite of the unanswerable objections of Walshe, Lebert, and others to the word—seemed to me at once so illogical and mischievous, that I determined to bring the subject for discussion before the Medical Society of London, in the hope of inducing my professional brethren to agree with me in the necessity of stricter terms. In the course of my investigations, I endeavoured to analyze the term malignant into the several qualities which, when combined, constitute 'malignancy;' and, at the same time, I endeavoured to frame a statistical table of morbid growths, which should show how far, and in what proportion of cases, the separate items of malignancy were exhibited by each growth. I soon found that accurate data for such a table are not yet to be had; but the analysis of malignancy may be of some use, as exhibiting certain pathological facts in a new light.

The vital characteristics of cancer may be described in about fourteen terms, the sum of which equals the word malignant. They are: 1. Constitutional Origin; 2. Rapid Progress; 3. Constant Progress; 4. Pain; 5. Return after Extirpation; 6. Secondary Deposits; 7. Diffusion; 8. Cachexia; 9. Resistance to Treatment; 10. An Infiltrating Mode of Growth; 11. Degeneration and Softening; 12. Ulceration; 13. Invasion of Lymphatics; 14. Heterology of Structure; 15. Death. As an accidental and occasional accompaniment, Inflammation or Hyperæmia deserves one word of notice.

1. *Constitutionality of Origin* is the first of these characters of malignancy; and since even now we sometimes hear the opinion that cancer is at first a local malady, that it contaminates the system at large only after a certain length of existence, and that, by means of early extirpation, this contamination may be prevented, we may state that the reason why cancer is called constitutional is, because, in an infinite majority of cases, if true cancer structure exists, though it be completely extirpated so soon as conceivable, there will yet be a return either in the part first affected or elsewhere.

But it is frequently held, even by most eminent pathologists, that, whilst cancer is constitutional, other growths, such as the fibrous, are local in their nature; and, since confusion in the use of terms is one of the greatest obstacles to the progress of any science, let me, without going into scholastic subtleties, endeavour to give a precise definition of these terms. A local malady is one the causes of whose origin and continuance are strictly confined to the part affected, or one which, if caused at first by constitutional causes, wholly or concurrently, is no longer dependent upon them. A constitutional malady, on the contrary, has its origin in causes affecting the whole body; or is perpetuated, extended, and maintained by such causes, even though its first origin were local. But, when we speak of causes affecting the whole body, we must refer either to deviations from the healthy composition of the blood, and of the other parts of the material fabric; or else to disturbances in the laws which govern growth and development; although we can hardly separate our ideas of such laws from our idea of the material substances by and upon which their existence is made known to us.

Now, tried by these definitions, I can scarcely conceive of a morbid growth which has not at least a concurrent origin in constitutional causes.

Take the case of the most innocent and local tumours, as they are reputed—those fibrous or fibro-plastic tumours which are caused by local irritation. Thousands of women have their ears bored for ear-rings; thousands of children have leeches applied; and men are cupped often: yet it happens as a most rare

case that a fibrous tumour is developed in the *borèd* lobules of the ears. I have seen raised circumscribed fibrous lumps in the site of each lèech-bite in a child, and similar ridges in the site of an old cupping; but, as these effects are not produced on all patients by these local injuries, there we must admit some constitutional peculiarity in the patient to account for them, or else we must abandon the doctrine which is the foundation of all physical science, that like causes produce, *cæteris paribus*, like effects.

I will pass over the fibro-plastic tumour, since it is generally admitted that many of the deposits which come under this head are connected with some cachexia, perhaps the syphilitic or scrofulous; merely adducing as an example the well-known *tarsal tumour* which infests the eyelids, and the origin of which I believe to be always referable to some degree of debility or cachexia. Certes, I have known patients affected by them at one period of their lives, and quite exempt at others. But I will adduce the glandular tumour, of which a fair example is to be found in the common bronchocele. Of this, a true tumour, consisting in exaggerated development of gland tissue, whether it be of the severe endemic sort prevalent in mountainous countries, or of the milder variety which may affect women anywhere, it is impossible to deny the constitutional origin. In every case which I have seen in London, there has been some concurrent deviation from health, and always on the side of deficiency—anæmia, atonic dyspepsia, or amenorrhœa. I have patients in whom the earliest sign of getting out of health is enlargement of the thyroid gland, and in whom that enlargement subsides as the health improves. In one lady, it is always caused by a residence at the sea, and disappears when she removes inland. The connection of the chronic mammary tumour (as the abnormal development of gland tissue in the breast is called) with deviations from health is sometimes palpable, often not so; but the true idea of the significance of such tumours is, as it appears to me, to be found in the study of the origin of the same tissue when occurring normally. Whatever meaning may be assigned to the term constitutional, there is no act in the whole career of life so truly constitutional as the development of gland tissue. From the Wolfian bodies, the thymus, and the supra-renal capsules, to the later developed sexual glands, there are no structures in the body whose development and decay are so inseparably connected with the most general laws of growth, and with the composition of the entire solids and fluids, as the glands; and what may be said of the normal, I do not hesitate to apply to the abnormal development of gland tissue.

Space compels me to pass over the bony, the cartilaginous, and other tumours, reputed local, with the observation that it is most difficult to conceive of hereditary, multiplied, and symmetrical tumours, as merely local: I must conclude this part of my subject by stating my belief that all tumours, properly so called, are constitutional. In what respects they differ, in this quality, I shall say presently.

2 and 3. *Rapidity and Constancy of Increase.* I may pass rapidly over the second and third signs of malignancy, because, though more characteristic of cancer than of any other growth, yet they have little absolute diagnostic value, and afford scope for little remark. Not so the fourth characteristic.

4. *Pain.* This is a symptom which attends morbid growths of all kinds, in differing degrees, and the *rationale* of which is not evident at first sight. Now, exclusive of pain depending on accidental inflammation, or on pressure or distension, or on the existence of an ulcerated surface, all of which kinds of pain may be common to other morbid growths as well as to cancer, there is another pain, of a purely neuralgic sort—stabbing, lancinating, or aching. But, although this is not so characteristic of cancer as it is of certain small fibrous tumours which, from that very circumstance, are called *painful subcutaneous*; and, although glandular tumours of the breast, and other tumours elsewhere, are sometimes accompanied with extreme neuralgic pain, yet it is extremely valuable as a diagnostic sign, and especially in the case of secondary cancer. This was pointed out to me long ago by my friend Dr. Ferguson, from whom I derived the following most useful practical rule: If, after extirpation of a doubtful tumour, the patient complains of neuralgia, secondary cancer may be

predicated. By this rule, Dr. Ferguson was able to give a diagnosis in a case of cancer within the skull, at a time when the doctrine of the painfulness of secondary cancer was not generally received by the profession. Of neuralgic pain, too, there are many varieties. Dr. Ferguson believes that, at the time of the formation of secondary deposits of cancer, much of the most acute and agonizing pain may be owing to the presence of cancerous plasma within the capillaries—a thing rendered probable by the intense pain caused by the injection of morbid fluids into the arteries of animals. At all events, the significance of pain, as a rational sign of cancer during the latter two-thirds of its existence, is scarcely to be overrated.

5. *Return after Extirpation.* In speaking of the fifth malignant character, a distinction must be drawn between tumours which return because some portion of the original is left to repullulate, and others which return in spite of the freest extirpation; but yet the doctrine so often insisted on by Lebert, that the return of non-cancerous tumours is generally due to incomplete extirpation, is, I think, scarcely consistent with true pathology. At the same time, this doctrine tends to keep out of sight the true facts relating to the return of tumours, which, I think, exhibit strongly their agreement in constitutional origin, and their differences; which may be summed up in a formula something like the following, viz: That, of some tumours, the constitutional causes are transitory; the morbid material is consumed, the necessary conditions are exhausted, the aberrant laws fulfilled, by the very production of the tumour; which, if then extirpated, is no more reproduced than an arm or leg would be; that, of other tumours, the constitutional cause, once established, is permanent; and extirpation is sure to be followed by reproduction.

That there is some common bond of connection between various tumours, especially the glandular, cartilaginous, and epithelial, is another proposition which may be proved at the same time.

On tabulating a certain number of cases of tumours of external parts (not including the uterus or viscera), including those mentioned by Paget in his Lectures, in the Catalogues of the Museums of St. Bartholomew's, and of the Royal College of Surgeons, many from Hughes Bennett, Lebert, and Warren, and from the medical periodicals and private sources, it appears that there is no tumour whatever which may not return after extirpation, even though the general characteristic may be the reverse. No better example can be desired than the case related by Paget,¹ of a fibrous tumour, yielding gelatin on boiling, removed twice, and returning, whilst similar tumours were developed in the lungs, as was ascertained by *post-mortem* inspection. In this series of cases can be found a dozen instances of such tumours removed twice, one instance of six removals, ten instances of cartilaginous tumours returning, and so forth. The true conclusion seems to be, that return after extirpation, from a continuance of the original cause, is a thing *possible* in the clinical history of any tumour whatever, and deserves to be known and commented on as a fact. But when we find the fact of recurrence made by some pathologists a ground of classification, and read of subspecies of 'recurring cartilaginous,' 'malignant glandular' tumours, &c., we must say that this fact is no better a ground for classification than if some botanist, disregarding structural affinities, should erect the *triticum repens* or horseradish into separate divisions from their kindred species, on the plea of the difficulty of uprooting them from the soil.

The comparison of the same series of cases tends to show that there is some alliance or connection between the causes of various kinds of tumours. This is evident from the instances of various growths combined in one tumour, of enchondroma combined with cancer (or, in the testicle, with any other growth), of glandular tumour, and of epithelioma combined with cancer. It is evident, too, from the instances of one sort of tumour succeeded after extirpation by another. For instance, in the St. Bartholomew's Catalogue; a glandular tu-

¹ Lectures, ii. 153; also Catalogue of the St. Bartholomew's Hospital Museum. For particulars of the cases referred to, see Paget, Lectures; Warren on Tumours; Hughes Bennett on Cancer and Cancroid; Hannover, Epithelioma; Catalogues of the Museums of St. Bartholomew's and of the Royal College of Surgeons.

mour of the breast, excised; tumour returns in opposite breast; excised; followed by a tumour in the former breast, half fibrous, half medullary; excised; followed by a brain-like medullary tumour on sternum. The affinity of various tumours, too, is evident from the instances of enchondroma and other tumours occurring in persons of cancerous families, and from the occurrence of cancer in persons whose parents had died of epithelioma. Lastly, it is evident from the acknowledged frequency with which cancer and epithelioma supervene on warts, moles, and nævi. Now, since this fact, and since the true nature of warts and moles are, from their very triviality, overlooked, let me observe that they are true fibrous or fibro-plastic tumours of the skin, which, in warts, are accompanied by hypertrophy of papillæ; in black moles, by deposit of pigment; whilst in the common raised smooth mole, there is a distinct deposit of a fibro-nucleated matter within the meshes of the cutis, but without the black pigment. The fact that these growths are not mere hypertrophy was pointed out many years since by Müller. Now, these growths are the favourite seats of epithelioma, of the corroding ulcer or lupus, and of cancer, especially of the soft and black varieties—a thing commonly attributed to ‘malignant conversion.’ The only true explanation I believe to be, that the constitutional causes of cancer or of epithelioma are superadded to those of the growth which existed before, which proves alliance between them, but neither identity nor conversion.

6. *Diffusion*; and 7. *Secondary Deposit*. By diffusion is understood a multiplicity of tumours arising from increase of the *materies morbi*; and by secondary deposit is understood the deposit into a fresh place of materials absorbed from the first seat of the disease. Paget noticed secondary deposit in two out of seven cases of epithelioma; and in the College of Surgeons’ Museum, there is apparently a case of secondary deposit from epithelioma of the tongue of an ox. But, as a rule, cancer is the only disease which presents either diffusion or secondary deposit; in the case of the fibrous and other tumours, it is a rare exception; but it is from exceptional cases that the true limits of natural laws are to be defined.

8. *Cancerous Cachexia*. This characteristic is, I believe, much more indebted to imagination than to observation, for the colours in which it has been painted. As regards myself, I can gather no other conclusion than that the yellow anæmic hue, the emaciation, the sickness, loss of appetite, diarrhœa, despondency, and sleeplessness, are in most cases distinctly traceable to two sources: first, the pain, discharge, and irritation, which produce in the cancerous neither more nor less than the amount of cachexia which they cause in any other cases; and, secondly, the interference with the functions of internal organs. I believe that cancer may, and usually does commence, not only without cachexia, but with an apparently good state of health; therefore, that the cancerous material is not poisonous *per se*; that in most cases the cachexia, in other words, the decay of health, is directly proportionate to local mischief; that it is diminished by those measures which remove or palliate local mischief: yet that in some few cases, there is a degree of cachexy apparently disproportionate to the local disease. This, in my search after positive results, is the best conclusion I can come to. Certes, within this year I have seen a man dying of cancer of the stomach, whose life was slowly drained away by inanition; and, when it is considered that the only support for four months was given in the shape of enemata, it does not seem that the cachexy was of a very mortiferous sort. At an early period of my studies, I was struck with observing that cancerous patients were not by any means infallibly marked with visible cachexia; and this in a part of the country where a yellow waxy hue of the features, emaciation, bloodless lips, and other signs of impoverished blood, are so common in the peasant women after forty, that, as I pointed out to a very intelligent surgeon, one-half of the women past maturity whom we met might sit for portraits of the so-called ‘characteristic aspect of malignant cachexia.’

9. *Resistance to Treatment*. Passing over this characteristic, that no medicine exists capable of causing a cancerous growth to wither, it may be remarked that the same is equally true of other growths, except those fibro-plastic growths connected with common inflammation, or with syphilis or scrofula,

some glandular tumours, and a few vascular. But it is a very curious circumstance, the morbid deposits which we have most power over, are precisely those which resemble cancer the most in their cell constitution, and in their infiltrating properties.

10. *Infiltrating Mode of Growth.* On this, as a practical diagnostic sign, too much stress can hardly be laid; as a philosophical ground of classification, none at all. I allude to the infiltrating properties of cancer as contra-distinguished for the circumscribed nature of other growths. So much importance has been attached to this by an eminent pathologist, that it enters into his very definition of cancer. But, on stricter inquiry, it will be found that infiltration, like many other properties, is accidental or occasional; that all cancers do not infiltrate, but may be circumscribed, even to a late period of their existence, which is particularly the case with soft cancer: and secondly, that other growths do infiltrate. The common fibro-plastic or inflammatory exudation infiltrates all tissues; so does tubercle notoriously. And lest it may be alleged that there is a difference between the mode of infiltration in these cases, I may state that I have made this the subject of special examination, and have found that whether muscle, for instance, be infiltrated with fibro-plastic or cancerous matter, the process is identical. The muscle, to the naked eye, looks pale and fatty; the fibrillæ are found to be losing their clear markings, and to be infiltrated with oil near the effusion, whilst in the very seat they are separated and broken up, in the one case by cancer, in the other by fibro-plastic cells and nuclei, and by much unformed matter; by the increase of which the fibrillæ at last disappear.¹ When, therefore, we hear of the infiltrating properties of cancer, of its being *substituted* for other tissues, or of other tissues being converted into it, we may ask how, in kind, this infiltration, substitution, and conversion, differs from that which goes on in the tissues around a spot of common inflammation; from the prolonged action of which, as we well know, all tissues whatever lose their identity, and become converted into a puriform fluid in acute, or gelatinous mass in chronic inflammation.

11. *Softening.* This process has heretofore even been enumerated amongst the defining marks of cancer, and almost as a speciality. It was formerly stated that the difference between cancer and other morbid growths consisted in its being unable to maintain its vitality beyond a certain period; after which it softened and underwent a process of ulceration; the older portions perishing as newer portions were being deposited.

But we find as a matter of fact, that the kind of softening which is most usual in cancer, is that which is met with in all other tissues natural and morbid, and which is commonly known by the term fatty degeneration. A cancer in this condition exhibits to the naked eye spots, streaks, and patches of softish yellow matter; on examining which the cancer cells are found either shrivelled, or filled with oil-globules, or broken down into an amorphous mass of granules.

But this softening from fatty degeneration is common to all other growths without exception. For instance, Mr. Haynes Walton lately sent me a tumour which he had removed from the nose of a young man, quite unadherent to the skin and periosteum, of five years' duration; it was composed of spherical masses of cartilage, some clean, firm, and pellucid; others almost liquid; others soft, yellow, and cheesy; the last were found to be mixed with granular masses, oil-globules, and cholesterin. There is no morbid deposit, or scarce any, in which similar changes may not be found; but there is one, in which it is so early and so constant, that it has been considered a speciality apart. I need hardly say that I refer to the so-called yellow, cheesy tubercle of the lung; which, heretofore regarded as a definite primary morbid product, must now be considered only as a degraded condition of some pre-existing material; and truly we want some new name for the product, of which the cheesy stuff is a degeneration, or else, if the original matter be still called tubercle, we want a name which shall express that cheesy degenerate condition which, as I insist, is no more peculiar to tubercle than it is to cancer.

¹ See a drawing at p. 24 of the Surgeon's Vade Mecum. Sixth edition.

But there is another kind of softening which may affect cancer, viz: the liquefaction of the intercellular structure, which converts what was a solid mass or sponge, with cells in its interstices, into a milky liquid with cells floating independently. Here springs up a whole crop of pathological analogies. The liquefaction of the fibrillary constituent of an ordinary sthenic inflammatory effusion constitutes common pus; that of other deposits constitutes *pseudo-pus*; and it may be said that there are as many kinds of *pseudo-pus*, or puriform fluid, as there are of effusions.

The nature and alliances of these kinds of softening will be illustrated by the case, of which the following is an abstract. For permission to observe the patient and quote the case, I am indebted to my friend Mr. Henry Lee, under whose care the patient was in the Lock Hospital.

CASE.—M. C., a girl aged 21, was received into the Lock for great enlargement of the labia, with warty growths, consequences of repeated gonorrhoea and syphilis. Mr. Lee removed a large mass, by incision, from the right labium; from the wound he hooked out, with the handle of the scalpel, considerable quantities of soft and rotten tissue; on examination of the excised mass, it was found infiltrated with epithelium, and large portions of it completely softened down. The wound healed. Bright's disease of kidneys and dropsy supervened. Death occurred from pneumonia, and sloughing of the groins and labia. On *post-mortem* examination, several lymphatic glands in the posterior mediastinum were in a state of liquefaction, as well as the superficial glands in the right groin, and the sloughing tissues. When I say liquefied, I mean softened into a pseudo-puriform fluid, not in a state of fatty degeneration.¹ This case proves that large masses of tissue may be in a state of complete rottenness from degeneration, and yet cause neither abscess nor ulceration.

12. *Ulceration* is the twelfth alleged characteristic of cancer. But although it is perfectly true, as Lebert says, that hard cancer of the breast and stomach do usually ulcerate, yet this is not true of cancer in general; and when ulceration does occur, it is rather as a mechanical result of distension or of infiltration of the skin, than an essential property of the cancer itself. Tubercular deposits are more prone both to softening and to ulceration. But there are three affections often confounded with cancer, in which ulceration is the special mortiferous element. These are epithelioma; and those deposits in the skin, or mucous membrane, on which there has supervened that most intractable ulceration known as the *corroding ulcer* of Sir C. M. Clarke; the *lupus* of various authors; the *esthiomène* of Huguier. There is the superficial lupus which produces wide-spreading, shallow ulcers on the face, or on the female genitals. There is the exudent lupus or *esthiomène perforant*, which, in the face, usually attacks an old mole, or some new tubercular elevation, filled with cacoplastic material; and on the female genitals usually affects parts that have been infiltrated with similar deposit. In the face, it may destroy everything before it to the base of the skull; in the female genitals, it may perforate the vagina, bladder, rectum, and lay open the peritoneum, or even cause, by contiguous irritation, a fatal peritonitis, if life is not worn out first by pain and suffering; but yet this truly malignant affection has nothing of cancer but its local inveteracy; it does not affect the lymphatics, nor appear in any other part but that first attacked. Again, in cases of epithelial disease, corroding ulceration may supervene, and the lymphatics may be infiltrated; but the disease kills by its effects on the first-attacked spot, and does not kill by secondary deposit or diffusion, as cancer does.

13. *Infiltration of the Lymphatics*. This characteristic of malignancy is confined to cancer, epithelioma, tubercle, and cacoplastic ulcerations, including the syphilitic.

14. *Heterologousness, or Heteromorphosity of Structure*. Of this supposed characteristic of cancer, I shall only say that they are two admirable Greek works, and serve like many other things said respecting cancer, to keep up a

¹ I must take some other opportunity of publishing some remarks on *pseudo-pus*, puriform fluid, &c., which space does not allow me to introduce here.

sort of romantic feeling, injurious to true practical knowledge. That the cancer cell is not quite like anything else may be admitted; but so are some cartilage cells unlike anything normally found in the adult body.

As an accidental accompaniment of cancerous deposit, I am lastly bound to mention inflammation or hyperæmia. As in various processes plastic effusion may be or not accompanied by this condition, so may or may not cancer. The following case may be an illustration:—

CASE. M. B., aged 40; soft cancer of skin of chest supervening on mole; enlarged axillary glands; excision; cure; diminution of glands; severe neuralgic pain; incipient repullulation of cancer *in situ*; relief to neuralgia by cauterizing the newly sprouting warts; fatigue just before menstrual period; sickness, abdominal tenderness and signs of hepatitis; jaundice; sudden enormous swelling of liver; inflammatory symptoms subside; slow death by inanition from cancer of liver.

Having thus gone through the alleged characteristics of malignancy, we find, on summing them up, that cancer is the only malady which possesses them all; that epithelioma, the Greek elephantiasis, lupus, and tuberculosis, come the nearest to it; that the glandular, fibrous, cartilaginous, and fibro-plastic usually display only four or five; yet that whilst there are exceptional cases, which prove the possibility of a cure of cancer, so there are conversely exceptional cases which prove that tumours, usually innocent, may destroy life, in the same way in which cancer does.

Therefore, I argue that the vital characteristics of these growths being variable, and the structure constant, the latter alone can be taken as a basis of classification; and that, since cancer is, as a rule, the only *malignant* disease, using that word in the sense of incurable, it were better, when cancer is spoken of, to call it cancer; because, if the term malignant be applied to any other disease, it occasions confusion and mischief; it may lead to the belief that a disease is cancer, when it is not; and that it is incurable, when perhaps persevering treatment might be successful.

A good instance in illustration falls under my notice even whilst I am revising this paper. Within these few days, I have seen, in consultation with my friend Mr. Hunt, a man, aged 60, with an immense ulcer, which has destroyed the right half of the lower lip, and great part of the right cheek, laying bare the lower jaw-bone. It began thirty years ago, as a little indentation, covered with a thin scab, which became a foul ulcer, but, after a long time, healed. Again it broke out, and healed. On breaking out again, it was cut out by Mr. Bransby Cooper, and was well for a time. It reappeared, however; and the patient, amongst a multiplicity of medical advisers, before he came under Mr. Hunt's care, met with one physician who told him that it did not signify; and with a surgeon, who said it was 'malignant,' and ought not to be touched. There is, however, no sign of cancer about it; the ulcerated lip, at its edge, is as supple and thin as possible, and the only induration is in the periosteum at the edge of the denuded bone. I cannot help thinking that the case might have turned out more fortunately had the case been more perseveringly followed up. But, in truth, the word malignant, applied to a disease, is like the *Græcum est, non potest legi*, of the mediæval ecclesiastics; it is an excuse for non-interference; it appeals to the imagination, invests disease with mystical supernatural qualities. Thus the practitioner, instead of treating his patient, lulls himself to sleep with dreams of heteromorphosity.

But since, under the term malignant, have been grouped diseases palpably different, which nevertheless have all been condemned as hopelessly incurable (thus, even in a work published in 1853, cancer, epithelioma, and corroding ulcer of the female genitals, are all styled malignant, or cancerous and incurable), one practical benefit resulting from the disuse of this term will be greater care in discriminating the various species of disease, and in assigning to each its most appropriate treatment.

Thus, of the corroding lupus, it seems now a well-established fact, that efficient excision or cauterization may be followed by absolute cure. In the case of the late Pope Gregory XVI., an ulcer of this kind was eradicated from the face by the arsenical paste, and there was no return during the eight remaining

years of his life. In the corroding ulcer of the female genitals, too, thorough excision or cauterization have effected permanent cures.

In the case of the epithelioma, and of warty growths resembling it, which are liable to become the seats of this affection, there can be no doubt of the propriety of excision as early and as freely as possible.

In the case of cancer, the general practice of the profession seems, so far as regards extirpation, as sound in principle as the present state of knowledge permits it to be. Regarding the treatment of cases not capable of extirpation, I have reason to know that the vague term malignant, and the idea of death through cachexia, operate most unfavourably by inducing the surgeon to neglect palliation or treatment; but if the opinion were acted on, which I believe to be the just one, that cancer, as a rule, destroys life as other diseases do, by its local effects, then the surgeon would feel that every particle of skin he can save from excoriation, every portion of the foul and irritable ulcer that he can cleanse or soothe, will be so much taken from one palpable cause of death. I know of one case in particular, in which the more than commonly assiduous use of common means, especially blackwash and bismuth, was of immense service in prolonging life.

As to general treatment, much may be hoped from the persevering use of means to neutralize pain and exhaustion. The neuralgic pains, in the case last related, were immensely soothed by ether and warm negus, more than by opium, though this last, with chloroform, is unhappily the sheet-anchor.

At present, all treatment of cancer is palliative, and directed against local manifestations and their results. But, when we consider that, ten years ago, before Hughes Bennett taught us the use of cod-liver oil, consumption was almost regarded in the same light, we surely need not yet despair of finding some substance which may destroy and eliminate what is noxious, or supply what is deficient, and so bring this intractable disease within the proper sphere of therapeutics. That there is a potentiality of cure, is shown by the degeneration of actual cancers; that real cures may have happened, cannot be denied; but, before any fact can be held as certain, or can be estimated statistically, a reform of nomenclature and classification, and the disuse of that most noxious word 'malignant,' are essential."

26. *Arsenical Injections in Cancer of the Uterus.*—Mr. LLOYD has for a long time employed, in cases of cancer of the vagina or uterus, which have come under his care in St. Bartholomew's, injections of arsenical solutions for the purpose of preventing fetor. He states that such treatment is perfectly innocuous, and that it succeeds far better than the ordinary disinfectants, which at the best are but of very temporary benefit, the smell generally returning as bad as ever quickly after their employment. The strength of the solution used is from two to eight grains of white arsenic to the pint of water. The great difference between the chemical disinfectants, and such applications as this, no doubt arises from the circumstance that the one only destroys fetor, while the other prevents it. The extraordinary influence of arsenical applications in cleaning cancerous ulcers is well known to those who have tried them. It is not necessary that they should be made to act as escharotics, since repeated dressings with a paste only strong enough to stimulate will effectually serve the end. The following prescription is that of the powder used by Mr. Startin at the hospital for skin diseases for the purpose mentioned, and it differs, we believe, not very materially from one which was a great favourite with Dupuytren: *R. Hydrarg. chloridi ʒiiss.; hydrarg. bisulphureti ʒij.; acidi arseniosi ʒj. Misce.*

This powder, made into a paste with water, is more or less lightly, as required, to be brushed over the sore, the application being repeated once a week, or fortnight, or month, as may be necessary. Mr. Lloyd's injection, in all probability, acts in the same manner on the sloughy, cancerous ulcerations concealed within the vagina. It must be borne in mind, some real good is probably produced in addition to the prevention of fetor, since the diminution of discharge and sloughing will save the patient's powers, and perhaps, also, somewhat retard the growth of the cancer.—*Med. Times and Gaz.* July 1, 1854.

27. *Amputation at the Knee-Joint.*—WILLIAM FERGUSSON, Esq., in a clinical lecture lately delivered at King's College Hospital, related the following case of violent inflammation of the leg followed by necrosis, in which he amputated at the knee-joint, and remarks on that operation:—

“W. M., aged 11, is a native of Sydenham, and states that he has always had remarkably good health up to the time of his present illness, which began six weeks ago. After having been out sliding the whole of one day, he came home in the evening complaining of pain in both legs, more especially in the right knee, upon which he had fallen in the course of the day. In a few days after this he was seized with shivering and violent deep-seated pain in the right leg and ankle-joint, which was followed by considerable swelling of the limb, commencing at the ankle and extending up to the knee-joint. The integuments appeared red, as if very erysipelatous. His sufferings now became excruciating, more particularly if pressure were made on the limb, or if he attempted to move it. Notwithstanding the active measures employed by his own surgeon, the inflammation continued to increase, and matter formed, which soon became discernible in the soft parts. An incision was consequently made on the outer part of the ankle-joint, and about a pint of pus evacuated. A few days after this, another puncture was made in the upper part of the leg, and more matter was discharged. During this time, his general health had become much impaired, and he became extremely emaciated.

“When admitted into the hospital, January 25, 1854, Mr. Fergusson made an accurate examination while the boy was under the influence of chloroform, and found the knee-joint much diseased, the surfaces of the bones being rough and denuded, and a considerable collection of matter in the upper part of the leg, which was evacuated.

“The patient was supported by stimulants for a few days, till his health was deemed sufficiently good to stand the shock of an operation.

“When placed on the operation-table under chloroform, a small opening was made a little above the knee, and a quantity of unhealthy pus was evacuated. Mr. Fergusson then performed the operation of amputation at the knee-joint, in the way detailed in his own work.

“The state of the bones of the leg clearly demonstrated the necessity of their removal. A section being made of the tibia, the cancellous tissue of the upper part of that bone was found filled with pus, while that tissue at the lower part was necrosed, and the epiphysis separated. The articular cartilages of the ankle-joint had ulcerated, and the ends of the bones were eroded. The articular cartilage on the head of the tibia was so soft that a probe passed readily through it, and the bone was bare and carious in several spots, especially around the articulation with the fibula.

“The patient rapidly improved, and was discharged cured, March 11, 1854.”

Now, if I be not mistaken, the late Mr. Liston performed a similar operation at University College Hospital; but I am uncertain as to its performance by that gentleman, or to the date, if it were performed. When I first performed the operation, to my knowledge it had not been previously done in England, for I cannot find any record of the fact, and such an operation performed for the first time would not likely be passed over without some notice being publicly made.

This operation has since been followed by others, among the first of whom I may name Dr. George Williamson, now in India, one of my former assistants. The first time I performed this operation was on the person of a full-grown man (the case is mentioned in the third edition of my work on *Practical Surgery*); and I have rarely seen or made a better stump. He has repeatedly walked forty miles a day, and once walked one hundred and twenty miles in three days; and, what is more astonishing, his false leg was but indifferently made and padded, the spoke of an old wheel being considered by the man an excellent substitute for a more expensive contrivance.

In consequence of such great advantages arising from my first trial of the operation, I have since frequently performed it. Notwithstanding, several objections have been made. Mr. Syme, who had performed the operation in Scotland before I attempted it here, had taken a dislike to the proceeding from something that went wrong in his own cases. Mr. Syme imagines that greater danger

is incurred by a larger surface of bone being exposed, by the removal only of the condyles, than if the bone be sawn higher up in the shaft. But mischief, I think, is more apt to occur when the bone is sawn in the shaft. Where the bone is vascular, I think there is little chance of necrosis, and much less of caries; and you have frequently seen how kindly the two cut surfaces of the spongy portions of bone heal in cases of excision of the elbow joint.

Another objection made is that the length of the stump is very awkward. This I do not admit; if the stump be short, an apparatus cannot conveniently be fitted, and the bone, when cut too high, is liable to be tilted forward by the psoas and iliacus muscles. Indeed, I cannot perceive any objection to a long stump. Objections have been made to a long stump of the leg, and amputation of the leg is often recommended to be done a short distance below the knee; but I am doubtful of the utility of such a step as a general rule.

Again, a long stump in the thigh can never hinder in any way; besides, the leverage is much greater than it if were only half the length. In addition, the great breadth of bone, when well covered, is better able to support the weight required to be borne.

This objection might be raised by some, that this operation is not truly an amputation at the joint, as the condyles were taken away. If such be allowed, then one might say that Mr. Syme's operation at the ankle is not an amputation at the joint, for he always removes the malleoli; but such an objection could not be held reasonable, and the operation now under consideration I deem a great addition to the history of amputation, and have taught it as such for the last ten years.

I now perceive that, for nearly the first time, mention is made of this operation in the surgical Journals of the day, which, I make no doubt, will greatly tend to the advantage of this department of surgery.

When I first commenced my profession, it was an understood rule, with but few exceptions, that the coverings of the bone in an amputation should be taken from the sound parts of the region when the operation was required; as, for instance, in amputation of the thigh, the soft parts are always taken from the substance of the thigh; so also in amputations of the leg. But in this case, and in amputation at the knee, the soft parts covering the end of the femur are actually the tissues that originally constituted the calf of the leg.

In the history of amputation, it has always been the aim of the surgeon to make a good stump, its quality depending greatly upon the proper covering of the bone. If the soft parts be scanty, a bad stump must result; if, on the other hand, the covering be too large, the result will likewise be unsatisfactory. A remarkable instance of this latter kind was under notice last summer in this hospital. But there is more danger of the covering being scanty than profuse. The fleshy condition of the covering, as you know, is ultimately converted more or less into a fibrous texture.

Though amputation cannot be said to be the opprobrium of surgery, an axiom I laid down in my first paper on Conservative Surgery—"For the greater proportion of sound material that we can save in any operation on the body, the nearer we come to the perfection of good surgery"—yet I think amputation at the knee-joint may fairly have at least a footing in the province of conservative surgery.

This operation has lain for some time in abeyance; but I now find my name associated with it in the journals, in papers which have recently appeared on the subject, from the abler pens of my friends, Mr. Greenhow, of Newcastle, and Mr. Jones, of Jersey, men who, with myself, I would fain hope, have no desire to have their names connected with novelties, unless it be for the good of our fellow-creatures and the advancement of surgery.

In cases of injury of the joint, including great contusion of soft parts, I am doubtful whether the operation should not be effected above the seat of injury. As to the mode of performing the operation, I first make a small anterior flap, drawing the knife across the front of the joint, and then, inserting the point of the blade behind the femur, thrust it through to the other side, close to the condyles; then, carrying it downwards, cut the posterior flap from the

calf of the leg. The saw is then applied a little above the condyles, and the flaps brought together as in an ordinary amputation.

My friend Mr. Greenhow, of Newcastle, saws through the bone before making the posterior flap; but I prefer the method I have described, although the great aim is to obtain sufficient material to cover the bone.

In some instances, I first effect the separation of the leg at the articular ends, and thereafter cut away as much of the femur as seems needful. In all cases, it is requisite to take the full length of the calf for the posterior flap, as the soft parts in the back of the thigh contract very much in the course of time. The patella might be saved in some examples, but in general I think it would be best to remove it.

I have been thus particular in my remarks, as you will not find any particular mention of the operation in any English work prior to my own.—*Med. Times and Gaz.* July 8, 1854.

28. *Advantages of operating in certain cases of Harelip at a very early period.*—HENRY SMITH, Esq. advocates (*Med. Times and Gaz.* March 25, 1854) operating at the earliest period in certain cases of harelip. "It is obvious," he says, "in the first place, that an infant with harelip cannot so readily take in that nourishment which is offered by nature. If, however, the deformity be remedied, the child will be placed by the aid of surgery in a much more favourable condition to receive the nutriment afforded by the mother's breast. It is plain, too, that the deformity excites most unpleasing and painful sensations in the mind of the mother and those around her; and, if the source of this anxiety can be removed at once, it is of great importance that it should be accomplished."

A third argument in favour of very early operation for harelip consists in the circumstance, that as the growth of the child is very rapid in the first period of life, the lip, with other structures of the body at this time, becomes more fully and fully developed, and thus, after an operation has been successfully performed, there will be a much less chance of subsequent deformity in the part.

But it is in those instances where the harelip is complicated with a more or less extensive fissure in the palate that an early operation for the cure of the former is so imperatively demanded, and is attended with such beneficial results; and it is to this point especially I wish to draw attention, because, although in some recent works of surgery an operation at an early period after birth is recommended (and I may especially allude to the *Practical Surgery* of Professor Fergusson, and to the *Surgeon's Vade Mecum*, by Dr. Druitt), the most important reason for such a proceeding is not alluded to. And I now refer to the effect which is produced upon the fissure in the hard palate by the approximation of the edges of the lip.

As long as the harelip remains in its primitive state, there can be no pressure upon the hard tissues underneath; but, if it be united by the surgeon, a considerable amount of pressure is exerted upon the edges of the cleft in the palate; and, in a child aged only a few days or weeks, the bones are so soft and compressible, that they are to a great extent influenced by the pressure which constantly obtains, and in the course of time the fissure becomes either entirely closed or diminished in size to one-third or one-fourth of its original extent.

I have had various opportunities of noticing this effect in instances where a very early operation has been performed for harelip, complicated with more or less extensive fissure in the hard palate; and so convinced am I of the importance of performing the operation as soon after birth as possible, that I invariably recommend it. And it has fallen to my lot to be called upon to perform the operation very soon after birth, where there has been, at the same time with the harelip, considerable malformation of the palate; and I have been able to notice the results some length of time afterwards.

More than three years ago I operated upon an infant only four days old; here there was an extensive fissure extending through the hard palate into the nos-

tril. I had an opportunity of seeing this child only a few days since, and the opening in the front portion of the palate was closed up. In this case the soft palate was extensively cleft, and that still remains open; but the parts altogether are in such a condition that, some years hence, they may be completely closed by staphyloraphy.

A few weeks since, a little patient was brought to me, on whom I operated at a very early age, two years ago. In this instance there was a fissure in the hard palate, and great deformity of the jaw, a portion of which I removed at the time. There is now an admirably developed upper lip, and complete closure of the opening which existed in the palate.

In another instance, where I operated at an early period, there was an immense chasm running through both soft and hard palate into the nostril. I had an opportunity of seeing this patient a few days since, and found that the anterior portion of the cleft was much diminished in size. The operation was done more than a year ago.

Mr. Bateman, of Islington, who pays great attention to this matter, operated, three years since, upon an infant only four hours after birth. In this case there was an extensive fissure in the palate. This gentleman kindly showed me this case, and, in reply to my inquiry regarding the effect which the operation had had upon the palate, he wrote word the other day that the child had died of hooping-cough last winter, but that its mother remarked that before death the fissure, which had at birth been 'so large that she could put her thumb into it, had contracted so much that it would scarcely admit the edge of a sheet of writing-paper.'

About a month since I operated upon an infant only six days old, with perfect success. In this case I was partly induced to perform the operation at this early period because there was a fissure in the hard palate, extending into the nostril. I have little doubt that, in time, if the child lives, the fissure will be completely closed."

29. *Paracentesis Thoracis*.—Dr. J. R. BENNETT read a paper on this subject before the Medical Society of London (May 27, 1854), the chief object of which was to inculcate the importance, in cases of inflammatory hydrothorax, of not hastily resorting to the operation of paracentesis. He founded his objection to this proceeding in the early stage of the disease, both on the non-necessity of the measure, on the amenability of the disease to general treatment, and on the mischief which was likely to arise from puncturing the cavity of the chest. He showed the non-necessity of the operation, by the relation of cases in which there had been a large collection of serum in the chest, but which had been absorbed by general treatment and the use of counter-irritants, consisting either of blisters, or of the application of a very strong tincture of iodine. The medicines administered consisted of very small doses of blue pill, with squill and Dover's powders, an infusion of cascarrilla, with iodide of potassium, and sweet spirits of nitre. The patients were placed under non-stimulant but nutritious diet. He objected to the use of mercury—to the production of the specific effect of that medicine, which he regarded as injurious. He related a case to which he had been called in the country, of hydrothorax, in a young gentleman, in whom the symptoms were not of such an urgent character as to require operation. He recommended the employment of remedies similar to those which have been mentioned, and with every prospect of their being useful. Another physician was called in, however, before a fair trial was given to the measures proposed, and paracentesis was performed. The fluid was serum; but on a second operation being required, about three weeks afterwards, the matter evacuated was purulent. This illustrated one of the dangers to which tapping exposed a patient suffering from hydrothorax. With respect to the diagnosis of the nature of the fluid in the chest, this could be determined without danger by the passage of an exploratory needle. If the fluid were found to be purulent or contained albuminous flakes, he recommended a gradual and continued drain of the fluid rather than its sudden removal.—*Lancet*, June 10, 1854.

30. *Excision of Ovarian Tumours.*—Prof. JOHN ERICHSEN, in a paper on this subject read before the North London Medical Society, makes the following remarks:—

“In considering the subject of ovariectomy, two questions present themselves: 1. As to whether the operation is a sound one and ought to be retained in practice; and 2. If retained, in what class of cases it should be had recourse to?

“In answering the first question, we must refer to the course that is taken by ovarian tumours, and to the result of the operation as deduced from published cases.

“The course taken by ovarian tumours necessarily varies greatly. In some instances, these growths do not appear to be incompatible with prolonged existence and a fair share of health. In other instances, again, after remaining quiescent for some length of time, they assume considerable activity of development, and interfere so seriously with the other abdominal organs and with the general operations of the economy, that life is attended by great misery and discomfort; and in a third class of cases they rapidly and steadily run their course to an unfavourable termination.

“After a time, in the majority of instances, the tumour may be materially lessened in bulk by tapping; but when once this operation has been had recourse to, it will require to be repeated with increased frequency, the intervals between each succeeding tapping being diminished; and then a fatal termination at a comparatively early date may usually be looked for. Mr. S. Lee states that of forty-six patients who were tapped, thirty-seven died, and only nine recovered; and that of the thirty-seven who died, more than one-half did so in four months from the first tapping, twenty-seven out of the thirty-seven in the first twelve months; and of these, eighteen were only tapped once.

“The objection has been raised against ovariectomy, that the mortality from it was so high as not to justify a surgeon in performing it. Undoubtedly, a very high rate of mortality after an operation would constitute a serious bar to its performance; and the more so, if it could be shown that the disease for which it was practised was not a necessarily fatal or even a very serious one. Thus, Mr. Phillips has collected the particulars of 61 cases in which ovarian tumours were extracted: of these, 35 were successful, and 26 died. Mr. Lee gives 90 cases; of these, 57 recovered, and 33 died: and Dr. Robert Lee, the most recent writer on the subject, has collected 102 cases of ovarian extraction, of which 60 did well, and 42 terminated fatally. From these statistics, it would appear that the mortality after the removal of ovarian tumours amounted to rather more than one in every three cases. This rate appears to me not to be excessive, when we compare it with the result of operations for various severe surgical injuries or diseases.

“The next question that occurs in connection with ovariectomy is as to the class of cases in which it should be practised. Now here we come upon ground in which much difference of opinion must necessarily exist. Ought ovariectomy to be performed in all cases, or even in the majority of instances of ovarian disease; or ought it to be limited to a few and exceptional cases, and practised as a last resource?

“The most ardent advocate for this operation will scarcely, I think, advise that an attempt should be made to extract the ovarium from every woman labouring under tumour of this structure; but would rather recommend the employment of palliative treatment, until the growth had begun to interfere seriously with the comfort of existence, or with the healthy action of the abdominal organs. When these injurious effects of pressure have once fairly begun to manifest themselves, the patient wasting, suffering much discomfort from her size, with difficulty in breathing, repeated vomiting, gastric irritation, &c., then the question of relief by operation will necessarily obtrude itself, and must be discussed dispassionately and without prejudice. Under these circumstances, I confess I see no chance of giving the patient relief, or of prolonging her existence, except by the removal of the tumour. Medical treatment is of no avail in such cases, and tapping only gives temporary relief, and will eventually, and often very speedily, wear out or destroy the patient. Under such circum-

stances, therefore, we must either leave the patient to her fate, or have recourse to the ablation of the tumour, which, it has already been shown, may be done with the prospect of success in nearly two cases out of every three.

"So far then as the strictly *surgical* aspect of this question is concerned, I should say that as the recoveries after this operation amount to at least 60 per cent., it is proper for the surgeon to propose and to perform it when all other means of relief have failed, and when the patient's health is giving way under the extension of the disease.

"There is one aspect under which we have not as yet considered this matter, and which has a most important bearing on the operation. I mean in its *diagnostic* point of view. This, which may with justice be termed the *medical* part of the question, and may be considered as belonging rather to the province of the physician than to that of the surgeon, has to be considered in two of its bearings: 1. As to the existence of such adhesions between the enlarged ovarium and the other abdominal organs as to render the extraction impossible; and 2. The diagnosis between ovarian and other abdominal tumours.

"The existence of such adhesions between the ovarian tumour and the abdominal viscera, as will prevent the possibility of its extraction, is of frequent occurrence, having been met with in rather more than one-third of the whole number of cases operated on; according to Dr. Lee, in 60 out of 162 cases. The frequency of the occurrence of this complication renders its diagnosis one of extreme importance. Its existence may in general be *suspected*, when it is ascertained that the patient has been the subject of attacks of peritonitis, when the abdominal tumour does not appear to change its position on the patient taking a deep inspiration, and then expiring freely; and when, on the patient's raising herself into the sitting posture, the sac does not tend to move forwards into the space between the recti muscles. So also much light may be thrown upon this important point by ascertaining the existence of a crepitant or crackling sensation between the anterior abdominal wall and the tumour; and more particularly, if when the tumour is tapped, it draws down over it the abdominal parietes, or sinks into the pelvis without exercising any traction on those parts. The condition also of the pelvic viscera, viz. the bladder and uterus, as ascertained by examination with the sound, may tend to show whether connections exist in this quarter or not.

"By such examinations as these, the presence of adhesions may doubtless in many cases be suspected; but in other instances, and more particularly when the mass is bound down posteriorly, there is no possibility of determining this point, and then the surgeon may, to his great annoyance, find that after laying open the abdomen, the operation cannot be completed, and the tumour requires to be left. In those cases in which there is any reason to suspect the presence of adhesions, it is, I think, a wise precaution to make a small exploratory incision into the abdomen, through which the fingers or hand may be introduced, and the connections of the tumour examined. If these are too extensive to admit of removal, the aperture may be closed, and possibly the patient may escape without any very serious consequences ensuing; as in these cases the peritoneum has in a great measure lost its character as a serous membrane, and is not so susceptible of the diffuse and destructive forms of inflammation that would otherwise be likely to occur in it.

"The diagnosis of ovarian from other abdominal tumours, falls so entirely in the province of the physician, that I do not feel myself prepared to enter upon it; but would prefer leaving that point to be elucidated by those members of the Society who have paid special attention to this subject."—*Assoc. Med. Journ.* Jan. 13, 1854.

31. *Ununited Fractures*.—A lecture has recently been delivered at St. Bartholomew's Hospital, by EDWARD STANLEY, Esq., on the very practical subject of ununited fractures, and the various modes recommended and adopted to remedy this troublesome surgical disease. In an experience of sixteen years in the largest hospital in London, Mr. Stanley first remarked, either among his own cases of fracture, or in hundreds of other fractures under Mr. Lawrence, he did not recollect a single case of ordinary fracture treated in hospital which did not

turn out favourably. The cases of "ununited fractures," which he wished to speak about, had come into hospital *as such*, and in too many instances seemed all but incurable. Mr. Stanley seemed of opinion, from talking the matter over with other eminent surgeons—Brodie, Fergusson, Skey, &c., that a great deal could be done in such cases, but we should first understand the nature of the accident we have to deal with; what was useful in one case of false joint, or badly-united fracture, proving very little use very often in others. The first element in the calculation of all such accidents Mr. Stanley alluded to is that of *time*. In an ununited fracture of the femur, for instance, of a few weeks or months, we should still use steady compression on the parts by a leather splint, combined with the most absolute rest. We may find, in fact, several conditions of the party—for instance, an entire failure of the reparative process on the part of Nature, or what Mr. Stanley thinks, the real state of things: a piece of muscle gets torn and thrust between the ends of the bones, and prevents the callus thrown out provisionally from acting as it should in joining the parts thus separated. This, he is inclined to think, occurs oftener than is suspected.

2. We may again find, at a longer interval of time, after years of annoyance to the patient, the ends of the bones "eburnated" or polished, hard in texture—a rude capsule formed round a kind of joint, even new cartilage and a synovial-like fluid (Mr. Stanley exhibited several preparations of this kind from the museum of Bartholomew's—one very perfect; the humerus of a woman broken from slight muscular contraction of the deltoid, a subsidiary olecranch and elbow formed with capsular investments, formed at the insertion of the deltoid)!

3. Here is a class of cases (and they are those commonly found in hospital) where the process of union continues to all but the last or the stage of ossific deposit: you have a fibro-cartilage or cartilaginous formation, but no earthy matter. When we grasp the limb, and expect to find it firm, we find it to yield like a hinge, and a point on which Mr. Stanley lays great stress as diagnostic of false joint—there is always great PAIN. This is, in fact, the best test that union has not been completed. An elderly woman, now in Bartholomew's, shows this very well, as the least handling of the limb is agony to her. A popular idea would lead us to the belief that ununited fracture arises from debility or a cachectic habit of body; but, "in my practice," said Mr. Stanley, "I cannot say I have observed this; a man lately in hospital is the picture of rude health; and in fifty-six cases collected by Mr. Anneslie, fifty-two were strong, stout subjects, many occurring just at the middle period of life, when persons are most mixed up with the bustle of active life, and most subject to fractures—of course, exceptions may occur."

From the result of cases of his own, as well as of those under the hands of other surgeons of much experience, Mr. Stanley believes some "*local*" cause has more to do with the disease than anything of a constitutional or general kind; if the first period of irritation and of provisional callus forming after fracture passes by without a perfect joining of the two ends of a bone, the latter are likely to remain ununited until a new irritation is again set up in the parts. In a case now in Bartholomew's, which Mr. Stanley has been obliged to amputate, this local cause has been a large piece of muscle (the crureus) jammed in between the broken ends of the bone. The treatment of ununited fracture is one on which both Mr. Lawrence and Mr. Stanley can speak with no little authority, as in private practice as well as in hospital, they have seen everything tried, and almost everything fail or succeed. Mr. Stanley mentioned more than twenty methods of treating ununited fracture, but referred them all to the following heads: Firstly. As most useful, perhaps *compression* and *absolute rest*, effected by different arrangements of stiff leather splints, felt, gutta percha, &c. After sixteen years experience of this plan at Bartholomew's, he thinks such means scarcely ever fail. The chief difficulty is to procure perfect immobility of the ends of the bones, prevented by the movement of the joints next the fracture. This must be particularly remembered, or all our ingenuity is useless. Secondly. *Rubbing the ends* of the bones together. This will be found a most useful adjunct, but is chiefly to be relied on in recent cases before you have a false joint and synovial cavity with cartilage. It may be done now more carefully, too, with the aid of chloroform, and should always perhaps pre-

cede more active modes of treatment. Thirdly. Cutting down and applying *caustic*. This plan has been tried, but Mr. Stanley does not at all recommend it. You have exfoliation, which may be troublesome, with very severe inflammation, &c. Fourthly. *Saving the ends* of the bone. This is generally called for only in very old cases, where there is a false capsule, and a rude kind of false joint to contend against. This, too, can be now well performed under chloroform. Fifthly. *Setons*. Some cases treated in this way, under Mr. Paget, have done very well. A sixth mode, that by *ivory pegs*, he has lately tried; but it must be confessed with indifferent success. The author of this plan was Dieffenbach, of Berlin; he noticed in some gunshot wounds, that where bullets and other foreign bodies had lodged in bones, a large quantity of healthy osseous matter was thrown out, as if nature tried to bury them, and prevent irritation of parts in juxtaposition. Mr. Stanley exhibited a curious specimen of this kind, from his private collection. A soldier, wounded at Waterloo, received a ball which went in at the back and stuck in the posterior part of the sternum. The man lived a long time, for the bullet is half buried in *new* osseous structure, thrown out around it. Dieffenbach at once tried the plan; and with a common gimlet, a few pegs, and proper splints and bandages, succeeded quite wonderfully. Two other modes, drilling holes through the ends of the bones, and then keeping them in contact by *wires*, and also another, that by *blisters*, Mr. Stanley seemed to think had not much in their favour, the wires keeping up too much irritation, and the blisters acting perhaps, like ordinary setons—the latter a plan very deserving of trial before recourse is had to other means.

Mr. Stanley concludes his lecture, by citing two cases in his practice by the ivory pegs: one very successful; the other now in St. Bartholomew's, not so encouraging, but where it would appear that the process of reparation was prevented by a cushion of the crureus muscle getting between the ends of the bone, as shown by the leg after amputation, performed last month (June, 1854), a large quantity of bony matter thrown out round the pegs.

CASE I. Among his notes, Mr. Stanley finds the following case: About four years ago a man was brought into St. Bartholomew's (Bentley's ward). He had been working on the railway, and sustained a very severe injury, fracture of a compound character of both tibia and fibula, if we say he was lying on his back eight months; and at the end of this period, though the external wound was healed up, yet the union of the bones was not complete; it is quite sufficient to indicate his condition and the very tedious nature of the case. In a word, it seemed one of those cases which to Pathological Society men, would seem doomed never to get well. The man at length got about on crutches, but it was with most dreadful pain and torture of the limb, so that he at length gave up, wishing his leg to be cut off. It was decided, however, in consultation, to do something for the cure of ununited fracture. The first plan tried was a succession of blisters for a month, with no equivalent benefit; the limb was then encased in strong leather, stiff, and about as immovable as the limb of an Egyptian mummy: but this plan did not prove either very successful. After ten or twelve weeks, the question came up really of amputation; but this time with the rather fanciful alternative of ivory pegs, as suggested by Dieffenbach. The ivory pegs succeeded perfectly; and Mr. Stanley has the pleasure of seeing the man constantly brought to new life and activity, by four of these rather incongruous remedies borrowed from a cribbage-board! Four were inserted, one in each bone, at each end of the fracture: small holes drilled with a common drill, respectively, in tibia and fibula, then the pegs tapped well in with something like an auctioneer's hammer; and here Mr. Stanley said, perhaps, the most curious part of the matter is worth observing, in a physiological point of view, observed also in a similar case at Leeds; one peg was entirely absorbed, the other appeared eaten away, like carious pieces of human bone! When withdrawn, the small head of the ivory peg left out of the bone through the integuments was not changed at all. The successful issue of the case (after a whole year's unsatisfying ordeal in hospital) was most grateful, and the man left town quite well. In the second case, marked by all Mr. Stanley's kind attention and care, the result has not been so satisfactory; it has been a case brought into hospital, of long standing, of ununited fracture, high up in the shaft of the femur, both

ends freely movable and grating together. He first tried pad and splints, next inclosed the limb in stiff leather case twenty months. After injury, the question of amputation, or ivory pegs, as in the former case, was again considered: the pegs were tried, but the quantity of bone thrown out was of no use, and amputation, as a last resource, was performed.—*Dublin Med. Press*, August 2, 1854.

OPHTHALMOLOGY.

32. *Facial Anæsthesia, with Simultaneous Destruction of the Eye.*—The following extremely interesting case of this character is recorded (*Med. Times and Gaz.* 15th July, 1854) by Dr. R. TAYLOR, Surgeon to the Central London Ophthalmic Hospital.

Eliza Martin, aged 46, became an out-patient of the Central London Ophthalmic Hospital, March 17, 1853.

Six months previously, after exposure to cold and wet, she had been confined to bed for a fortnight with a violent cold, and pains all over the body. On awaking one morning, she felt her left eye painful; on putting her hand to the part, she found that the feeling of the whole of that side of the face was gone, and on examining further, she discovered that the features were drawn to the same side, the eye blood-shot, and the vision impaired. The pain soon became very severe, radiating all over the side of the head and face, and in a few days the sight of the eye was completely gone. She was then received into St. Bartholomew's Hospital, where she overheard her case, so far as the eye was concerned, described as one of abscess of the eyeball. She remained in the hospital for six weeks, during which the pain completely left her, and the eye, which had been very prominent, returned nearly to its proper position in the orbit; the sight, however, did not return, and the eye looked, she said, as if there was something white in it. In the beginning of March, the eye again began to protrude, but painlessly, and in a few days it burst, discharging a quantity of matter.

I found the left eyeball enlarged, filling up the orbit to its margin, and protruding considerably; the eyelids were swollen and livid in colour, and the conjunctiva was chemosed, pale, and flabby; the lower half of the cornea was gone; the upper, with a sharp and clean-cut edge, overlapped what appeared to be the remains of the iris; the opening into the eye was blocked up by a soft scab, on detaching which, and pressing gently, pus and discoloured vitreous humour flowed out; a probe was introduced through the opening, and moved freely about without being felt in the slightest degree; on examining microscopically what adhered to it, it was found to consist of pus and broken fibres of the lens.

The whole of the left side of the face supplied by the fifth nerve was insensible to such a degree, that she could merely tell when she was roughly touched, but felt no pain when pinched or pricked with the point of a pin; even this amount of sensibility she represented as being of recent occurrence; and on the forehead she had no feeling whatever. The anæsthesia affected equally the nostril, inside of the cheek, gums, roof of the mouth, and tongue of the left side. The skin of the upper lip, at the entrance of the nostril, was raw and excoriated, apparently by the thin mucous secretion which trickled over it.

The features were drawn to the left side decidedly, but not to a great extent; she said that a considerable improvement had taken place in this respect. She could not close the right eye; when she attempted to do so, the eye rolled upwards and inwards, so as to conceal the cornea, while rather more than a quarter of an inch of the sclerotica remained exposed. Neither could she close the lips, from the left corner of which, owing to its being rather more depressed than the other, there was a constant dribbling of saliva. The left temporal and masseter muscles remained perfectly passive during mastication; on the right side they acted in the usual manner. She was unable to use the left side of the

mouth in eating. The food accumulated between the teeth and the cheek, and remained there till it was pressed out purposely; and as, from the absence of sensibility of the parts, she was unconscious of its presence, it was sometimes allowed to remain until it became offensive. There was no accumulation of food in the right cheek in eating. The left side of the tongue was much atrophied, being not more than half the size of the right; it was protruded towards the left corner of the mouth. Neither the uvula nor the soft palate appeared to be implicated in any way.

As the patient unexpectedly discontinued her attendance at the hospital, the opportunity was lost of examining minutely into the state of the senses of taste and smell; the former, she said, was completely gone on the left side, except towards the back of the mouth, and as she was in no way prompted, this answer may be considered as satisfactory to a certain extent; the sense of smell, though blunted on the left side, was never altogether lost. The hearing of the left ear was not at all impaired, and this was the more evident, as she had been completely deaf on the right side for several years. Her speech was very indistinct, apparently from her inability to articulate the labial sounds, owing to the paralyzed state of the lips.

I had another opportunity of seeing her very recently. The eyeball had shrunk into a small stump, marked by the action of the muscles; she had severe neuralgic pain in it some time ago, which lasted for about ten days; but, with this exception, it has been free from uneasiness. The sensibility of the face is slowly returning in parts; on the eyelids it is perfect; on the lower part of the cheek it is little inferior to that of the right side; on the forehead and side of the nose it is not at all improved. The skin of the upper lip is still excoriated and slightly ulcerated; the sensibility of the nostril is much improved, and the sense of smell is nearly as acute as on the other side. There has been no change in the state of the mouth, except that the gums are somewhat swollen and unhealthy, and the left side of the tongue has, if anything, slightly increased in size. The sense of taste was more carefully inquired into, and her former statement found to be correct. The features are now quite straight, but the lips and the right eyelids remain paralyzed as formerly; when she attempts to close the former, the muscles of the chin, on the left side, are seen quivering under the skin. When she uses the jaws in mastication, the temporal and masseter muscles of the left side are now felt to swell under the finger, but less firmly than those of the right side, and not quite synchronously with them; the muscles of the right side act first, and then, after an appreciable interval, those of the left.

The suddenness with which the paralysis occurred, and the fact that the nerves of both sides were affected simultaneously, render it probable that the cause was an effusion of blood at the base of the brain. The situation of the lesion, whatever may have been its nature, is accurately pointed out by the symptoms which it produced, which indicate that both roots of the fifth, and the hypoglossal nerve of the left, and the seventh nerve of the right side, were the parts implicated. The complete paralysis of the orbicularis oris might at first sight lead to the idea that the seventh nerve of the left side was also involved; but, as this was the only muscle under its control whose power was at all impaired, some simpler explanation will probably occur to those who are conversant with nervous pathology.

Of the various points of physiological and pathological interest which this case presents, the most remarkable is the occurrence of the destructive inflammation of the eyeball simultaneously with the first appearance of the facial anæsthesia. In the well-known experiments of Magendie, which have since been carefully repeated by Valentin, complete division of the fifth nerve within the cranium, in rabbits, was followed by inflammation of the eye within twenty-four hours; but, in the human subject, where the anæsthesia is the effect of disease, the interval is much longer; in some instances, even when the paralysis of the nerve is complete, the eye remains totally unaffected; in others, the immunity lasts for many months, and it is very rarely that the interval is less than several weeks. These remarkable differences, which are as yet wholly unexplained, have led several eminent physiologists to maintain that the disease

of the nerve exerts no direct influence in producing the inflammation of the eye, but that the organ suffers secondarily from the irritation of dust or other foreign particles, the presence of which is unfelt, from the loss of common sensibility; or which, as Sir Charles Bell suggested, may remain unremoved, owing to concurrent paralysis of the eyelids. The fallacy of the latter suggestion is strikingly shown by the case under consideration, in which the fifth and seventh nerves were paralyzed upon opposite sides, the right eye escaping all injury, though its eyelids could not be closed, while the left, that on the side where the fifth nerve was diseased, was destroyed. In an able paper, in the twenty-eighth volume of the *Medico-Chirurgical Transactions*, Mr. Dixon has adduced much valuable evidence in favour of the correctness of Magendie's conclusions; still, in every case which has hitherto been recorded, so far as I am aware, the long interval which has elapsed before the eye was affected, has afforded some countenance to the opposite opinion. So far as a single case bears any weight, the one now reported appears conclusive; for, taken in connection with what has been already observed, it is impossible to consider the inflammation of the eye as merely a coincidence, and the rapidity with which it occurred excludes the possibility of its having been occasioned by any external irritation.

33. *Complete Amaurosis following Retinitis, successfully treated with Strychnia.*—By DANIEL GRIFFIN, M. D. John O'Donnell, a labourer, aged 62, was received into the County of Limerick Infirmary on the 11th of November, 1852, perfectly blind, but otherwise in good health. He could hardly perceive the strongest light. He said he was but five weeks ill. The complaint began with pain over the brows and in the top of the head, attended with vertigo, so severe as nearly to render him incapable of walking. He had neither been bled nor leeched, and had taken scarcely any medicine; indeed, very little seemed to have been done for him except blistering the occiput and vertex about a fortnight after the first accession of the attack; and to this he attributed his blindness, as his sight had, he says, remained unimpaired up to that time. The iris is sluggish in its movements, but unchanged in colour, and shows no depositions on its surface; the pupils of both eyes are perfectly black, and moderately dilated; there is a zone of red vessels round the cornea, extending a considerable distance from its edge, and the conjunctiva is slightly injected throughout; he stands with that staring, vacant look which betokens the absence of all perception of external objects; there is considerable vertigo still, but the pain is abated.

Eight leeches were applied to each temple. He was ordered blue pill, hydragrym cum cretâ, nitre, and Dover's powder, in moderate doses, three times a day. On November the 15th, four days after his admission, the leeches were repeated as before, and he was directed to persevere in the use of the powders, which on the 29th had produced so little effect that they were ordered to be taken at shorter intervals.

December 1. There is scarcely any appearance of mercurialization, and the only change observable in the eyes is the removal of the increased vascularity, and their return, in this respect, to a healthy aspect; there is not, however, the least sign of returning vision, even hardly so much as the perception of light. As I rather distrusted his statement regarding the length of time he was ill—many persons of his class seeking admission to the infirmary merely for the sake of temporary support—moreover, as I suspected, from the severity of the pain and vertigo, that the affection was associated with organic disease of the brain, I was very much in despair as to his chances, and was about to dismiss him as incurable. On my visit next day, however, he told me he thought there was some slight amendment; I therefore directed him to remain, and ordered the following pills, one to be taken every night: Strychnia, one grain; extract of gentian, six grains; to be divided into twelve pills; the powders to be omitted. 9th. A decided and considerable improvement; the pills to be continued. 13th. Rather stationary since last report; to take a pill night and morning. 27th. The improvement goes on rapidly. He can see all objects around him with considerable distinctness, and walks much more steadily. January 13, 1853. Very much improved. 26th. The right eye is perfectly

restored, and the left nearly so. He says he can read his prayer-book quite as well as he ever did. Dismissed.

I observed, during the progress of this case, that the retina recovered its functions, not by a gradual amendment extending over its whole surface, but in parts and patches separately, almost as if these parts had no physiological connection. It was singular to observe the odd attitudes he threw himself into, as the improvement went on, to find out those parts of the membrane with which he could see when an object was placed before him, such as the hand or the finger. In all cases which have their origin in the inflammatory affection, it would seem to be of great importance to remove all traces of inflammatory action, as well as to pay extreme attention to the general health, before the use of strychnia is commenced.

This remedy has long had a high repute in the treatment of amaurosis; yet M. Riberi says, in the cases which he has hitherto administered it, he has derived little or no benefit therefrom. He suspects, however, that in some of these he has not persevered long enough; and remarks that the successful cases hitherto on record seem to indicate its use in incomplete rather than in complete amaurosis. I believe he always used the endermic method; and, from some published cases of his, he seems to depend more on nuxvomica than on strychnia. It is singular to observe what a small amount of this drug is sufficient for success in some instances. The whole quantity which produced a complete restoration to sight in the case of O'Donnell was somewhat short of eight grains, and this spread over a space of about eight weeks, making about one grain per week.—*Dublin Quarterly Journal Med. Sci.* Aug. 1853.

34. *Ophthalmia in Nursing Women.*—Under the name of retinitis, Dr. MacKENZIE, of Glasgow, has published an excellent notice of this frequent complaint in the *Glasgow Medical Journal*, for April. The most important part of the disease is inflammation of the retina; but almost all the other tissues of the eye may be involved. We are ourselves inclined to consider it as a disease exactly analogous to the ophthalmia which is apt to occur after fever, smallpox, or even in syphilitic cases—a disease which may affect the retina *alone*, in which case it goes under the name of amaurosis, or which may spread to, or originate in, the visible tissues of the eye, when it is denominated ophthalmia. When the retina only is affected, there is no pain and no redness; but as the choroid, iris, and sclerotica participate in the diseased action, pain, redness, and various visible lesions ensue. In all cases, however, the disease is of an asthenic character; and, although requiring local depletion and the action of mercury for its cure, neither of these powerful means are to be used in excess, and both are to be accompanied by a general tonic and invigorating treatment. In the case of the ophthalmia of nursing women, it is very obvious that the child must be taken from the breast, as the very first step in the treatment. Mild mercurials, with belladonna locally applied, if the iris be affected, preceded by local bleeding proportionate to the pain of the eye and the strength of the patient, and accompanied with nutritious diet and rest of the eye, are our best means. Dr. Mackenzie advises that quinia should be given along with the mercury: our own experience would incline us to give the mercury alone in the first instance, till the gums have been moderately touched, and to follow it by the exhibition of quinia or bark. Blue pill, in combination with Dover's powder, is the preparation we commonly employ, rubbing into the temple at the same time a mixture of blue ointment and extract of belladonna, which has often a magical effect in removing the pain. After the mercurial action has been maintained for a certain time, we have great faith in the administration of iodide of potassium, which is of no use, according to our experience, while the inflammatory symptoms continue, but, as we think, very valuable as a "sorbefacient," in accelerating the absorption of the effusions to which must be attributed the amaurotic symptoms so often persisting after an attack of deep-seated inflammation of the eye. In no other kind of amaurosis have we ever seen iodide of potassium of the least use. It is quite obvious that the length of time which has elapsed since the birth of the child, is no test whether the nursing has been unduly prolonged or no. A few weeks is in some cases quite enough to impoverish the

blood, and to exhaust the nervous system. The important point is, that practitioners should bear in mind that no affection of the eyes, occurring in a nursing woman, is to be trifled with; though merely conjunctival at first, it may soon lapse into deeper-seated disease; and slight dimness of vision may, if neglected, rapidly pass into confirmed amaurosis.

We may notice here a paper by Dr. GRIFFIN, of Limerick, in the last number of the *Dublin Quarterly Journal of Medical Science*, on "treatment as a test of pathological conditions;" one of his "glaring instances" being a case of iritis, in which the aggravation of all the symptoms under mercurial treatment, led him to give that up, and resort to the use of turpentine, which rapidly cured the patient. In another instance, a feeble infant, labouring under bronchitis, being nearly killed by the application of leeches, was soon restored by stimuli! Now we humbly submit, that here we have merely the old fact that our art is eminently tentative, and that we often fail in determining, before seeing the effects, what is the best plan of treatment. But surely this is from the imperfection of our diagnosis, and our want of skill only, and is "a test" of our own deficiency, as well as of our patient's "condition."

We mention this here, because in no department more often than in eye diseases, do we find lesions and objective symptoms so very nearly alike, and yet depending on pathological conditions the most opposite, and of consequence demanding the most diverse means of cure.—*Ibid.*

35. *Surgical removal of Opacities from the Cornea.*—M. SZOKALSKI contributes to the *Revue Médico-Chirurgicale* a *compte-rendu* of thirty-two cases in which he has practised this operation. He performs it by scraping the cornea with a delicate knife, as we scrape paper to remove ink-marks from it. The epithelium of the cornea adheres pretty firmly in all cases, but when opaque, is often so tough that it cannot be removed at one sitting. The proper substance of the cornea is still more difficult to remove; in fact, the author considers that, when it yielded to his mode of treatment, the success was owing, not to mere mechanical abrasion, but to the process of absorption set going by the irritation which the operation produces. This sounds like common sense, and at the same time goes far to invalidate M. Szokalski's own results, and to induce the prudent surgeon to trust to gentler means of stimulating the absorptive process. These we see wonderfully efficacious in cases of which we might at first despair. Much, however, as all practical oculists know, depends upon the age of the patient. In infants, we never despair, though the cornea be apparently quite opaque, so wonderful are the cures which nature sometimes effects; and M. Szokalski's neglect to notice the age of those on whom he operated renders his results much less useful than otherwise they might have been.

Of the thirty-two eyes, the author reckons fifteen successful, and eight partially successful operations. In five cases, there was no change effected, and in four the occurrence of inflammation rendered it necessary to stop the proceedings. Of the fifteen successful cases, there were eight in which the opacity was limited to the corneal epithelium, five in which it affected the superficial layers of the cornea proper, and three in which it was deeper still, as it was in all the eight partially successful cases. Thus, taking into consideration the remarks we have already made as to the spontaneous disappearance of opacities in young subjects, and our author's own admission as to the *modus operandi* of his process in the case of the deeper albugines, we do really think that the use of stimulating collyria, etc., and *time*, would have produced results as favourable as his. Again, he states that three of his total failures were cases in which the opacity was merely nebulous and superficial; and, above all, the *accidents graves* which happened in four cases were of the most serious description. He states that, when inflammation does occur as a consequence of these proceedings, it always affects, not the conjunctiva, but the iris and other internal parts of the eye, and may terminate in complete destruction of the globe!

Our conclusion is, that the operation is not to be recommended, *except* in occasional cases, where the opacity fails to yield to judicious treatment of the ordinary kind continued for many months, where it is dense enough to render the

eye useless, where the other eye is also blind, and where the formation of an artificial pupil is out of the question.

One remark of the author we may quote—that his scraping process is much more likely to produce inflammation, when carried on near the circumference of the cornea. This is so far fortunate, since it is precisely in that part that opacity of the cornea is of least consequence.—*Assoc. Med. Journ.* June 23, 1854.

MIDWIFERY.

36. *Paralysis occurring during Gestation and in Childbed.*—The *Dublin Quarterly Journal of Medical Science* (May, 1854) contains a very interesting article on this subject, with the particulars of 84 cases collected from various sources, by Dr. FLEETWOOD CHURCHILL.

The following is a summary of these cases, with his remarks upon the more important points connected with them. The number of cases he admits, however, are too few to justify decided conclusions from them:—

“Of the 34 cases, in 22 the attack occurred during pregnancy; in 12, either during or after labour.

“In 23 cases where it is mentioned, I find that with 10 it was their first child, with 1, the second; with 4, the third; with 2, the fourth; with 3, the fifth; with 1, the sixth; with 1, the thirteenth; and 1 had several children, but the number is not specified.

“Of the 34 cases, there were 17 of complete hemiplegia, and 1 partial; 4 of paraplegia, in 2 of which only one leg was affected; 6 of facial paralysis; 5 of amaurosis, and 3 of deafness; but in some of these latter local palsies were combined with the cases of hemiplegia. Of 14 cases of hemiplegia, in which the side affected is mentioned, I find that 11 were of the right, and 3 of the left side.

“Of the 34 cases, 4 died.

“It may be well, however, to consider these cases somewhat more closely, and for that purpose they may be divided into two classes, those which occurred during pregnancy, and those which were attacked during or after labour.

“Of the 22 cases in which paralysis occurred during pregnancy, 12 were examples of hemiplegia; 1 of paraplegia, which had occurred previously; 4 of facial paralysis; 2 of amaurosis; and 3 of deafness. There is no regularity as to the period of gestation at which the seizure took place, for of 13 cases in which this is mentioned, in 1 it occurred in the second month; in 1 in the third or fourth; in 1 in the fifth; in 1 in the sixth or seventh; in 3 in the seventh; in 2 in the eighth; and in 4 in the ninth month; from which it would seem, upon the whole, that it is in the latter months that pregnant women are most liable to the attack.

“Of 19 cases, 11 appear to have been cured before or by delivery, and in 8 the disease continued for a longer or shorter time afterwards.

“Of the 20 cases, only 1 died, and in this case it is evident that death was rather owing to disease of the brain, of longer standing than the pregnancy, than to the paralysis which increased during that process; so that I do not think we can reckon it as impairing the comparatively innocuous character of these attacks during gestation.

“In 3 cases only was the paralysis preceded by convulsions. In most of the cases it does not appear that there were any premonitory symptoms, little or no headache, or any other circumstance calculated to excite apprehension until the paralysis supervened. The characteristics of the palsy resembled very closely those of similar attacks unconnected with pregnancy; the motor power was enfeebled or altogether lost; in some the sensibility was increased, diminished, or modified; but in others, I infer from the silence of the reporter, that it was little, if at all, changed from its natural condition. The intellect seems to have preserved its integrity in all the cases. A peculiarity of great interest

in many of these cases, and to which I shall revert by and by, is the presence of albumen in the urine, whenever that secretion was carefully examined.

"The second class, consisting of 12 cases, is characterized by the attack occurring during or after labour. It is remarkable that in 3 cases only (Cases 23, 24, 25) did the paralysis take place during labour, and of these, 2 were cases of convulsions; in all the others it not merely succeeded labour, but in most cases after an interval sometimes considerable: for example, in Case 23, it took place on the first day after delivery; in Case 27, two days afterwards; in Case 32, three days; in Case 34, seven days; in Cases 26, 29, 31, eight days; in Case 30, ten days; and in Case 33, a month afterwards.

"Of these 12 cases, 5 were cases of complete hemiplegia; in 1 only the arm was affected; 1 was a case of complete paraplegia; in 1 the right, and in 1 the left leg only was paralyzed; 2 were examples of amaurosis; 1 of facial paralysis; and in 3 only of the cases of hemiplegia the face participated in the attack. In Dr. Levy's very remarkable case, the paralysis of the motor power of one side was accompanied by loss of sensibility on the other. In some of the cases the sensibility was diminished, in others unaltered, but in none increased. The phenomena of the disease were not peculiar: in the majority of the cases the attack occurred generally without warning, and without any obvious cause. In 2 cases convulsions terminated in amaurosis, but in Mr. Forrest's case the paralysis preceded the convulsions, and during the latter, the paralyzed limbs shared in the convulsive movements.

"The duration of the disease varied a good deal, the paralysis gradually subsiding in most cases: in Case 22, after several days; in Case 23, in six weeks; Case 31 recovered the use of the arm in a fortnight, but vision remained imperfect for some months; in Case 34, in a month; in Cases 28, 29, in two months; Case 22 recovered the power of walking in two months, but was then attacked by another disease which proved fatal; Case 26 left the hospital without improvement.

"In 3 cases death occurred: in Case 32, on the fourteenth day, and in Case 34, on the twenty-fourth day after the paralytic seizure. Dr. Ley does not mention on what day his patient died.

"I have already alluded to the fact that in most of the cases the attack occurred without warning, and without apparent cause. Some cause there must be, of course; but it is much easier, in most cases, to say what it is not than what it is. For example, in none of these examples except one, did it appear to depend upon any external influence—upon cold, exposure, violence, &c.—or upon mental distress; in few, if any, was there evidence of previous cerebral congestion, or disease of any other organ.

"It has been suggested that the palsy may be merely the termination of convulsions; and certainly some of these cases would seem to support this view; but if this were generally true, we should find convulsions more frequently preceding the paralysis; and, also, we should meet with more cases of convulsions terminating in paralysis. Now, in all the cases I have quoted, a large majority exhibited no convulsive movements at all, and, on the other hand, of all the cases of convulsions related by Drs. Collins, and McClintock and Hardy, there is not a single instance of such a termination; we must therefore refer both convulsions and paralysis to some common or different cause.

"I have no doubt, as Dr. Romberg has observed, that in a number of cases, especially those which occur during gestation, the palsy is due to a reflex action from some organ or structure in a morbid condition, and in which the nervous system seems to be merely the channel of transmission, offering no central disorganization. In such cases the exciting cause may possibly be some injury or morbid condition of the generative organs, or perhaps merely a transient excitement, such as that of pregnancy. It is possible, also, that some of the instances occurring during gestation ought rather to be classed under the head of hysterical paralysis, as described by Drs. Laycock and Romberg; but it is not always easy to make the distinction.

"Obstruction of the arteries has been recently shown by Professor Simpson¹

¹ Edinburgh Monthly Journal, February, 1854.

to be an occasional occurrence in childbed, either from arteritis, a coagulum, or a detached vegetation; and a degree of paralysis may be the result; but inasmuch as the death of the limb, and ultimately of the patient, is the direct consequence of such an occurrence, the history of the cases I have quoted removes from them the suspicion of being thus caused.

"It might naturally be supposed that the stress and exertions during labour, which give rise to such great congestion of the face and head by also occasioning congestion of the brain, might be considered one of the principal causes: but such a supposition is not borne out by facts, for, excluding the cases of convulsions, in only one case did the paralysis occur at the time of labour; in all the others it either supervened before labour, or subsequently, at a time when all such direct action must have ceased, and in some, after such an interval that we cannot suppose it even a remote effect of the parturient agony. On the other hand, when we remember the number of severe labours in which no such attack occurs, or compare its frequency with that of convulsions during labour, we can scarcely attribute much influence to this cause.

"Again, as we have seen, paraplegia has been attributed to severe and prolonged labour, and to the consequent mechanical pressure upon the nerves and muscles of the pelvis, and at first sight this seems an adequate and feasible explanation, and of which no one could deny the possibility; yet so far as our cases are concerned it can hardly have been so, for in all but one the labour was natural, easy, and not prolonged: in the exceptional case the patient had been delivered by the forceps; moreover, the period at which it occurred was too distant to justify our attributing it to this cause in the other cases. On the other hand, if we recollect the number of severe, prolonged, and instrumental deliveries which take place, without any such result, no example being recorded by Drs. Collins, McClintock, and Hardy, or, with the exceptions I have quoted, in any of the reports of the British and foreign hospitals, so far as I am acquainted with them, I think we must also reject this peculiarity of labour as a necessary or frequent cause.

"In two cases the attacks seem to have been connected with an anemic condition, consequent upon hemorrhage, either from the direct effect of a deficiency of the circulating fluid, or indirectly from the increased susceptibility of the nervous system, under these circumstances, to ordinary exciting causes. In another case paraplegia appeared to result, from cold; but, in the majority of cases, as I have already observed, there was neither plethora nor anemia; neither exposure, want, injury, advanced age, mental distress, nor sudden shock; in short, there was no apparent cause.

"Unfortunately for the cause of science, there are very few *post-mortem* examinations on record, from which we might decide with some degree of certainty upon the nature of the affection. In all the slighter and more partial cases, life is preserved, and when death occurs in the more severe instances, permission to examine the body cannot always be obtained. Of the four fatal cases I have here detailed, two only were examined; in these, and I doubt not in the other two also, disease of the brain or its membranes existed. In Dr. Ley's case, he states that "no positive disorganization of the brain could be detected. The ventricles, however, contained more than the usual serum; and there was found, more especially opposite to the original seat of pain, thickening and increased vascularity of the membranes, with moderately firm adhesions in some parts; in others an apparently gelatinous, transparent, and colourless deposit interposed between them." In short, there appears to have been an attack of partial meningitis, and the contrast between the peculiar train of symptoms to which it gave rise, and the absence of all symptoms except the palsy in Dr. Duke's case, is very interesting, when we remember the remarkable disorganization we discovered in the latter case.

"Now in these cases we may fairly assume that the palsy and death itself were the result of the disease of the brain and its membranes, but to what are we to attribute the slighter and more numerous cases? Do they not appear to belong to the class described by Dr. Abercrombie, as 'depending upon a cause which is of a temporary nature, and capable of being speedily and entirely removed?'

"What is this temporary cause, producing so serious a disturbance, and yet scarcely, if at all, endangering life? May it be the one to which Dr. Latham refers, as observed 'in those convulsions and apoplexies which appear and disappear, the chief circumstance which attracts our attention being albuminous urine?' At any rate, it deserves our careful attention. Of the fact of the concurrence of albuminuria with certain affections of the nervous system during pregnancy and childbed, there can be no doubt whatever. Both Drs. Lever and Simpson have detected it in cases of convulsions during pregnancy and labour: the former observes: 'I have carefully examined the urine in every case of puerperal convulsions that has since come under my notice, both in the Lying-in Charity of Guy's Hospital and in private practice, and in every case but one the urine has been found to be albuminous at the time of the convulsions.' 'I have further investigated the condition of the urine in upwards of fifty women, from whom the secretion has been drawn during labour by the catheter, care being taken that none of the vaginal discharges were mixed with this fluid; and the result has been that in no cases have I detected albumen, except in those in which there have been convulsions, or in which symptoms have presented themselves which are readily recognized as precursors of puerperal fits.' Dr. Simpson's observations about the same time, and those of more recent observers, Sabatier, Legroux, Richelot, and others, have confirmed the conclusions of Dr. Lever as to the presence of albumen in the urine in cases of puerperal convulsions, so that no doubt now exists as to the fact, although we occasionally meet with cases of convulsions without albuminous urine, and of albuminuria without convulsions.'

"Now, as paralysis in some cases occurs in connection with convulsions, if not as a consequence of them, we might, not unnaturally, expect albumen in the urine of such patients, and accordingly, in a patient of Dr. Lever's, and in others, we find that it has been detected.

"But we may go a step further, and state that in cases where no convulsions have preceded the paralysis, albuminuria has been equally observed. Dr. Lever says of his cases, that in none in which he examined the urine did he ever fail to find albumen, and the great experience of Professor Simpson is in close accordance with this, as may be seen by the quotations I have given, and by the cases with which he has favoured me. This was observed also in Dr. Duke's case, where the paralysis succeeded the delivery; and in which I think there is ground for believing that the albumen had diminished at the time the urine was first examined. In all probability it would have been detected in many others, had an investigation been made.

"Thus we find that albuminuria may be a marked symptom in puerperal convulsions, whether terminating in paralysis or not; and in the palsy of pregnant and puerperal women, whether partial or complete, whether local or general; and if the observations are yet too few to draw any very positive conclusions, it is, I believe, because our attention has not been drawn to the subject. And when, in addition, we find, as Dr. Lever states, that as the albumen diminishes, the paralysis subsides, we can hardly doubt that there is some important connection between them.

"What, then, is the precise pathological significance of albuminuria? We

¹ It may be of interest to append Dr. Seyfert's conclusions on this subject. "1. Albuminuria is not an essential accompaniment of normal, healthy pregnancy. 2. The theory, ascribing albuminuria to the pressure of the enlarged uterus on the renal vessels, is inadmissible. 3. When anasarca, from Bright's disease, occurs during pregnancy, the patients are seldom attacked by eclampsia. 4. The albuminuria, in cases of eclampsia, is occasioned by the interruption of the functions of the respiration and circulation by the attack. 5. In such cases the albuminuria terminates with the attack. 6. Albuminuria is not present in all cases of eclampsia. 7. Albumen is found in large quantities in the urine of epileptics, immediately after an attack; but not invariably after every seizure, or in every case of the disease. 8. Provided there be no Bright's disease, this albuminuria among epileptics ceases soon after the convulsions, and only returns after the next attack."—*Edinburgh Monthly Journal*, Feb. 1854, p. 168.

may assume as established, that although it occurs in Bright's disease, *it alone is no proof of the presence of that disease*; but in the present state of our knowledge it is very difficult, perhaps impossible, to come to any very decided conclusion upon the matter. It is conceivable that an unusual, morbid, or noxious ingredient in the urine may be produced in either of three ways: 1. By simple elimination from the blood, in which it was present; 2. As the result of diseased action of the kidneys, excited either by some noxious principle in the blood, or by a morbid condition of these organs; or 3. As a new compound, the result of chemico-pathological action, which we may or may not be able to explain.

"Now, albumen in the urine cannot be placed under the latter category, as it is not a new principle, but one already existing in the blood. Nor does it come under the first, for although it is possible that it might be eliminated from the blood in which it is present, it cannot be as a noxious element, nor would this simple elimination account for the condition of the kidneys or for the concomitant symptoms. So that it would appear this secretion of albumen must be owing to some disordered action of the kidneys, excited by some morbid element, in kind or degree, which they are endeavouring to separate from the blood. This seems at least to be the opinion of a high authority, Dr. George Johnson, of London, who, in describing acute desquamative nephritis, in which albumen is so largely secreted, observes; 'that all the changes of structure commence in the secreting cells of the gland, and are the result of an effort made by the cells to eliminate from the blood some abnormal products, some materials which do not naturally enter into the composition of the renal secretion.'¹ This view is further confirmed by a *post-mortem* examination into the state of the kidneys themselves in albuminuria. Dr. Handfeld Jones, in a recent paper, has described three varieties: 'The first is the condition of engorgement, such as is seen in those who die in the early stages of acute anasarca, or in that of dropsy succeeding scarlatina. The organ is enlarged, dripping with blood in every part; its tissue not destroyed, but many of the tubes are seen, under the microscope, to contain coagula of exuded fibrin, entangling blood-globules, and more or less of epithelium.' 'The second form of diseased renal structure is that of the large, heavy, often mottled and pale kidney. In this there is no hyperemia, but rather the reverse state usually exists. The cut surface has not the appearance of healthy structure, and gives one the idea of some matter having been implanted among the natural constituents, so as to obscure them and to produce a confused aspect. The tubes are found impacted with epithelial matter, but not by any means constantly obstructed or blocked up, although they may be irregularly dilated, &c.' 'The third variety of morbid change is that so familiar to observation as the dwindled, granular kidney.'²

"When we consider the temporary nature of the albuminuria in many of the cases of paralysis, we need have little doubt that the condition of the kidneys answers to the first variety here described, or that of extreme congestion, and this opinion is confirmed by the examination of Case 34, in which we found a high degree of congestion, which had indeed passed into a more advanced stage. I think, therefore, that we may fairly assume the albuminuria is due to a congested state of the kidneys, and I confess I cannot but think that the explanation given by Dr. G. Johnson and others, that this congestion is excited by the effort to eliminate some noxious element from the blood, is more in accordance with our present knowledge than any other, yet I must not omit to mention that by some this congestion has been attributed to pressure of the gravid uterus upon the renal vessels. Dr. Seyfert, as we have seen, rejects this mechanical explanation, and seems to attribute the albuminuria to the eclampsia, in consequence of the interruption of the functions of respiration and circulation.

"But, if the former theory be true, what is this morbid element, morbid in kind or degree? It is very difficult to answer this question. Dr. Simpson suggests that it may be an excess of urea or some morbid quantity or quality of caseine in the blood. Dr. George Johnson's observations seem to prove that

¹ Diseases of the Kidney, p. 105.

² Medical Times and Gazette.

in these cases, in addition to a change in the proportion of the normal constituents of the blood, of which the diminution of its albumen is one, there is always an excess of urea.

"Then it may be asked, 'to what is the effect upon the nervous system owing?' One can conceive that it may result either—1. From the continued presence of the noxious principle in the blood; or, 2. From the balance of the constituents of the blood having been destroyed; or, 3. From the diseased condition of the kidney—though to which of these we ought to attribute it, would be difficult to decide.

"But at whatever conclusion we arrive with respect to these interesting points, I am sure all will agree with me, that, taking the circumstances into consideration, it is probable the kidneys play a more important part in these paralytic affections than has been suspected, and that the subject deserves more attention than it has received. For, we find that in cases of convulsions terminating in paralysis, we may have albuminuria; in paralysis before delivery, without convulsions, we may have albuminuria; in paralysis occurring after delivery, we may have albuminuria; and further, that in the slighter cases, both the convulsions and paralysis diminish with the decrease of the albuminous secretion. Whether, therefore, the paralysis be caused by the state of the kidneys, or the renal congestion and paralysis be both the result of some morbid matter in the blood circulating through the system, it is clear that a new element may be added to those which have usually been considered as giving rise to paralysis.

"Nor is this barren theory only; but, if it be true, it has a direct bearing upon practice, inasmuch as our attention ought not to be confined to the secondary affection of the nervous system in such cases, but must be directed to the relief of the renal malady, and to the restoration of the kidneys to such a state of efficiency as may enable them to remove the morbid constituents of the blood; and for our encouragement, we have seen that a diminution of albumen in the urine is followed by mitigation and cure of the paralysis. For the latter affection, bloodletting, general when the system will bear it, or local by means of leeches or cupping; blisters, purgatives, and mercury, are the remedies usually employed; these must be modified according to the condition of the patient, the circumstances of the attack, and the duration of the disease. When much blood has been lost during labour, bloodletting must be omitted, and we must confine ourselves to counter-irritation; perhaps a series of small blisters to the neck, down the spine, or along the limb, will be the best mode of proceeding. The patient's strength must be supported judiciously by good diet, and it is quite possible that some stimulant, such as ammonia or camphor, may be necessary. When the paralysis has become chronic, strychnia or galvanism may be found useful; and I believe Dr. Stokes has found galvanic acupuncturation very beneficial in facial paralysis.

"The renal disorder should never be treated by diuretics, but by external irritants, such as mustard poultices, or rubefacient liniments to the loins, and internally by diaphoretics, as suggested by Dr. Osborne¹ of this city, and when more chronic, by gallic acid, iron, &c."

37. *Inversio Uteri taking place during Labour.*—DR. GEORGE JOHNSTON, submitted to the Dublin Obstetrical Society, a case of this which came under his notice while assistant in the Dublin Lying-in Hospital.

Esther Page, aged 19, a thin delicate-looking woman, of fair complexion, was delivered of her first child, a healthy girl, on the 31st of July, 1851. Her labour so far was easy, and of about six hours' duration. The gentleman in attendance, after having tied and separated the funis, had maintained the contraction of the uterus with the hand above the fundus—in accordance with the usual practice of the hospital—for about a quarter of an hour, when finding a tendency to "draining," he increased his pressure; but, as he said, not nearly to the extent it had been, on frequent occasions, found necessary to employ, in order to assist in the expulsion of the placenta, or restrain hemor-

¹ On the Nature and Treatment of Dropsies, &c. 1837.

rhage. The uterus was felt suddenly to yield and recede from his grasp, and he immediately saw it expelled from the vagina, an inverted mass, with the placenta still attached. Dr. Johnston was at once sent for; on his arrival he found the woman pallid, exceedingly anxious, complaining of considerable pain, and a sensation of sinking; the pulse was weak, indeed scarcely to be felt.

Examination proved the uterus to be inverted with the placenta attached to its fundus; the funis was of the ordinary length, and there was then no hemorrhage. Recollecting that the lapse of every minute was of consequence, Dr. Johnston proceeded to replace it at once, which he accomplished in the following way: He first detached the placenta—a matter of no difficulty, there being no morbid adhesion—and he was pleased to find that after it was separated, no hemorrhage followed, owing, he considered, to the constriction the vessels underwent at the cervix; he then restored the cervical portion of the inverted organ, which was easily returned within the vagina, and reinverted as far as the body; but it was some time (five to seven minutes) before he could reduce the fundus, which required the fingers to be held in a flexed condition against it, while he made counter-pressure with the left hand above the pubis. Some wine had been given to the patient to relieve the sensation of exhaustion, but it was not till the uterus had been restored to its natural state that she could be persuaded her immediate dissolution was not close at hand; ergot was afterwards administered, and she was kept longer in the horizontal position than ordinarily. Milk was secreted on the third day; she made a perfect recovery, and was discharged quite well.

Dr. George Johnston then proceeded to remark as follows:—

“That inversion of the uterus very rarely takes place, is universally allowed by all obstetric writers.

“Denman, Rigby, and Churchill, all agree in believing it to be an accident of ‘rare occurrence.’

“Ashwell says: ‘It is *most rare*,’ and in proof states that, in more than 8,000 labours occurring in Guy’s Hospital, and upwards of 1,600 in another charity, there was not a single instance of it; if others were wanting, the records of our own hospital show (according to Hardy and McClintock) that, during the masterships of Doctors Clarke, Labatt, Collins, Kennedy, and Johnson, the number of deliveries amounting to 75,911, not one case of this description occurred; and I may add that, subsequent to that period, for three years and eight months of the management of the present master, Dr. Shekleton, 7,336 patients were delivered previous to the occurrence of the one I have just related, so that in 83,247 deliveries, there was not a single instance of its being met with.

“But, ‘*inversio uteri*,’ although rare, is nevertheless considered to be one of the most formidable complications that affect the lying-in patient, being attended with very alarming symptoms, and threatening the most serious consequences. For instance, the patient is found complaining of excruciating pain, with a sense of sinking and extreme exhaustion; an almost imperceptible pulse; a countenance expressive of great anxiety and collapse, together with nausea, vomiting, and sometimes hemorrhage and convulsions; in fact, the symptoms in a case of inversion resemble in many points those attendant on rupture of the uterus; but this former accident can be generally distinguished from rupture by the period at which it takes place, viz., after the birth of the child; by the sudden recession of the round, circumscribed tumour, if the uterus be grasped after the delivery of the child, or its absence, when searched for. All doubt is set at rest by finding a globular fleshy body occupying the os uteri, if the inversion be partial; or protruding quite through the os and vulva, if it be complete; thus forming, as it were, a sac lined by peritoneum, a diverticulum from or extension of the abdominal cavity, filled by the small intestines. It is this sudden evacuation of so large a portion of the contents of the abdomen, which produces that severe shock on the nervous system, from which there is great danger the patient may not rally, death in more than one instance having been the result.

"This accident has been attributed to various causes, viz :—

"1. To undue pulling at the funis for the purpose of extracting the placenta before it is completely separated.

"2. To an over-amount of pressure with the hand on the fundus uteri.

"3. To too rapid delivery, especially if the woman be standing at the moment of the quick expulsion of the child.

"4. To the cord being too short, or twisted round the neck or body of the child.

"5. To violent straining during the last pains; violent efforts, as coughing, vomiting, sneezing, or by sudden attempts to rise in bed, by which the abdominal muscles are put into violent action.

"6. It may take place spontaneously.

"That it may be produced by the first two causes, no doubt at all exists, particularly the first—'if,' as Dr. Ramsbotham observes, 'the placenta be adherent to the fundus; if the adhesion be strong; if the funis does not give way to the force applied to it; and if the uterus be flaccid, and has not contracted round the mass.' And with regard to the second, Rigby goes so far as to say: 'If immediately after delivery, especially where the uterus has been suddenly emptied of its contents, any force be applied to the fundus, it may easily be pushed down into the cavity.'

"That it may result from the occurrence of too rapid delivery, especially while the woman is in the erect posture, is possible: but such cases of delivery have frequently come under the notice of those connected with the Dublin Lying-in Hospital, and yet no such accident has taken place.

"As to shortness of the cord, or the twisting of it round the neck of the child, numerous instances of the kind could be brought forward where there was no attempt at inversion.

"That violent strainings or efforts, or sudden attempts to rise in bed, have been the cause of this accident, instances are given by Cazeaux: one, where complete inversion took place nine hours after delivery, from rising to the night-chair; another, where complete inversion resulted twelve days after delivery, in consequence of straining efforts at stool. Lastly, that it occurs spontaneously; an instance is given in the author's notes on Denman, where, after the funis had been divided, and the doctor engaged with the child, not the slightest extension having been made on the cord—'in fact, it had not been touched by the hand'—the uterus was inverted, with the placenta attached.

"Dr. Rigby, in giving a case of this description, says: 'The descent was so rapid and forcible through the os externum, that it would have been quite impossible to have resisted the unnatural action by which the organ was carried down;' and Ruysch saw it take place after the expulsion of the placenta, although delivery had occurred in the most favourable way.

"The predisposing causes of inversion are, according to Radford, owing to atony of the uterus, or active constriction of one part, with an atonic condition of another.

"Dr. Tyler Smith says it depends upon an irregularly active and a very unusual condition of the uterus, by which the fundus is first depressed, then carried downwards by the annular contraction of the organ, and finally completely inverted.

"Cazeaux states, that the uterus, being for a moment in a condition of inertia, the pressure or weight of the intestinal mass upon the fundus may depress it like the bottom of a bottle; and when the placenta is inserted directly on the superior part of the uterus, its weight alone may (in a case of complete inertia) draw down the fundus. This, he says, however, generally corrects itself by the contraction of the viscus; but if (this depression not being perceived) traction is made on the cord, or pressure on the fundus, it may be greatly increased, and converted into complete inversion: this was probably the cause in the case just narrated.

"With regard to the treatment of this accident, it is universally agreed upon, that the immediate reduction of the inversion is the best practice, when 'we find'—as Denman remarks—'no difficulty, or very little, in restoring the uterus to its perfectly proper situation.' But difference of opinion exists as to

the mode of acting when the placenta is still adherent to the uterus; some recommending the returning of the tumour before detaching it. For example, Newenham advises 'returning the uterus first, and exciting it to throw off the placenta afterwards in the usual way; 'which,' he says, 'will have good effect in bringing on that regular and natural contraction which is the hope of the practitioner and the safety of patient: that the removal of the placenta first, in order to diminish the bulk of the inverted fundus, cannot possibly be attended with any beneficial consequences, whilst the irritation induced by such a proceeding will necessarily tend to bring on those pressing-down efforts which would present a material obstacle to its reduction, and would increase the hemorrhage at a period when every ounce of blood is of infinite importance.' Denman goes half way with the above maxim, for he says: 'If the placenta be partly separated, it will be proper to finish the separation before we attempt to replace the uterus; but if the placenta should wholly adhere, it will be better to replace the uterus before we endeavour to separate the placenta.' And his reason he asserts to be: 'That while we are separating the placenta, the cervix of the uterus is speedily contracting, and the difficulty of replacing it increasing, which is a far greater evil than a retained placenta.'

"Now, though Dr. Newenham's and Dr. Denman's ideas may be perfectly correct in cases where the uterus is partially inverted, that is, where the fundus only is displaced, nevertheless, in cases of complete inversion, although we find Dr. Williams saying, that 'the organ, with the placenta still adhering, was promptly returned to its proper situation,' Dr. Merriman states, that the mass of the placenta was the chief cause of the difficulty in a case which he has related. 'I tried,' says he, 'to effect the reduction without removing the placenta, but could by no possibility accomplish it till I had first separated the placenta; this being effected, I succeeded to my entire satisfaction in re-inverting the fundus.'

"For my part I am of opinion that, in cases of complete inversion, it is the best and simplest treatment to detach the placenta in the first instance (which is easily, and in a very short time accomplished), and then to reinvert the uterus, a matter also of but little difficulty, as it is then much diminished in bulk. The objection that Denman raises to this practice is not, I conceive, a very strong one; for 'the time' it takes to peel off a placenta, under these circumstances, would be far less than that which would be occupied in trying to force so large a body as the placenta with the uterus back through the vagina and os. As to the occurrence of an increase of hemorrhage from thus proceeding, I consider there is no danger, or at most but trifling. Even Dr. Denman, in the history of the case, where he pursued this line of treatment, informs us, that 'the hemorrhage was not profuse,' and 'that regular and natural contraction,' which, as Dr. Newenham observes, 'is the hope of the practitioner, and the safety of the patient,' is, I think, more likely to be produced when the uterus is empty, than when it still contains the foreign body, which its efforts to expel had been the cause of the production of that very displacement we are called upon to remedy. The placenta having been detached, we proceed to reduce the tumor by grasping the body of the organ, and pushing it with the vagina in the axis of that passage, continuing this pressure till we meet the os, when the first obstruction presents itself; but, by the steady maintenance of our pressure, and by moulding, if I may so express myself, gradually and progressively, first, the cervix, and then the body through the os, we complete the reduction by pushing, lastly, the fundus upwards.

"With regard to the manner in which the last stage of the operation is completed, one recommends the fingers to be held in the form of a cone, another spread out at equal distances; but I held them in a flexed condition, pressed against the displaced fundus, and continued steady pressure till that portion was righted; and, in fine, held the hand in the cavity of the uterus, till it was expelled by contraction.

"In the above case I made counter-pressure over the pubis, finding the uterus yield so much from my reach, owing to the relaxed state of the vagina; and my object in administering the ergot was to secure uniform construction.

"It is said by Cazeaux that inversion of the uterus having once taken place

in a labour, there is a tendency to its recurrence at a subsequent delivery, and I regret that in this instance an opportunity has not been afforded of testing this fact."—*Dublin Quart. Journ. Med. Sci.* Feb. 1854.

38. *Phlegmasia Dolens in the Non-Puerperal State*.—Dr. McCLINTOCK read to the Dublin Obstetrical Society (April 30, 1853), a communication upon phlegmasia dolens, as a disease of women *not* in a puerperal state. After a brief historical sketch of the notices by Puzos, Willan, Lee, Copeland, Meigs, and others, of this disease under the circumstances just mentioned, he proceeded to narrate an instance of the kind that had fallen within his own knowledge.

The case differed from those related by any of the above authors in its purely idiopathic character. The possibility of any chronic uterine disease having existed was not admissible, neither had there been anything like symptoms of metritis, nor yet was the crural attack preceded by *sudden* suppression of the catamenia.

The subject of the case was a young lady, aged eighteen, who had been for some time under treatment for scanty menstruation and symptoms of incipient chlorosis, consequent upon a change of residence from the country to town. One evening in autumn, whilst the menses were present—though, as usual, in small quantity—she stood for nearly two hours together on damp grass. On the following morning she felt the right leg rather stiff and painful; towards evening it grew worse, and began to swell. Two or three days passed over before alarm was excited, or any treatment adopted, and by this time the symptoms had become fully developed. The pulse was 120; the right leg was swollen, tense, and free from discoloration; no pitting on pressure; not much pain complained of, unless the limb was moved or handled; marked tenderness in the groin, over the femoral vessels. The line of treatment pursued was the same as that usually employed for phlegmasia dolens, and consisted in the application of leeches over the femoral vessels in Scarpa's space, constant stuping of the entire limb, absolute rest, and low diet. Under this management, the acute symptoms subsided in the course of a week. One relapse took place, which necessitated a recourse to the antiphlogistic treatment, and considerably retarded her convalescence: after this was subdued, some stiffness, and enlargement of the leg from the knees down, still remained, and continued for very many months, in spite of bandaging, frictions, &c. These symptoms were always increased towards evening, or after much walking or standing. Nearly eight months elapsed before the limb had so far recovered its former state and condition that she could use it in the ordinary movements of progression without feeling any pain or inconvenience.

In conclusion the author remarked, that in this case the existence of chlorosis, which is universally held to be a blood disease, tended to confirm the views of Dr. M'Kenzie, in regard to the etiology of phlegmasia dolens, that physician being of opinion that vitiation of the blood has much to do with its production. —*Dublin Quarterly Journ. Med. Science*, Nov. 1853.

39. *Duration of Pregnancy*.—Dr. J. MATHEWS DUNCAN, in an interesting paper (*Monthly Journ. Med. Sci.* March, 1854), gives the following conclusions:—

1. That the interval between conception and parturition (the real duration of pregnancy) has not been exactly ascertained in any case.

2. That the average interval between insemination and parturition (commonly called the duration of pregnancy) is 275 days.

3. That the average interval between the end of menstruation and parturition is 278 days.

4. That the intervals between insemination and parturition, and between menstruation and parturition, have no standard length, but vary within certain limits.

5. That while absolute proof of the prolongation of real pregnancy beyond its usual limits is still deficient, yet there is evidence to establish the probability that it may be protracted beyond such limits to the extent of three or even four weeks.

40. *Cæsarian Section with Good Result both for Mother and Child.* By Dr. HAMER, of Ommerschans.—A primipara, aged thirty, who had suffered in her youth from rachitis, experienced her first pains February 2, 1853, at the normal term of pregnancy. At 8 P.M. the membranes burst; but the head did not descend into the pelvis, and the pains ceased. It was therefore thought desirable to turn. The hand, however, encountered an insuperable obstacle in the promontory of the sacrum, between which and the pubes there were only two and a half inches. At 11 P.M., the Cæsarian operation was performed. The first incision was made along the linea alba; the second incision into the uterus extended into the placenta, from which much blood flowed; but the extraction of both child and after-birth occupied but a few seconds, after which the uterus contracted firmly and sufficiently. The child, a boy, survived. The abdominal wound in the mother was united by sutures, and a proper bandage was applied. Upon the occurrence of pain towards evening, in the hypogastric region, a catheter was introduced into the bladder, and some urine drawn off; and this was often repeated, to the patient's great relief. On February 4, there was a tympanitic state of the abdomen; the tongue was dry; pulse frequent; lochial discharge normal. Ordered an enema; the application of ice. The day following there was severe pain in the abdomen; vomiting of green matter, by which three sutures gave way, and the gaping uterus was exposed at the bottom of a deep wound. Twenty leeches were applied, and the wound now reunited by plaster. A tube was introduced into the rectum, by which much gas was drawn off, and the tympanitis was diminished. A quarter of a grain of morphia was then administered, and the patient obtained some sleep. To support the abdomen, two pieces of wood, to which silk threads were attached, were rolled in long pieces of plaster. The strips of plaster were then applied from the lumbar region in such a fashion that the wooden staffs lay near the mesial line longitudinally on either side of the wound; the silken threads were then tied, and the aperture was firmly closed. In six weeks she was well.—*Med. Times and Gaz.* Aug. 12, from *Schmidt's Jahrb.* bd. 83.

41. *Case of Pregnancy in a Rudimentary Horn of the Uterus, with probable Advance of the Ovary from the right Ovary into the left Horn of the Uterus.*—In his *Handbook of Pathological Anatomy*, Prof. ROKITSANSKI describes an unique preparation, in the Viennese anatomical collection, exhibiting a pregnancy in a rudimentary uterine horn. The example of Professor Scanzoni is an interesting addition to this extraordinary class of cases.

The woman generally enjoyed good health, and was regularly menstruated. She was married in her 28th year, had a miscarriage of twins five months afterwards, and subsequently bore three children, which still live. In her last two pregnancies she tried by violent exercise to induce premature labour. During the first half of all her pregnancies she suffered much from vomiting, toothache, heartburn, and œdema of the lower extremities.

In July, 1852, she conceived for a fifth time, and again used every means to induce premature labour. No disorder of general health occurred in this pregnancy. On the 21st Nov. she had a quarrel with her husband. The same evening, and also on the next, she complained of slight colicky pains in the left hypogastric region, which, however, did not prevent her from going about. But she soon became very weak, and had to be carried to bed. This was at ten o'clock, forenoon. At midnight, Dr. Scanzoni visited her and diagnosed an extra-uterine pregnancy, with profuse internal hemorrhage, in consequence of the rupture of the containing sac. She died almost immediately afterwards.

At the *post-mortem* examination, there was found in the lower half of the abdomen a great quantity of variously altered blood. After its removal, there was observed on the left side a round swelling of about the diameter of 3½ inches, with a laceration on its outer border. In it were contained the membranes and placenta of the fœtus, along with the coagula of blood. The cord was six inches long. The corpus luteum was in the right ovary. Both Rokitsansky and Scanzoni, on first viewing their cases, thought they had to do with tubal pregnancy, and only on further examination discovered the true nature

of the cases. Such cases are described by Rokitansky as an intermediate link between uterine and tubal pregnancy.

Scanzoni's case is interesting physiologically, as belonging to a series where the corpus luteum is formed on the ovary of the side not corresponding to the uterine horn or tube which is pregnant. Such cases at once suggest the idea that the ovum must have performed a long journey from the ovary of the one side, through the uterus, and into the passages belonging to quite the other side. Scanzoni states that many analogous observations have been made in the lower animals, but he is mistaken in supposing that his own is the first in the human subject.—*Edinburgh Med. and Surg. Journ.* Jan. 1854, from *Verhandlungen der Phys. Med.* 1853.

HYGIENE.

42. *Lead Cisterns and Pipes in a Sanitary point of View.* By ROBERT DUNDAS THOMPSON, M. D.—Having been consulted on the subject of the supply of water to Glasgow, Gorbals, Dumfries, Kilmarnock, Sterling, London, Newcastle, Swindon, Liverpool, &c. &c., and very extensively on the sanitary condition of waters, the subject of the action of water on lead has for many years been familiar to me, and I have had considerable opportunities of meeting with cases where paralysis had been produced by the action of lead on the human system. As far as I can ascertain from the experience of hospitals, the occurrence of disease from the corrosion of lead pipes is an exceptional circumstance, if it has ever occurred in these institutions, the general cause of affections from lead being occupation in white lead manufactories.

Waters, from whatever source, appear to act on a freshly polished surface of lead. Thus, I have found the water taken from the Thames, Clyde, Gorbals waterworks, Kypes River, Paisley waterworks, Givel River, sources in the neighbourhood of Glasgow, to act on lead; the greatest amount of saline matter in these specimens being in that of the Thames, which contains about twenty-two grains of solid residue in the imperial gallon, while the Givel contains only about seven grains. Again, I find the water from a well at St. Thomas's Hospital London, to act very sensibly on a fresh surface of lead, although the solid constituents in the gallon amount to about one hundred grains. This experiment is further corroborated by a circumstance in reference to a well, respecting which I was consulted several years ago in Glasgow. The wells of Glasgow, like all those of large cities, are known to be impure. They contain from fifteen to one hundred grains of salts in the imperial gallon, and yet, on one occasion, water was brought to me which had been pumped through a new lead pipe from a well in a garden, which contained a considerable quantity of oxide of lead diffused through it. I inferred that the oxide of lead was principally in suspension, from the fact that when the water was filtered through a single paper filter, no lead could be detected in the water when it had passed the paper; and it is a well ascertained fact that water contaminated with oxide of lead, is entirely freed from it by permeating a filter of sand. All these facts relate to lead possessing a bright surface. For lead, when allowed to remain in these waters for a few days, ceases to suffer appreciable corrosion; or, if the lead be removed from the water, exposed to the air, and afterwards immersed, but an insignificant action, if any, can be detected.

Loch Katrine water I examined several years ago, when it was proposed to be introduced for the supply of Glasgow. I considered it then, as I do now, a water admirably adapted for domestic use, and I have not had my opinion in the slightest degree affected by the laboratory experiment exhibited on the committee's table, as I am convinced, from my acquaintance with the subject, that if the Loch Katrine supply had been introduced into Glasgow, nothing would have been heard of its influence on lead. I found it to contain about two grains of solid matter in the gallon, its main constituents being organic matter, common salt, sulphate and carbonate of lime.

When lead with a clean, bright surface is introduced into it, the lead is rapidly acted on, and white scales of oxide fall to the bottom of the vessel in which the experiment is made. When such water, with the suspended oxide of lead, is passed through a double filter of paper, the oxide is detained on the filter, and little or none seems dissolved in the water which passes through the paper. Hence it would happen, that should any corrosion occur on the first use of new lead cisterns, the insoluble oxide will be deposited at the bottom of the cistern, and will only intermingle, in a trifling degree, with the contents of the cistern; while in old cisterns, or after the new cisterns have become tarnished, no action will occur. But to prevent any corrosion on first using the cisterns or pipes, the plan sometimes adopted at Tunbridge Wells might be had recourse to, of brushing over the fresh surfaces with a coating of lime. No description of water could be purer or better fitted for a beverage, or for culinary purposes, than the water supplied by lead pipes to the Trossach's Hotel, at Loch Katrine, where I lately tasted it when inspecting the experiments made under the charge of the engineer, at the outfall of Loch Katrine.

To set the objections at rest which have been urged against the use of Loch Katrine water, I may detail an experiment on a sufficiently large scale of a parallel nature, which has been in action for forty years. About 1814, a plumber at Tunbridge Wells introduced, at his own risk, a spring of water, by means of lead pipes and lead cisterns, into the houses of that place. A similar objection was taken to its use as on the present occasion. Traces of lead were even detected in some places in that portion of the water in immediate contact with the new lead cisterns, but none in the body of the water, or in the water discharged from the cisterns. Specimens of this water were sent to London in 1815, and tested by Dr. Thomas Thomson, without his being able to detect a trace of lead. I have a letter in the handwriting of the late Dr. Wollaston, dated 27th of December, 1815, in which he states that he could detect no lead in water sent to London from Tunbridge Wells. Traces were occasionally detected in the new cisterns, and, as I was assured by the late Dr. Thomson, only on the margins in contact with the lead, the largest quantity obtained being one grain in twenty gallons. Yet, from these incidental results, the water supplied to the village was condemned by the opponents of the scheme as *poisonous*. But the water still continues in use: the village has increased to a large town of 10,000 inhabitants; it is a popular place of resort for invalids; and after careful inquiry, I have not been able to discover among its residents even a suspicion of its contamination by lead. I examined the company's engineer, who was employed at the original works, and laid down many of the lead pipes and cisterns in his capacity, at that time, of plumber, and persons in the town, residents of about twenty years, who assured me that they had never heard the subject of danger to water from lead pipes mooted.

When bright lead is introduced into this water, it is acted on immediately. It contains between three and four grains of salts to the imperial gallon, and is, upon the whole, one of the purest waters which I have examined in reference to the supply to towns. In examining the reservoir in which the water is retained for the supply of the town, I observed a perforated plate of lead, through which the water passed to the iron conduit pipe, which was covered with a coating of oxide of lead, precisely similar to the coating lining the lead-pipes which I have seen brought from Inverness, a town with whose water supply I am well acquainted. The plate, I was told, had been in this position for six years. The deposit was therefore insoluble, or nearly so, in the water, and acted as a protecting covering against any further action on the lead. Hence it would appear that this water, by its rapidly oxidating power on lead, furnishes with so much the greater efficiency security against further corrosion. Perhaps no stronger fact could be adduced in proof of the perfect confidence of the inhabitants of Tunbridge Wells in the sanitary quality of the water than that—of which I was assured by the engineer—the company is now paying an annual dividend of ten per cent. The original spring introduced for supplying the village is situated at about the distance of a quarter of a mile to the south, and is emitted from the northern aspect of a declivity. Many years after it was in use, from the increase in the population, the supply was found to be inadequate

to the demand, and another spring, about a mile distant in the same direction, but issuing on the southern declivity of the same ridge, was collected in a similar reservoir of brick, and pumped into the village reservoir, to mix with the waters of the first spring, which is conveyed to its destination by gravitation. The second spring I found to be more rapid and more extensive in its action on lead than the water of the first spring, which alone, as far as I could learn, had attracted any attention; and even the circumstances to which I have already referred were quite unknown to any person with whom I came in contact. Still more recently, a third spring, under different management, but of very soft water, and therefore with the corrosive qualities upon lead of the waters affording the previous supply, has been introduced into the town, with the universal approbation of the inhabitants, as far as regards its wholesomeness, purity, and softness.

Irrespective, then, of the probability that the Loch Katrine water will lose much of its corrosive power by its contact with the various strata over which it must pass in the conduit, during its flow to Glasgow, I am of decided opinion that no more permanent danger is to be apprehended, in reference to health, from the transmission of the water through lead pipes, and detention in lead cisterns, than there is in the case of other waters supplied to towns. I may add, however, that I have always recommended the substitution of iron and other materials, as water-pipes, as much as possible, for lead; and, even when lead is employed, that it should be alloyed with tin.—*Lancet*, July 29, 1854.

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

43. *Poisoning by Solanin*.—An opinion has been prevalent, that certain diseased states in domestic animals arose from the deleterious action of solanin contained in potatoes, on which they had been fed. In testing the truth of this opinion, Dr. FRAAS made the experiments, of which the following account is an abstract. Otto had previously found that the ripe potato, unsprouted, contained only a trace of solanin; that more of this principle was discoverable in the stalk of the plant, but the greatest quantity in the sprouts, especially the shorter ones. A similar result was obtained by Berchtold, in the spring of 1853. In 100 grammes of the green potato-sprouts, he discovered 4 milligrammes of solanin; and another time, in the very short sprouts, 12 milligrammes.

The first observations on animals were made on two pigs, to one of which were given sprouted potatoes boiled, together with the water in which they were boiled, which necessarily contained all the soluble solanin; while to the other were given the boiled potatoes without the liquor, the food being, consequently, assumed to be free from solanin. This was continued from the 15th of April to the 8th of July, without any signs of ill health appearing in either animal, notwithstanding that the outbreak of the disease in pigs was mentioned in the journals as having appeared.

The next observations were made with pure solanin, 10 grains of which were given to a pig, without the least injurious result, and the next day 20 grains were given to the same animal, with no further result than producing a diminished appetite and frequent white thin stools, the animal being quite well again the next day. On the third day, 20 grammes of acetate of solanin were given without effect.

Five grains of pure solanin were given to two dogs, the only results of which were vomiting and dilatation of the pupil. Ten grains were now given, and the œsophagus was tied; strong efforts at vomiting were made, and there was much vascular excitement, but the dose was not fatal.

The sulphate of solanin was injected into the veins of dogs. In nine dogs, death resulted from doses of from 5 to 2 grains thus administered; smaller doses only served to excite the circulation. In the case of one dog, into whose

right jugular vein 5 grains were injected, respiration suddenly became difficult, accelerated, and spasmodic, with convulsions, and tetanic extension and drawing back of the head, and death occurred in seven minutes.

Two grains of solanin were injected into the right jugular vein of a horse without any result being observed. Into the jugular vein of another horse, 30 grains of sulphate of solanin were injected. The animal was suddenly attacked with severe difficulty of breathing, and exhibited a great increase in the action of the heart, with convulsions, so that he appeared to be dying. In the course of twenty minutes he appeared quite recovered.

Two grains of acetate of solanin were injected into the rectum of a rabbit. The symptoms produced were heaviness, apathy, and slowness of movement; dilatation of the pupil followed, increased activity of the circulation and respiration, and convulsions, which, however, ceased in about two hours' time. The animal now moved but little, but when it did attempt locomotion, it dragged itself along with difficulty; but no special loss of power of the hinder extremities was observed. It died in six hours.

To a second rabbit, a certain quantity of acetate of solanin was given daily by the mouth, commencing with one grain. After several days, no effect being produced, the dose was increased to 2, and after another interval, to 3 grains. Some days after this the appetite lessened, but no other alteration being observed, the dose was increased to 4 grains. At last, the animal became heavy, slow in its movements, remained lying the greater part of the day, and at last died, without paralysis or any considerable fever having been observed.

Dr. Fraas is inclined to the opinion, that the pernicious effects, which often follow feeding upon raw potatoes and potato-stalks, are due to the great quantity of alkaline and earthy salts which they contain, and never to the solanin. An analysis of the potatoes used in his experiments, gave the following results: In 100 parts of dried potatoes there were 4.22 per cent. of ash; or in 100 parts of fresh potatoes, 1.17 parts of ash. 100 parts of ash contained—

Sulphuric acid	2.90
Phosphoric acid	12.37
Silicic acid	A trace.
Chlorine	4.23
Potash	52.23
Soda	A trace.
Alumina	A trace.
Oxide of soda	A trace.
Magnesia	2.41
Lime	3.68
Carbonic acid	20.18
Loss	2.0
		<hr/>
		100.00

The potatoes contained no solanin.—*Brit. and For. Med.-Chirurg. Rev.* July, 1854, from *Archiv. für Pathol. Anat. und Physiol. und für Klinische Medicin*, Bd. vi. Heft 2.

44. *Causes of Death from the Use of Anæsthetic Agents.*—Dr. K. KING relates (*Edinburgh Med. and Surg. Journ.* Jan. 1854) a case of death occurring forty hours after the administration of chloroform, and makes the following remarks on the causes of death from the use of anæsthetic agents, which are worthy of consideration:—

“The subject of anæsthetic agents is one comparatively new to the profession; and although its rapid adoption and speedy admission as one of the recognized steps of every very important surgical operation, seem to disprove that slowness to leave the beaten track which has often been objected to against medical practitioners, it must be admitted that there still is a division of opinion as to the extent of their applicability, and the amount of danger which attends their use. In the opinion of some, whenever pain is to be inflicted, these agents may be employed, unless special circumstances, as diseased heart,

&c. contraindicate their use. Others, again, would limit their application to the severest surgical operations, considering that a certain amount of danger always attends their administration; and that therefore they are inapplicable in all cases which are usually painful, but unattended with fatal results. It is admitted by all, that deaths have not unfrequently occurred after the use of anæsthetic agents; but, according to some, the deaths actually caused by these agents are very rare, some of them being caused by accidental circumstances, and others by a neglect of those means which, if applied in time, would have obviated the fatal result. On inquiring into the mode in which death is produced, we find a remarkable discrepancy of opinion. In a very excellent paper in the September number of the *Edinburgh Monthly Journal* by Mr. Bickersteth, several cases and experiments are adduced to prove that death takes place usually, if not universally, by asphyxia. He gives as the result of his observations, that 'the respiratory movements cease before the cardiac;' and that, 'if artificial respiration be resorted to before the cardiac contractions are seriously affected, and be properly maintained for a sufficient period, the respiratory functions may be re-established.' He states it as his opinion that 'the pulse should not be taken as any guide during the administration of chloroform. It should be wholly disregarded, except under certain circumstances.' The attention should be mainly directed to the respiration; and, without denying the possibility of exceptions owing to peculiar idiosyncrasies, he considers that while the respiratory act is regularly performed, the state of anæsthesia may be produced without incurring any danger; but if the exhibition of the agent be persevered in while that state continues, syncope and asphyxia may occur simultaneously and cause sudden and irreparable death. But the fact of the pulse becoming less frequent is of no consequence—indeed is 'so constant, that by some it is supposed, and correctly supposed, that anæsthesia is not sufficiently complete for the performance of surgical operations, unless the pulse be below its standard.' Such at least is the general impression which his very admirable paper conveys to my mind; and if we can subscribe to his opinion, the induction of anæsthesia loses almost all its terrors, for death is preceded by a period of warning when, by prompt and simple measures, the fatal termination may be averted. The patient is in the condition of one seemingly drowned, but still capable of resuscitation. But unfortunately we find a different theory and different practice as decidedly and as ably recommended. In the *Lancet* of November 19, 1853, there is an abstract of a paper by M. Jobert De Lamballe, in which he says that 'the administration of chloroform should cease immediately when the beats of the heart decrease in number and force;' and again, 'the action of the heart should always be our guide; it is the best means of ascertaining the saturation of the nervous system by chloroform, and of judging how far this system has been influenced by the anæsthetic agent.' As to practice, M. Jobert 'prefers acting principally on the nervous system, rather than lose time by exciting the functions of respiration and circulation.'

"Without going at any length into this matter, I believe that there is much greater probability of asphyxia than of syncope supervening from the use of chloroform; because the muscles of respiration (and consequently the respiratory act) are governed by nerves connected with the cerebro-spinal system, which is much more readily affected by this agent than are the sympathetic nerve and its branches, which govern the cardiac movements, and which we have no reason to suppose are affected except secondarily. It is therefore natural to conclude that in most cases the failure of the respiratory act will take place before that of the heart; and that on its being noticed and the proper means adopted, time will be given to arrest the circulation of venous blood in the arteries, the continuance of which would necessarily prove fatal. At the same time, I fear we must consider it as proved that syncope may supervene without this premonitory notice. In a case recorded by Dr. Dunsinville in the November number of the *Monthly Journal*, the breathing was not observed to fail before the pulse stopped. Those 'who had an opportunity of observing the respiration, positively assert that the breathing did not cease before the pulse.' It is stated that after 'artificial respiration was had recourse to, in a

few moments he made a long inspiration, which was followed by four others at gradually lengthening intervals,' while 'no pulsation could be felt in the radial arteries.'

"An anonymous correspondent of the *Medical Times* (October 15) concludes from these facts that this was a case of 'mere syncope,' and that therefore, as a rule, 'the state of the pulse should be attended to, as betraying the first symptom of impending danger.' But it must be remembered that in such cases every inspiratory act is necessarily observed, while we have no clear evidence of the last contraction of the heart: the mere fact of the pulse at the wrist being imperceptible is no evidence that the heart had not contracted; and I have some difficulty in believing that five acts of inspiration took place without a single contraction of the heart! I believe, in this instance, that a failure of the heart's action (syncope) and a paralysis of the respiratory muscles (leading to asphyxia), occurred at the same instant—that a partial reaction took place, when most probably the observed acts of inspiration were accompanied by unobserved contractions of the heart—but that the efforts of nature were incapable of setting the machinery in motion, and that from the last natural inspiration the man's death is to be dated. The anonymous writer before referred to seems to think that this event is to be dated from the last perceived pulsation; but it would be monstrous to apply the term death to a man who was still capable of making natural respiratory efforts; and, if it were as capable of demonstration, I believe that in this case, contractions of the heart would have been found taking place after the last act of inspiration. But I do not deny that deaths have occurred in which failure of the heart's action has been the prominent symptom, and that syncope has been attended by its usual symptoms of pallor of the countenance, &c. (In Dr. Dunsmore's case the face and heart are said to have been a good deal congested.) Two cases are mentioned in the *Dublin Medical Press* (April 20, 1853), in which syncope having supervened, M. Nelaton restored suspended animation by inverting the body, after which the face recovered its colour, and the cases terminated favourably. And in a case of M. de Vallet, in the Hôtel Dieu d'Orleans, the patient became suddenly pale, respiration ceased, and tracheotomy, artificial respiration, and galvanism were of no avail to prevent the fatal result. I conceive, then, that it is clearly established—1. That a continuance of the use of anæsthetics would in all cases, if sufficiently long employed, paralyze all the muscles supplied by the cerebro-spinal axis, including the muscles of respiration; and, secondarily, those dependent on the sympathetic system; that in all cases an overdose of chloroform may be expected to produce asphyxia, which, however, may, in most cases at least, be arrested, if observed before the cessation of the heart's action, by the methods usually employed. 2. That in some cases, altogether independently of organic disease, the heart's action may suddenly cease, even without an undue quantity of the drug having been employed; when, if the heart can be stimulated to contract, respiration (which cannot in the living body go on independently of the heart's action) may be restored and life preserved. The treatment here would be that usually adopted in syncope; for if the heart can be stimulated to contract, the respiratory muscles are capable of performing their functions; the patient is simply in the condition of one who has fainted. This class of cases must be rare, because anæsthetics (as before remarked) act on the cerebro-spinal much more readily than on the sympathetic system. 3. That in another class of cases death is produced by the simultaneous failure of the respiratory and cardiac movements; that these are the most dangerous of all; and that the treatment employed ought to combine the methods used in the first two cases.

"At one time I thought that death always resulted from asphyxia; but the reported cases before alluded to, and the observations both of myself and others, convince me that we must be prepared to meet with other and even more formidable dangers. However much this is to be regretted, I think it must now be admitted, and ought to be generally known; for if the profession and the public are to have the full benefit of anæsthetic agents, it is necessary to look the dangers and difficulties attending them fairly in the face, when, by a proper discrimination of the causes likely to lead to a fatal result, we may be

prepared *promptly* to apply those remedies more particularly suited to the case in question. We must be ready to lay down preconceived views and prejudices; and no man need be ashamed to change his opinion on a subject still new to the profession, and regarding which every day is accumulating experience and elucidating new facts.

"But to return to my case, from which I have wandered further than I intended: it is quite clear that by none of these methods was death produced. The administration of chloroform presented no peculiarities; he recovered from its effects in the usual way. When I saw him ten hours after, there was nothing about him to induce the slightest apprehension. Are we therefore to exonerate the chloroform from any share in the fatal result? Is the fact of a perfect recovery from the immediate effects of the anæsthetic agent to be received as proof that the system had entirely thrown it off, and that the condition of the individual is precisely what it was before its administration? The symptoms which on the 2d September manifested themselves, bore too strong a resemblance to those artificially induced on the 1st, not to suggest the idea that the two events were in some way connected. There was the loss of the intellectual powers—diminished sensibility, the strongest sinapisms produced no appearance of uneasiness—the respiration was impeded and stertorous—in a word, the condition in which he was on the 2d, seemed only an exaggeration of that produced on the 1st. Indeed chloroform acts by suspending the powers of the cerebro-spinal axis; its influence being directed especially to the sensitive nerves. Could we find an agent whose actions are limited to this, we would have reached the acme of anæsthetic discovery. In addition to the fatal results of asphyxia and syncope, are we to add to the dangers of administering chloroform those which may arise from a return of coma? Is it possible that the comatose state induced by chloroform may outlast the period of its administration, or even return after some hours of apparent health? This question is one which has often forced itself on my mind, and to which the mere fact of no such occurrence having ever taken place seemed hardly a sufficient answer. Convulsions or epileptic fits seem not uncommon results. Dr. Dunsmure, in speaking of his fatal case, remarks, that 'he seemed to take a slight convulsion like an *epileptic fit*, and such as I have seen on several occasions in people who have led an intemperate life.' Dr. Murphy, in the *Association Journal*, September 2, 1853, details two cases in which chloroform was administered during parturition; in the first of which 'distinct dyspnoea, excessive lividity of the face, and all the signs of extensive engorgement of the lungs and head' came on an hour and a half after labour; and though temporarily relieved, half an hour after 'she suddenly awoke with a return of the distressing dyspnoea, that was soon followed by *convulsions* and almost immediate death.' In the second case, two hours after the termination of labour, the patient was seized with slight cough and some difficulty of breathing: in the evening, dyspnoea had so much increased, that she was cupped to 10 ounces. She slept for two hours, 'but when she awoke the dyspnoea returned with still greater distress, and continued to increase during the night. On the following morning she became asphyxiated, and died about 2 o'clock P. M.,' 24 hours after the termination of labour. In remarking on these cases, Dr. Murphy asks the very appropriate question: Is it probable that, when no dangerous symptoms presented themselves during the administration of chloroform, fatal effects could result from it 'at a time when the vapour was dissipated, or, if any remained, its force greatly weakened, and less likely to cause such effects?' We know that the chloroform acts on the nervous centres, and that its action is usually evanescent; but is it necessarily so? After the cause has been removed, may not the effect remain? The second of Dr. Murphy's cases I have sometimes thought to have a shade of resemblance to the one I have just detailed, as far as symptoms go. The account is too scanty to enable us to form a decided opinion on this subject. Dr. Murphy states that she died asphyxiated. Now my patient died asphyxiated, *i. e.* the heart beat rapidly and convulsively after the respiration had become almost impeded; but I do not consider asphyxia to have been the *cause* of death, because, though its immediate precursor, it came on merely in the train of events. Death commenced from the brain (coma), and extended

to the nerves of respiration (asphyxia). Had there not been *post-mortem* appearances clearly pointing to different causes of death, one might have thought that in both cases the fatal supervention of paralysis of the nervous centres might have been prepared by its first artificial induction. Dr. Murphy states that it is improbable this should occur, seeing that he cannot 'find the report of a single case in which the patient who inhaled chloroform without an unfavourable symptom was attacked two hours after with dyspnoea, still later became asphyxiated and died.' And again he states that 'he cannot find a case of the kind in the whole range of surgical practice.'—'The danger—the only danger—which the surgeon dreads, is sudden death during the operation.' I do not mean to say, that in Dr. Murphy's cases death was caused by chloroform; but I do think that we have a right, derived from *a priori* reasoning, to fear that all danger is not past when the patient emerges unharmed from the anæsthetic condition; and that it is only more extended observation which can prove whether those fears are wholly chimerical. My own impression is, that in Mr. T.'s case death was not caused by the administration of chloroform; but I ground that opinion, not on the fact that no other case of the sort has been recorded, nor on the supposition that, in the nature of things, such an event might not occur, but on the appearances presented by the *post-mortem* examination, coupled with the symptoms for which he first sought my advice.

"A comatose condition was induced by chloroform, which was as evanescent as that state so induced commonly is—twenty-four hours afterward true coma supervened, and proved fatal. But the *post-mortem* examination of the head showed that this latter condition was connected with effusion of bloody serum—a common cause of coma, but not (as far as we know) ever observed as a result of chloroform. In cases where death has resulted from the employment of this agent, effusion has never been found, and in many it is distinctly mentioned that the brain was not even congested; so that the comatose state is produced without the intervention of vascular excitement, and therefore by a process different from that which was found to have occurred in the case I have related. I admit, however, that it is possible that an influence may be exerted on the brain, capable of superinducing, after a longer or shorter period, effusion of serum or other secondary effects; as we know that a person may recover from concussion, and yet sink under other results of the same cause which produced the concussion. Nor will this appear a far-fetched comparison, if we suppose, as I think we must do, that chloroform acts directly on the nervous centres. But there is a feature of the *post-mortem* examination which I now would call attention to, and that is the pus found between the parietal and visceral layers of the arachnoid. This is a rare phenomenon, and therefore not so well understood as other lesions of the brain and its membranes, which have been more frequently observed. It was found as a thin layer spread over the summits of both lobes, and adherent to the visceral portion. It did not extend to the basilar portions of the brain, and therefore can hardly be supposed to have been caused by any extension of inflammation from the mucous membrane of the nose. Indeed, neither during life nor after death was there anything to indicate that inflammation of the Schneiderian membrane had taken place. It could not have caused compression, and therefore is of importance only as proving the existence of previous arachnoid inflammation. But there was no redness or visible sign of inflammation of the arachnoid. There was much congestion of the cerebral vessels, but the products of their inflammation would have been thrown out in the subarachnoid tissues. Now I cannot help thinking that had there been so intense inflammation of the arachnoid as to have caused purulent effusion in the space of time between my visit at ten o'clock P. M. and his death twenty-seven hours after, there would necessarily have been some *post-mortem* evidence. I conceive, therefore, that the condition which led to the formation of pus was pre-existent to the administration of chloroform. The feeling of stupidity, difficulty of articulation, loss of sense of smell, &c., may all have resulted, not from the closure of the nares by polypi, but from this internal disease. There was no heat of head—no apparent increased vascular action of any sort—nothing to call attention to any other cause than the mechanical obstruction. But after calmly weighing the whole

facts of the case, I firmly believe that a low obscure form of arachnoid inflammation produced those symptoms which seemed fairly and naturally to be referred to the presence of polypi. What effect the administration of chloroform may have had in such circumstances it is difficult to say, but I feel convinced that it at all events did not set up the action which terminated so fatally.

"The *post-mortem* appearances in this case were very peculiar. The position of the pus on the free surface of the arachnoid must be held conclusive as to inflammation having existed in that membrane. Dr. Watson mentions the fact of such effusion as being rare (vol. i. p. 380), and quotes from Dr. Abercrombie a case in which it was observed in a child who had been three weeks ill; and says, that 'if simple arachnitis of an acute kind ever happen, it has not been my fortune to see or to recognize it.' While pus was found in the summit of the brain, the effusion of serum at the base was into the arachnoid cavity, and not in the subarachnoid space; and though bloody (*i. e.* red-coloured) serum was found in the ventricles in more than usual quantity, it was not excessive, nor by itself at all sufficient to cause death. There was also congestion of the bloodvessels of the brain; not inflammation of its substance. But that and the effusion of serum might, and probably did, take place within the last twenty-four hours of life—the purulent deposit I think hardly could.

"On these grounds I maintain that in the case I have related death did not result from chloroform. At the same time, I give it as my decided opinion, that until we find an agent which is capable of producing anæsthesia—inaction of the nerves of sensation—without implicating the functions of the cerebrum and motor nerves, we must be prepared to encounter occasional dangerous cases of asphyxia and syncope, and fatal ones in which these states occur simultaneously; and further, I would be inclined to expect, that cases might yet occur to prove that with the cessation of the immediate effect of the anæsthetic agent danger has not entirely ceased. In the meantime, I think it the duty of every surgeon to communicate to the profession any fact occurring in his practice in relation to this subject which presents novel and interesting features. It is in this way only that we shall learn the true merits of the case, and decide the important question of the compatibility of the anæsthetic agents at present known with all constitutions free from actual organic disease."

45. *Detection of Blood-stains on a Knife covered with Rust.*—M. DAUBRAWA was requested to ascertain the existence of blood-stains on a knife which was suspected to have been used in the commission of a murder. The knife having lain a long time in a damp place, was rusted; but certain bright rust-free spots could be distinguished amid the rust. On heating the point of the blade, these spots scaled off, while the rust remained adherent; on the other hand, on immersing the knife in dilute hydrochloric acid, the bright spots remained untouched, although the rust was readily dissolved. It was probable that these bright spots were blood-stains; but as some non-nitrogenous organic acids will produce similar marks, some of the detached scales were heated in a test-tube, and, by the disengagement of ammonia from the hematine of the blood, caused a blue colour on reddened litmus-paper. The whole blade was then macerated for a long time in distilled water, which acquired a reddish discoloration, and by the aid of a lens, fibrin could be seen adhering to the blade in the situation of the bright spots. Ammonia added to the solution caused no precipitate; nitric acid gave a white precipitate; it became turbid from heat; solution of chlorine at first produced a green tint, this colour then disappeared, and white flocculi were deposited. These different fluids having been evaporated to dryness and burnt, and the residue dissolved in hydrochloric acid, demonstrated the presence of iron by its appropriate reagents.—*Journal de Chimie Médicale*, December.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

Puncture of the Bladder through the Symphysis Pubis.—In our number for April last, we published an account by Dr. D. Leasure, of Newcastle, Pa., of a case of retention of urine, in which he had punctured the bladder through the symphysis pubis. We have since had communicated to us, through Dr. H. J. Bowditch, a letter from Dr. J. M. BRANDER, dated Jersey, June 10, 1854, in which he sets forth his claims as the originator of this operation. As we have already published (see preceding number of this Journal, p. 256) abstracts of a paper relative to this operation, read by Dr. Brander at a meeting of the Medical Society of London, in April of this year; and as Dr. Leasure does not claim any originality in this operation, but distinctly alludes to its having been done by others, it will be sufficient to insert the following extract from Dr. Brander's letter:—

“In support of my claim to originality in this novel operation, I would recapitulate in a few words that its practicability and superiority occurred to me during my dissections in Paris, in the year 1825, at which period I had the honour of reading an essay on this subject before a meeting of the Society of L'Athénée de Médecine, in whose published transactions it is mentioned. The paper was subsequently read in a translated and amended form before the Royal Medical and Physical Society of Edinburgh, in 1839, repeating the same at a meeting of the Medical and Physical Society of Calcutta, in the *Transactions* of which, in the year 1842, it is published (*vide* vol. viii. Part II.), with a recently practiced successful case appended.

“The operation was further brought to notice in the *Lectures on Surgery*, by Professor Sir George Ballingal and the late Sir Charles Bell, at the University of Edinburgh.

“I may mention, also, that the operation has been successfully performed on several occasions by myself and others in India, precisely after the manner adopted.”

Case of Narcotism from Opium—electricity employed in the treatment. By W. R. BULLOCK, M. D., of Wilmington, Del.—Aug. 14, 1854. Was at 12 P. M. requested to visit J. P., who was “supposed to have taken something.” The patient is a man of about 40 years of age, married, and the father of several children; constitution naturally robust; shoemaker by trade; intelligence greater than is usual in persons of his position in life; addicted to the use of alcoholic liquors, though not to the extent of beastly intoxication. When under their influence he is foolish, irritable, and imaginative of evil against him on the part of others. In this state of mind, he took a quantity of laudanum about a year since, which produced nausea, and was in part rejected before I saw him. Emetics were administered, and the stomach emptied; no narcotism followed.

During the evening of the 14th, he had maintained his habitual manner, and retired about 10 P. M. About 11 P. M., the attention of his wife was attracted by the unusual character of his breathing, which, with the change

in his appearance, and the impossibility to arouse him, induced the request for medical aid at the time stated.

Found him lying on his back on a truckle-bed; countenance pallid, and bathed in a cold perspiration; lips somewhat livid; eyeballs turned up beneath the lids, and fixed; pupils contracted and immovable; head hot; insensibility to external impressions profound; respiration three to four per minute; jerking, and somewhat stertorous; extremities warm; pulse accelerated, full, and hard.

His wife stated that of late he had been drinking, but only so as to render him foolish, peevish, and suspicious; nothing having occurred to make her at the time fearful of any design on his part to repeat the attempt of last year, she found it difficult to believe that he had swallowed any poisonous drug. The breath afforded no indications whatever; deglutition impossible; venesection to twelve or fifteen ounces, when the pulse yielded, and the flow was stopped. Sinapisms to the lower extremities; cold to the head; simple enema; the greater part of the enema was retained, but no evacuation produced.

After waiting a short time to observe the effect of the treatment, and still uncertain as to his being under the influence of a narcotic, requested a search to be made of his clothing. A letter was found in the pocket declaring his intention to destroy himself, and assigning some trivial reasons therefor. A few moments after, a bottle was found near the bed, which had undoubtedly contained laudanum. No doubt now remaining respecting the cause of his condition, returned to my house (near by) for a stomach-tube, but found it impossible, from the stiffness of the tube near the point—the tube was a new one—to direct it downward from the fauces into the œsophagus, the extremity striking obstinately against the back of the pharynx, and rendering the danger of perforation imminent in case of the application of more force than was actually employed. The difficulty persisted in spite of every attention to the proper position of the head of the patient, and endeavours with the finger in the fauces to give the instrument the required direction. Then commenced the use of the magneto-electric machine (one of Kinné's), passing the current from the spine through the muscles of the chest, the abdomen, and the region of the diaphragm. At this time received the counsel and assistance of Dr. Askew in consultation. The Doctor made another attempt to pass the œsophagus tube, but encountered the same difficulty, and the same success. Ice was procured, and ice-cold water kept applied by compresses to the head. A sinapism was placed along the spine, and others to the extremities, and to the region of the diaphragm—the electricity being continuously applied. Whilst under this treatment the number of respirations increased to about five per minute, the pulse remaining good. About 4½ A. M. the patient opened his eyes, roused up, spoke, and recognized those about him; appearing also to be conscious of his situation. He sighed frequently and deeply; and the respiration became gradually more regular. Ammonia, and cold water, were now administered. There was some difficulty in deglutition, and a disposition to hawk and spit. The electricity was dispensed with, and some of the sinapisms removed.

He remained conscious and rational for about three-quarters of an hour, then complained of great weakness; broke out into a profuse perspiration, and rapidly sank into a condition even more unpromising than before. The electricity and sinapisms were immediately resumed. The pulse, which, during the state of consciousness, had risen to be so full and hard as to make me think that the lancet might be again required, diminished greatly in force,

whilst its frequency increased to from 150 to 160 per minute. The number of respirations was reduced to two or three per minute, and that under the influence of the full power of the machine passed from the neck in the course of the phrenic nerve to the region of the diaphragm. The current was also passed through the region of the medulla oblongata, and through the entire course of the spine, the muscles of the chest and abdomen being in like manner alternately stimulated. Frictions were also made to the chest and extremities. The electric current was particularly effective in the production of the inspiratory act when passed in the direction of the course of the phrenic nerve. The ribs would at all times be somewhat elevated when thus applied; but the sudden, sighing inspiration could be produced only at regular intervals, which were generally waited for. The inspirations thus excited were commonly followed immediately by one other more feeble in power. These efforts were steadily continued until 8 A. M., at which time the patient was again seen by Dr. Askew. The respiration had not at all increased in frequency, whilst the force of the pulse had declined; the lividity of the surface, and the prostration being so great at this juncture, that a much longer continuance of life appeared almost hopeless. About 9 A. M. the pulse had perhaps gained a little in force, and the warmth, which had diminished in the extremities and had left the head, was gradually restored; so that cold cloths were again applied to the latter. In this condition, with slight fluctuations in the heat of the surface and force of the pulse, the patient remained throughout the morning; the sustained action of the heart, which, throughout, seemed disproportionate to the respiratory power, affording the only plausible ground for hope that, by maintaining the respiratory act, a favourable issue might possibly ensue.

About 1 P. M. put the machine into the hands of attendants, with directions to keep up the action, and left to attend to some affairs of urgency. Returning about 2 P. M. found a person called to state that from some cause or other the machine, in their hands, had ceased to operate a short time after my departure, but that about half-past one P. M., to the astonishment of all present, the patient again roused up. Saw him immediately, and found him rational when his attention was strongly called; eyes open, and nearly natural in aspect; mind wandering when not spoken to, and disposed to revert into stupor. The respiration, though still very slow, was much improved; sighing, and the force of the pulse, was increased; head so hot that ice-cold water was steadily applied; much thirst. Cold water was frequently given in moderate quantities; also ammonia, strong coffee, and beef-tea. Determined, if possible, to prevent a relapse into stupor, to which there was a strong disposition. The sinapisms were continued, as also momentary applications of an electric current of diminished power. The extremities were rubbed, and warmth applied to the feet. By these means the patient was kept awake until evening, by which time the tendency to drowsiness was almost dispelled, and he sat up to take his drinks. Visited him at 8 P. M., leaving directions to allow him to sleep, but to be carefully watched, and roused if symptoms of prostration or stupor should occur; also to give a little ammonia and nourishment during the night.

Aug. 17. Passed a comfortable night, though somewhat disturbed by dreams and phantoms; better in every respect; complains of great weakness; pulse good, force moderate; bowels not opened; tongue much coated, and brown and dry; some nausea, with an occasional rejection of an acid watery fluid. From this time nothing of particular interest presented; the tongue cleaned after the administration of mercurial cathartics, which, how-

ever, operated with difficulty; and the patient was in a couple of days about his business, as usual. On inquiry, it was learned that the quantity of laudanum swallowed amounted to from fʒij to fʒiiss; not a drop of which was removed from the stomach. The electricity was continuously applied for nearly twelve hours up to the time of the last return of consciousness.

Case of Rupture of the Uterus at the Moment of Delivery. By HUTTERSLEY P. WORTHINGTON, M.D., of Maryland.—Dec. 16, 1851, I received a hurried summons to Mrs. S., a strong, robust, hard-working woman, the mother of eight children. Upon my arrival, I found the child, a healthy female one, delivered about fifteen minutes, but the mother much prostrated from profuse hemorrhage. Her attendant, an intelligent and experienced female, informed me that the labour had been rapid, but as the child's head passed the vulva, there was a copious dash of blood, which continued until and after the delivery. The placenta was still retained. My attention was not immediately attracted to the full amount of the hemorrhage, being rather directed to ascertain and remove the cause. Administering a copious draught of brandy, and applying cold cloths to the abdomen, following the course of the cord, I introduced my hand to remove the placenta; finding the uterus contracting, and not immediately reaching the placenta, I pursued my exploration for it, when I suddenly felt my hand slip through a jagged strictured orifice, and in contact with what I at once recognized as a portion of the intestine. The uterus was ruptured at its fundus; the placenta had slipped into the peritoneal cavity, and could not be reached. I continued the use of stimulants, with the effect of stopping all further hemorrhage externally; but a sudden rigor, twenty minutes after my arrival, showed that further efforts would be unavailing; the patient soon expired. I regretted my inability to obtain consent for a *post-mortem* examination.

This woman had for two or three months previous, though apparently in good health, expressed and acted upon the conviction, that she would not survive this labour. I was struck with her frequent enunciation of this conviction; and, as she was of an equable and cheerful disposition, and not likely to have expressed such fears without some morbid impression originating the feeling, I had endeavoured to discover any reason for it, but without avail. I had attended her in several former labours, all of which were easy. What connection could there have been between the cause of her death and this state of feeling? In our ordinary fevers, more particularly those of a congestive type, we frequently meet with instances of this prescience of death before active febrile symptoms have developed themselves, and account for them as effects of the morbid poison in its primary depressing action upon the system. Could there have existed a softened or diseased spot, say of ulcerative inflammation, in the *fundus uteri*, which during gestation, without giving rise to much general disturbance, could yet, through nervous depression, account for the woman's prescience, and which caused so weakened a state of the parietes as to cause them to give way during labour? A *post-mortem* examination might have elucidated this question.

A truly remarkable feature in this case, was the point of time at which the rupture took place; not until the head was low in the pelvis, and when there was no obstruction to its progress.

LAUREL FACTORY, Maryland.

DOMESTIC SUMMARY.

Case of Total Inversion of the Uterus, in which Extirpation of the entire Organ was successfully practised.—Dr. E. GEDDINGS, Professor of Surgery, &c. in the Medical College of the State of South Carolina, relates (*Charleston Med. Journ.* Sept. 1854) the following case:—

"On the 16th of May, 1854, I was requested by Dr. A. P. Pelzer, to meet him in consultation, in the case of a negro woman belonging to Mr. Robt. White, in King Street. On my arrival, Dr. Pelzer called my attention to a large pyriform tumour, equal in magnitude to a foetal head at the full term, which, proceeding from within the vagina, hung pendant between the thighs. This tumour was large and rounded below, but contracted into a rather thick pedicle above, which could be traced about three-fourths of an inch within the vulva, at which point its contour was surrounded by a kind of *cul-de-sac*, beyond which the finger could not be passed. Its whole surface was covered by a rough, thickened mucous membrane, abraded and ulcerated on many points, considerably inflamed, and disposed to bleed when roughly handled. In the general aspect, it bore a strong resemblance to a case of prolapsus of the uterus, of long standing, but the uniform roundness of the most dependent part, together with the absence of the *os tincae*, served at once to convince us that it was of a totally different nature.

"The first supposition that presented itself to my mind was, that it might be a case of prolapsus of the bladder, of such long duration, that the walls of the organ had become very much thickened, and otherwise altered in texture. But on introducing the catheter, and passing my index finger around the neck of the tumour within the vulva, I was enabled readily to discover that it was a case of complete inversion, with extensive hypertrophy of the uterus, of ancient date. The orifice of the urethra was but little removed from its normal position, and in passing my finger up, on the posterior and lateral aspect of the neck of the tumour, as far as the reflected walls of the vagina would allow it to reach, I could distinctly discover the elastic feel imparted by the convolutions of the small intestines, which rested on the partially inverted walls of the vagina.

"How long the inversion had existed, could not be satisfactorily ascertained; but as there is reason to suspect that the accident must have occurred at the period of her last delivery, an approximative conclusion may be drawn from the fact, that her youngest child, a daughter, was present, and had the appearance of a person of from eighteen to twenty years of age. The report of the woman herself was, that she had been greatly annoyed by the tumour for many years, but had generally been enabled, by partially forcing it up into the vagina, and sustaining it there by means of a T bandage, to pursue her ordinary avocations. Latterly, it had increased so much in size as to render this impracticable, and at the period of our visit, any attempt at replacement, however partial, was productive of excruciating pain. She was, besides, suffering so much from engorgement and inflammation of the inverted organ, that, considering this, together with the partial and uncertain benefit likely to accrue from any merely palliative treatment, it became a serious question how we could most readily and efficiently relieve our patient.

"Reflecting on all the circumstances of the case, it occurred to me that excision of the entire inverted organ presented a rational prospect of relieving not only the present sufferings, but also the cause of much future annoyance. The vagina being also partly inverted, the danger of such an operation was materially diminished, inasmuch as we would in consequence of that condition, be enabled to excise the entire mass by cutting through the vaginal walls, thus leaving the substance of the uterus untouched.

"Dr. Pelzer concurring with me, I seized the neck of the tumour as high up as possible, between the thumb and index finger, and manipulating in such manner as to satisfy myself that it contained none of the convolutions of the intestines, I proceeded to include it in a strong ligature, for the twofold purpose of preventing the protrusion of the intestines, and obviating any serious hemorrhage.

The neck of the tumour was then cut through, a little below the ligature, with a single swipe of a probe-pointed bistoury.

"The operation was exceedingly simple and easy; was attended with no great pain; and, as may be supposed, was executed in a few seconds.

"The after-treatment presented no features of particular interest, and the case progressed so favourably that after a few days, I was enabled to discontinue my visits, leaving the patient in the hands of Dr. Pelzer, who in a short time, transferred her to Prof. Frost, the family physician, who, at the period of our attendance, was absent from the city. She speedily recovered, and, as I understand, has since done well.

"On making a section of the tumour, it was found to present a solid homogeneous mass, of a grayish-white texture, and fibrous appearance. The whole cavity formed by the inversion of the walls had become obliterated by adhesions between the opposing peritoneal surfaces; but the point of junction between the vagina and the contour of the cervix could be distinctly recognized, the incision, as stated above, having passed through the walls of the vagina.

"Partial and complete extirpation of the uterus, for various objects—inversion, prolapsus, carcinomatus and other degenerations of its structure—has been so often practised, that the simple operation and the description of which I have detailed, possess no claims to interest in point of novelty; yet it has some value as affording an additional instance to prove that, under similar circumstances, the unfortunate victims of a displacement so deplorable, may often be relieved of much suffering and inconvenience. It might be interesting to collect full references to the numerous cases in which extirpation has been practised on account of inversion, but as I have not time to execute the task, I must content myself with this brief and imperfect exposition of a single case.

Treatment of Rheumatism.—Dr. Fuller, in his recent work upon rheumatism, regards the pathology of the disease as depending upon the presence of a "*materies morbi*," which he supposes to be an excess of lactic acid, owing to defective assimilation, or suppressed perspiration. With the view to the neutralization and elimination of this acid, he recommends the free use of alkalies, by which means he "hopes the average duration of an attack may be reduced, from a month or six weeks, to ten days or a fortnight."

Dr. JOHN B. CHAPIN, states (*New York Medical Times*, Aug. 1854), that during the attendance of Dr. Swett, at the New York Hospital, the present year, all the patients admitted with acute articular rheumatism, were put under the "alkaline treatment;" and he gives a statistical table of twenty-five cases treated by this method.

The salt chosen was the tartrate of soda and potassa, a neutral salt, possessing the property of rendering the urine alkaline.

"The plan of treatment usually pursued was, if the patient presented himself with unusual excitement of the skin and pulse, to administer a mixture of sulphate of magnesia and tartarized antimony, until the skin was relaxed, and the pulse reduced to a more natural standard. The Rochelle salt was then directed, in drachm doses, every two or three hours during the daytime, till the urine was rendered alkaline, when it was gradually suspended. A lotion of carb. potass. $\mathfrak{z}\text{ij}$, with opium $\mathfrak{z}\text{ij}$ to the pint of water, was directed as an external application. The administration of the salt was not attended with disagreeable consequences, with the exception occasionally of some ulceration about the fauces—in no case was its action so severe upon the bowels as to require its entire suspension. The persons attacked were in the full vigour of health, and the character of the disease acute in its form. The frequency of administration of the remedy was governed very much by the reaction of the urine.

"On the admission of the patient, the urine was tested, and, in all cases, was found to be of acid reaction, and the secretion of the skin presented the usual acid odour. The treatment was generally commenced the second or third day after admission, and the urine was rendered of decided alkaline reaction in an average of five days after its commencement; the longest period it resisted the alkaline reaction having been twenty days, and the shortest two. The secretions of the skin have not, I believe, been noticed to alter. In one case, attended

with profuse perspiration, which yielded readily to treatment, the coloured shirt the patient wore entirely lost its colour; and it was suggested whether the same change did not take place in the perspiration as in the urine. The average amount of the salt administered was from five to seven ounces.

"The average date of commencing improvement was seven days after commencement of treatment, coinciding, in the large majority of the cases, with the commencing alkalinity of the urine. The improvement was invariably permanent, and after the urine was rendered alkaline, *no new articulations were effected*, as a general rule.

"The average period of convalescence was twelve days after admission; and the whole duration of the disease, including the period previous to admission, was twenty-two days. Of thirty cases treated by Dr. Swett, during April and May, 1853, during which time no uniform course of treatment was pursued, the average duration was five and a half weeks. One of the most gratifying results of the alkaline treatment was the diminished frequency of cardiac complications. Twenty-one of the twenty-five were free from any complication, three were *admitted* with aortic obstruction, and one with mitral regurgitation. Not one patient was attacked with any heart complication during the treatment of the disease. Comparing this result with the practice last year, it was found that four had mitral regurgitation, six aortic complication, and three suffered from pericarditis; thirteen in all, out of thirty.

Hemeralopia.—Dr. C. S. FENNER, of Memphis, Tenn., says (*N. O. Med. and Surg. Journ.* Sept. 1854) that he has met with a considerable number of cases of this affection among negroes working in an extended range of prairie plantations, commencing in Pickens County, Alabama, and extending up through Noxubee, Lowndes, Oktibbeha, and Monroe Counties, Mississippi.

The disease makes its appearance almost invariably in the spring, or early in the summer months, before the young corn and cotton plants have attained sufficient size to shield the eye from the dazzling reflection of the sun's rays from innumerable small pieces of limestone, and fragments of shells that are thickly interspersed through the soil. These prairies constitute immense bodies of a peculiar black soil, lying on a strata of limestone. In many places, the rock approaches the surface, and is entirely bare, reflecting the sun so powerfully as to be almost insupportable to the eye. To a person standing in the middle of some of these large prairies, the horizon, in many directions, seems to meet the surface without a tree or shrub to obstruct the view. I attribute the frequent occurrence of night-blindness through this section of country, to the exhaustion of the nervous power of the retina from continued exposure day after day to the powerfully reflected solar rays, without having any objects, even at a distance (particularly trees covered with green foliage), to relieve the eye, by frequently looking up and bringing a change of scenery within the field of vision.

"Counter-irritation," he adds, "from a succession of blisters, and repose within doors for a few days, are usually all that is requisite to give entire relief. I have been informed of several cases that recovered after the lapse of a few months, without any treatment whatever; and in no instance have I known any permanent injury to vision result from permitting the disease to take its own course."

Nitric Acid as an Anti-Periodic.—Dr. GEO. MENDENHALL calls attention (*Western Lancet*, Aug. 1854) to the use of nitric acid in the treatment of intermittent fever. The facts upon which his paper is based are mainly taken from an Inaugural Dissertation, by Dr. E. T. Bailey, of Emmetsville, Indiana.

The latter states that in the section of country in which he resides, there is a large portion of marshy land, and, therefore, the circumstances are favourable to the development of autumnal fevers. His attention was first attracted to the use of nitric acid in the treatment of intermittent fevers, by noticing its effects in a case of chronic intermittent, which was attended with profuse night-sweats, and for which complication he administered the remedy. In this case, there had been daily paroxysms for the preceding five days; night-sweats profuse, the tongue coated, and the bowels constipated. Nitric acid was given in doses

of six drops, diluted with water, in the evening; and he was agreeably surprised to find that the paroxysms did not return on the following day; and this circumstance induced him to try its effect in other cases as an anti-periodic.

Since that time he has treated over ninety cases of intermittent fever with this article, with remarkable success. Of this number, all recovered promptly except ten; and in every one of these unsuccessful cases, the remedy was discontinued contrary to directions.

Fifteen of the whole number were of the tertian type, and seventy-five of the quotidian. In fifty cases, there was no return of the chill after commencing the use of the acid. The others were rarely attended by more than one paroxysm, and in no case by a third. When the patient had a paroxysm after taking the medicine, it was in every case diminished in intensity and duration.

In Dr. Bailey's practice, this remedy has entirely superseded every other article for the purpose of interrupting the paroxysms of intermittents. His mode of proceeding is to give from five to eight drops of the commercial nitric acid, properly diluted, once in six hours, without regard to intermissions or exacerbations. Cathartics and alterants may be necessary for the purpose of changing certain conditions of the system; but so far as the interruption of the paroxysms is concerned, the acid may be given without any preparation of the system whatever, if we choose to do so.

Remedial Properties of Simaba Cedron.—The number of the *New York Journal of Medicine* for September contains an interesting paper by one of the editors, Dr. S. S. PURPLE, on the remedial properties of the Simaba cedron, and on its employment as a substitute for quinia. The following are Dr. P.'s conclusions:—

“That it possesses decided anti-periodic properties, and is, therefore, applicable in the treatment of periodic diseases.

“That it is less likely than quinia to produce the aggregate of encephalic or neuropathic phenomena, induced by overdoses.

“That it may, in large doses, repeated often, produce griping of the bowels, and even diarrhoea; but that these conditions are easily controlled by appropriate medicaments.

“That, as a remedy in intermittent fever, it possesses properties in many respects equal to quinia, and in most cases is equally adapted to the cure of this disease.

“That, in the treatment of yellow fever, it does not appear to possess any particular advantages over quinia, but, nevertheless, is equally well adapted to fulfil the indications which call for the use of this latter remedy.

“That it possesses marked tonic properties, and deserves a prominent place in this classification of the *Materia Medica*.

“That in chronic dysentery, diarrhoea, dyspepsia, and all states of the stomach accompanied with impaired or difficult digestion, its use will be found to be attended with benefit.

“That, should a demand arise for its use in medicine, it is believed that it will be found not difficult to obtain a supply, in quantities sufficient to afford it at a much less price than quinia.”

Amputation at the Shoulder-Joint. Dr. M. CARNOCHAN records (*American Medical Monthly*, Sept. 1854) a case of amputation at the shoulder-joint “for the removal of a large osteo-procancerous tumour of the humerus.” The subject of the case was an Irishman, thirty years of age, admitted into the State Emigrant's Hospital. The tumour involved the whole extent of the humerus, measured in its largest circumference twenty-nine inches, and weighed, after removal, eighteen and a half pounds.

INDEX.

A.

Abel and Bloxam, Handbook of Chemistry, notice of, 192
 Abnormal obturator artery, 251
 Abortion, 368
 Abscess containing hair, 113
 Abscess of lungs without previous symptoms, 99
 Acids, vegetable and mineral in epidemic diseases of bowels, 521
 Ackley's case of hermaphroditism, 369
 Aconite, external use of tincture of, 100
 Adherent placenta, 104
 Air, passage of, per vaginam, 370
 Alabama, notice of Transactions of State Medical Society of, 467
 Alley, ovarian tumour, 113
 Amaurosis following retinitis, 552
 Amputation at knee-joint, 542, 253
 ————, shoulder-joint, 252, 582
 ————, statistical account of cases of, performed in Pennsylvania Hospital, 13
 Anæsthesia, local, 215, 217
 Anæsthetic agents, causes of death from, 569
 ————, effects of cold, 287
 Anæsthetics, cold and chloroform as, 511
 Anderson, belladonna in counteracting poisonous effects of opium, 225
 Aneurism of aortic arch, 392
 Arnott, cold and chloroform as anæsthetics, 512
 Arteries, elasticity of, considered as a cause of animal heat, 209
 Artificial cornea, 95
 Attaway, twins of different colour, 290
 Auscultation, Hughes on, 498
 Autoplastic operation for burn, 411

B.

Bache, yellow fever, 121
 Beaver, remarkable case of emphysema, 281
 Beck, diseases included under the term prolapsus uteri, 271
 Belladonna in counteracting the poisonous effects of opium, 225
 Bennett, abortive treatment of smallpox by zinc plaster, 224
 ————, on Pulmonary Tuberculosis, notice of, 501
 ————, paracentesis thoracis, 545
 Bernard, conditions under which certain substances usually retained in the blood pass into the urine, 212

Bethune, artificial cornea, 95
 ————, herpes preputialis, 94
 ————, protrusion, &c. of nictitating membrane in a horse, 103
 Betz, thyroid asthma of children, 530
 Bierbaum, legal and social rights of malformed beings, 276
 Bigelow, aneurism of aortic arch, 392
 ————, varicose arterial tumours, 395
 Bischoff, human ovulation and menstruation, 137
 Bladder, puncture of, through symphysis pubis, 256
 Blake, abortion, 368
 ————, passage of air per vaginam, 370
 ————, precocious infant, 371
 Blighted fœtus, 367
 Blood crystallization, 508
 ————, influence of cod-liver and cocoa-nut oil on, 509
 ————, influence of dilution of, on urinary secretion, 508
 Blood-stains, detection of, 574
 Bowditch, self-adjusting stethoscope, 85
 ————, tuberculosis, 91
 Bozeman, vesico-vaginal fistula, 283
 Brain, starch in, 210
 Brander, puncture of bladder through symphysis pubis, 256, 575
 Breast, tumour of, 85
 British Navy, notice of Statistical Reports of Health of, 179
 Bronchitis, compressed condition of lung after, 104
 Brown, ligature of carotid for epilepsy, 415
 ————, urinary calculus; 393
 ————, vaginal cystocele, 272
 Budd on pyrosis, 221
 Bullock, narcotism from opium, 575
 Burn, autoplastic operation for, 411
 Burnam, recovery after taking a large dose of prussic acid, 276
 Burnett, artificial cornea, 95
 Bushnan, Animal and Vegetable Physiology, notice of, 504
 Busk, starch in brain, 210
 Butler Hospital Report, notice of, 489
 Butyric acid ether for flavouring cream, 114

C.

Cabot, dislocation of humerus, easy manual reduction, 82
 ————, tumor of breast, 85

Cabot, ulceration of knee-joint, amputation, 89
 Cæsarian section, 565
 Cancer of rectum and vagina, 88
 Cancer, modern philosophy of, 531
 — of uterus, arsenical injections in, 541
 Carcinoma of liver, 390
 Carnochan, amputation at shoulder-joint, 582
 Carotid, ligature of, for epilepsy, 415
 Carpenter's Comparative Physiology, notice of, 503
 Casper, order of succession in the putrefactive process, 274
 Catheter, female, 391
 Cauliflower tumour of uterus, 117
 Cerebellum, tumour connected with, 89
 Chapin, treatment of rheumatism, 580
 Charcoal coverlet to prevent smell from gangrenous sores, 514
 Chateau, experiments on squills, 510
 Chemistry, notice of Abel and Bloxam's, 192
 —, notice of Fownes's, 193
 Chevers, cure of mercurial salivation, 528
 Childs on ovariectomy, 257
 Cholera and choleraic diarrhœa, sulphuric acid in, 522
 —, strychnia in, 523
 Chloroform in hypochondriasis, 520
 — in midwifery, 263
 Churchill, paralysis during gestation and childbed, 555
 Clark, poisoning by prussic acid, 103
 Coale, abscess of the lungs without previous symptoms, 99
 —, dropping fluid from vials, 366
 —, encysted tumour on forehead, 366
 —, gleet, its treatment, &c., 93
 —, syringe for injecting melted lard in vagina, 389
 —, sudden and complete cessation of premature labour pains, 370
 Cod-liver and cocoanut oil, influence of, on blood, 509
 Cod-liver oil, its efficacy in consumption, 285
 —, modes of exhibiting, 218
 Cogswell, medical constituents of lemon, 219
 Cold and chloroform as anæsthetics, 512
 Cold as an anæsthetic agent, 287
 Colliquative sweating in phthisis, 225
 Colloid tumour, 386
 Cooke, phlebitis, 241
 Corbyn, large doses of quinia in fevers, 514
 Cornea, artificial, 95
 —, opacities of, 554
 Cotting, membranous croup, 396
 —, fracture of face, 399
 Cotyledon umbilicus in epilepsy, 225
 Cough, hysterical and spasmodic, 322
 Crisp, deposit of fat in certain conditions of the bodies of the lower animals, 212

D.

Dalton, movements of glottis in respiration, 75
 —, office of gastric juice in digestion, 313
 Daubrawa, detection of blood-stains, 574
 Delphinin, properties of, 511
 Diabetes mellitus, 529

Diabetes, treatment of 222
 Diarrhœa adiposa, confirmation of Bernard's views with regard to the functions of pancreas, 212
 —, subnitrate of bismuth in, 524
 —, sulphuric acid in, 522
 Digestion of fatty matters by pancreatic fluid, 307
 —, office of gastric juice in, 313
 Dislocation of both femora, 247
 — of metatarsal bone of great toe, 249
 Duncan, duration of pregnancy, 564
 Dunlap, cases of ovariectomy, 407
 Druitt, modern philosophy of cancer, 531
 Durkee, tumour beneath the skin, 369

E.

Ecker, mucous membrane of stomach, 208
 Elbow-joint, caries of, 367
 Emphysema, case of, 281
 Empyema, case of, 105
 Encysted gelatinous tumour of neck, 110
 Epidemic diseases, meteorological changes in reference to, 277
 — of bowels, acids in, 521
 Epilepsy, cotyledon umbilicus in, 225
 —, ligature of carotid for, 415
 —, Radcliffe on, notice of, 492
 —, treatment of, 516
 Erichsen, excision of ovarian tumours, 546
 Erichsen's Surgery, notice of, 505
 Erysipelas after vaccination, 108
 —, tincture of iodine for, 108
 Excision of knee-joint, 254

F.

Face, fracture of, 399
 Facial anæsthesia, 550
 False anchylosis of lower jaw, 131
 Fascia of Scarpa, anatomy and surgical relations of, 208
 Fat, the relation it bears to sugar in livers of mammalia, 211
 —, deposit of, in certain conditions of bodies of lower animals, 212
 Femora, dislocation of both, 247
 Fenner, false anchylosis of lower-jaw, 131
 —, hemeralopia, 581
 —, yellow fever at New Orleans, 184
 Fergusson, amputation at knee-joint, 542
 Fevers, large doses of quinia for, 514
 Flint, Clinical Report on Dysentery, notice of, 155
 — on Chronic Pleurisy, notice of, 155
 Fœtus, diseases of, 502
 Fort Moultrie and Sullivan's Island, climate and salubrity of, 21
 Fownes's Chemistry, notice of, 193
 Fractures, ununited, 547
 Fraser, strychnia in cholera, 523
 Fuhrer, the spleen and its capillary circulation, 507
 Fuller, sulphuric acid in cholera and choleraic diarrhœa, 522

G.

Gaffard, formula for gouty, rheumatic, and neuralgic affections, 225

Gangrenous sores, charcoal coverlet to prevent smell from, 514
 Gardner on Gout, notice of, 502
 Gastric juice, office of, in digestion, 313
 Gautier, laceration of urethra, 413
 Geddings, extirpation of inverted uterus, 579
 Gibb, relation that fat bears to sugar in livers of manimalia, 211
 Gibson, rupture of ovarian cyst during pregnancy, 269
 Gleet, its treatment, &c., 93
 Glottis, movements of, in respiration, 75
 Goulden, treatment of diabetes, 222
 Gout, notice of Gardner on, 502
 —, notice of Wells on, 174
 Gouty, rheumatic, and neuralgic affections, formula for, 225
 Gray, structure and use of spleen, 491
 Griffin, amaurosis following retinitis, 552

H.

Hake on vital force, 320
 Hamer, Cæsarian section, 565
 Hamin crystals, 509
 Hardy, local anæsthesia, 215
 Harelip, advantages of early operating, 544
 Harriss, varicocele successfully treated, 284
 Hayes, butyric acid ether for flavouring cream, 114
 Hayward, ovarian tumor, 113
 Headache, intermittent, 90
 Hemeralopia, 581
 Hemorrhage from the bowels in a new-born infant, 280
 —, unusually persistent, 80
 Hendrickson, trial of, 447
 Hernia, radical cure of inguinal, 249
 Herpes preputialis, 94
 Hodges, monstrosity, 394
 Hohl, double uterus, 263
 Homans, remedies for rheumatism, 392
 Hooper, staphyloma, 93
 Hughes on Auscultation, notice of, 498
 Humerus, dislocation of, 82
 Hunt, metastasis in its practical bearings, 239
 Huxley, structure of spleen, 207
 Hydrometra, case of, 133
 Hydrophobia in France, 232
 — in a cretin, 233
 — five years after bite, 234
 Hypochondriasis, chloroform in, 520
 Hysterical and spasmodic cough, 232

I.

Insane Hospital Reports, notices of, 481
 Intussusception, discharge of portion of intestine, 241
 Inversio uteri during labour, 560
 Iodide of potassium, lead diseases treated with, 286
 —, membranous croup cured by, 286
 Iodine, gargles of compound tincture of, for mercurial salivation, 528
 —, topical uses of, 223

J.

Jackson, compressed condition of lung after bronchitis, 104

Jackson, digestion of fatty matters by pancreatic juice, 307
 —, irregularities of measles, 119
 — on Ackley's case of hermaphroditism, 369
 —, spleen enlarged, 92
 —, tumour connected with cerebellum, 89
 —, uterine polypus, 397
 Jameson, extensive fistula, 129
 Jaundice as the result of imperfect closure of ductus venosus, 240
 Jaw, exfoliation of, after extraction of tooth, 87
 Johnson, inversio uteri, 560
 Jones, morbid changes in mucous membrane of stomach, 237

K.

Kameyla, for tapeworm, 525
 Keiller, hysteria and spurious pregnancy, 265
 Kentucky, Report on Births, Marriages, and Deaths in, notice of, 157
 Kierulf, influence of dilution of blood on urinary secretion, 508
 King, causes of death from anæsthetic agents, 569
 Kinlock, mistaken personal identity, 275
 Klingner, veratria in scrofulous affections of joints, 226
 Knee-joint, amputation at, 89, 253
 —, excision of, 254
 —, needles penetrating, 79
 —, ulceration of, amputation, 89
 Knight, appearance of parts in a popliteal aneurism cured by compression, 305
 Kohlrausch, vascular tissue of Schneiderian mucous membrane, 203

L.

Labour, case of, 279
 — pains, sudden and complete cessation of, 370
 La Roche, origin of yellow fever in Philadelphia, 194
 Larynx, exercise of vocal organs in ulceration of larynx, 229
 Lead cisterns in a sanitary point of view, 566
 — diseases treated by iodide of potassium, 286
 Leasure, tapeworm expelled by pumpkin seeds, 281
 Lee, fatal effects of imperfect union of umbilical vein, 240
 —, jaundice as the result of imperfect closure of ductus venosus, 240
 —, paraplegia from concussion of spine, 400
 Lemon, medical constituents of, 219
 — juice in rheumatism, 83
 Lloyd, arsenical injections in cancer of uterus, 541
 Louisiana State Asylum Report, notice of, 489
 Lung, compressed condition of, after bronchitis, 104
 Lyman, death from laudanum, 383

M.

- Mackenzie, Diffenbach's operation for un-
united fractures, 255
———, excision of knee-joint, 254
———, ophthalmia in nursing women,
553
Mackinnon, new remedy for tapeworm, 525
Madge, diseases of fœtus in utero, 502
Malformed being, legal and social rights of,
276
Marston, cure of diarrhœa adiposa, 212
Maryland Hospital, notice of Report of, 487
McClintock, phlegmasia dolens in the non-
puerperal state, 564
Measles, irregularities of, 119
Membranous croup, 396
Mendenhall, nitric acid as an antiperiodic,
581
Mercurial salivation, 528
Merriman, chloroform in midwifery, 263
Metastasis in its practical bearings, 239
Meteorological changes in relation to epi-
demic diseases, 277
Miller, double monstrosity, 289
———, encysted gelatinous tumour of neck,
110
Minie rifle, wound from, 246
Minot, cancerous disease of rectum and va-
gina, 88
———, dislocation of thumb, 84
———, imperfect physical and mental de-
velopment in a child, 110
Moleschott, functions of liver and spleen, 507
Morland, extracts from records of Boston
Society for Medical Improvement, 79, 366
———, exfoliation of lower jaw after ex-
traction of a molar tooth, 87
———, foreign body in nostril, 389
———, powerful effects of small quantities
of opium, 379
Monstrosity, 394
———, double, 289
Mount Hope Institution for Insane, notice
of Reports of, 481
Murphy, neuralgic headache, 229

N.

- Narcotism from opium, 575
Needles penetrating knee-joint, 79
Neuralgia, history, treatment, &c. of, 417
Neuralgic headache, 229
Nictitating membrane, protrusion, &c. of, in
a horse, 103
Niepce, hydrophobia in a cretin, 233
Nitric acid as an antiperiodic, 581
Norris, statistical account of amputations per-
formed in Pennsylvania Hospital, 13
North Carolina, notice of Transactions of
Medical Society of, 480
Nostril, foreign body in, 389
Nursing woman, ophthalmia in, 553
Nussbaum, artificial cornea, 95

O.

- Oakes, tubular pregnancy, 378
Obturator artery, abnormal, 251
Opacities of cornea, removal of, 554
Opium, effects of belladonna in counteracting
poisonous effects of, 225
———, narcotism from, 575

- Opium, powerful effects of small quantities
of, 379, 383
Osborne, chloroform in hypochondriasis, 520
Ovarian cyst, rupture of, 269
——— disease, 118
——— dropsy, treatment of, by injection of
iodine, 259
——— tumour, 113
——— tumours, excision of, 546
Ovariectomy, 257, 407
Ovulation and Menstruation, notice of Bis-
choff on, 137
Ovum, blighted, 92

P.

- Pancreatic juice, digestion of fatty matters
by, 307
Paracentesis thoracis, 545
Paralysis during gestation and childbed, 555
Paraplegia from concussion of spine, 400
Parkman, colloid humour, 386
———, imperforate rectum, 79
———, vagitus uterinus, 371
Parrish, case of labour, 279
Parsons on neuralgia, 417
Paterson, portion of intestine discharged
after intussusception, 241
Patterson, tapeworm unsuccessfully treated
by kousoo and male fern, 524
Pennsylvania, notice of Transactions of State
Medical Society of, 476
Perineum, operation for laceration of, 404
Peritonitis, 111, 116
Personal identity, mistaken, 275
Phlebitis in connection with deligation of
veins, 241
Phlegmasia dolens in the non-puerperal state,
564
Poisoning by prussic acid, 103
——— by solanin, 568
———, review of trial for, 447
Popliteal aneurism, 94
———, appearance of parts in a
case cured by compression, 305
Porter, climate and salubrity of Fort Moul-
trie and Sullivan's Island, and on yellow
fever of Charleston, 21, 341
Potter, amputation at knee-joint, 253
Power, dislocation of metatarsal bone of
great toe, 248
Precocious infant, 371
Pregnancy, duration of, 564
——— in rudimentary horn of uterus,
565
———, spurious, 265
Prichard, dislocation of both femora, 247
Prolapsus uteri, diseases included under this
term, 271
Prurigo formicans, treatment of, 527
Prussic acid, poisoning by, 103
———, recovery after a large dose of,
276
Pulmonary Consumption, notice of Thompson
on, 501
——— Tuberculosis, notice of Bennett
on, 501
Pumpkin seeds, tapeworm expelled by, 281
Purple simaba cedron, 582
Putnam, blighted fœtus, 367
———, female catheters, 391

Putnam, irregularities of measles, 119
 ———, lemon juice in rheumatism, 83
 Putrefactive process, order of succession, 274
 Pyrosis, 231

Q.

Quinia, large dose of, for fevers, 514

R.

Radcliffe on spleen, notice of, 492
 Ranking, cotyledon umbilicus in epilepsy, 225
 Rectum, imperforate, 79
 Retinitis in nursing women, 553
 Revaccination, is it necessary? 371
 Rheumatism, lemon juice in, 83
 ———, remedies for, 392
 ———, treatment of, 580
 Richard, treatment of prurigo formicans, 527
 Richardson, effects of recumbent position during syncope, 214
 ———, meteorological changes in relation to epidemic diseases, 277
 Robertson, operation for laceration of perineum, 404
 Rokitsanski, pregnancy in a rudimentary horn of uterus, 565

S.

Salter, process of repair in teeth, 209
 Sargent, cauliflower tumour of uterus, 117
 ———, fatal peritonitis, 116
 ———, ovarian disease, 118
 Scarpa, fascia of, 208
 Schneiderian mucous membrane, vascular tissue of, 208
 Shanks, case of hydrometra, 133
 Shattuck, peritonitis, 111
 Shoulder-joint, amputation at, 252
 Sina cedron, properties of, 582
 Simpson, treatment of ovarian dropsy by injections of iodine, 259
 Sims, vesico-vaginal fistula, 283
 ———, gangrene of lungs cured by turpentine, 527
 Skoda on Auscultation, notice of, 499
 Smallpox, abortive treatment of, by zinc plaster, 224
 Smith, autoplasmic operation for burn, 411
 ———, advantages of early operation for harelip, 544
 Snow, local anæsthesia, 217
 Solanin, poisoning by, 568
 South Carolina Asylum Reports, notice of, 488
 Southern Central New York, notice of Transactions of Medical Society of, 479
 Spleen and liver, functions of, 507
 ———, and peculiarities of its capillary circulation, 507
 ———, enlarged, 92
 ———, structure and use of, 491
 ———, structure of, 207
 Spurious pregnancy, 265
 Squills, experiments on, 509
 Stanley, ununited fractures, 547
 Staphyloma following iritis, 93
 Starch in brain, 210
 Statistics of operations in London Hospitals, 260

Stethoscope, self-adjusting, 85
 Stokes on Diseases of the Heart and Aorta, notice of, 169
 Stomach, morbid changes in mucous membrane of, 237
 ———, mucous membrane of, 208
 Storer, adherent placenta, 104
 ———, carcinoma of liver, 390
 ———, cure of tænia, 377
 ———, erysipelas after vaccination, 108
 ———, external use of tincture of aconite, 100
 ———, premature birth, 108
 ———, question of revaccination, 371
 ———, ulceration of cæcum, 398
 Strong, unusually persistent hemorrhage, 81
 Struthers, anatomy and surgical relations of fascia of Scarpa, 208
 Strychnia, amaurosis cured by, 552
 ——— in cholera, 523
 Subnitrate of bismuth in diarrhœa, 524
 Suicidal suffocation, 275
 Sulphuric acid in cholera, 522
 Swift, lead diseases treated by iodide of potassium, 286
 Syncope, effects of recumbent position during, 214
 Syringe for injecting melted lard into vagina, 389
 Szokalski, removal of opacities of cornea, 554

T.

Tænia, 377
 Tapeworm expelled by pumpkin seeds, 281
 ———, new remedy for, 525
 ———, unsuccessfully treated by koussou and male fern, 524
 Tardieu, report on Hydrophobia, 232
 Taylor, facial anæsthesia, 550
 Teeth, process of repair in, 209
 Teichman, blood crystallization, 508
 ———, Hamon crystals, 509
 Thompson, influence of cod-liver and coconut oil on the blood, 509
 ———, lead cisterns in a sanitary point of view, 566
 ——— on pulmonary consumption, 501
 ———, oxide of zinc and hyoscyamus for colliquative sweating, 225
 Thumb, dislocation of, forwards, 84
 Thyroid asthma of children, 530
 Tincture of aconite, external use of, 100
 Todd, treatment of epilepsy, 516
 Townsend, blighted ovum, 92
 Transactions of State Medical Societies, 467
 Trouseau, exercise of vocal organs in ulceration of larynx, 229
 Tuberculosis, 91
 Tubular pregnancy, 378
 Tucker, acids in epidemic diseases of bowels, 521
 Tufnell, wound from Minie rifle, 246
 Tumour beneath skin, 369
 Turpentine, inhalation of, for gangrene of lungs, 527
 Twins of different colour, 290

U.

Ulceration of cæcum, 398
 Ulcers from irritation of nails, 246

- Ununited fractures, 547
 ———, Dieffenbach's operation
 for, 255
 Upper maxillary bone, excision of, 86
 Ure, amputation at shoulder-joint, 252
 —, treatment of ulcers from irritation of
 nails, 246
 Urethra, laceration of, 413
 Urinary calculus, 373
 — secretions, influence of dilution of
 blood on, 508
 Uterine polypus, 397
 Uterus, double, 263
 —, inverted, extirpated, 579
 —, rupture of, 578
- V.
- Vaginal cystocele mistaken for prolapsus
 uteri, 272
 Vagitus uterinus, 371
 Van Praag on delphinin, 511
 Varicocele cured, 284
 Varicose arterial tumours, 395
 Veins, deligation of, 241
 Veratria in scrofulous affections of joints, 226
 Vermont Asylum, notice of Reports of, 487
 Vesico-vaginal fistula, 283, *ib.*
 Vision, apparatus for accommodating eye to,
 288
 Vital force, its pulmonic origin, &c., 320
 Von Dursch, diabetes mellitus, 529
 Von Roth, apparatus for accommodation of
 eye to vision at different distances, 288
- W.
- Ware, case of empyema, 105
 —, intermittent headache, 90
- Warren, abscess containing hair, 113
 —, caries of elbow-joint, 367
 —, excision of upper maxillary bone,
 86
 —, needles penetrating knee-joint, 79
 —, notice of late works by, 206
 —, popliteal aneurism, 94
 Wells on Gout, notice of, 174
 —, radical cure of reducible inguinal her-
 nia, 249
 West, Ulceration of Os Uteri, review of, 146
 —, Lectures on Diseases of Infancy and
 Childhood, notice of, 168
 Wilson, Healthy Skin, notice of, 505
 Winn, elasticity of arteries as a cause of
 animal heat, 209
 Wood, cold as an anæsthetic agent, 287
 —, efficacy of cod-liver oil in consump-
 tion, 285
 —, hysterical and spasmodic cough, 232
 Worthington, rupture of uterus, 578
- Y.
- Yellow fever, Bsche on, 121
 —, Fenner on, 184
 —, La Roche on origin of, in
 Philadelphia, in 1853, 199
 —, Porter on, 21, 341
 Young, hemorrhage from the bowels in a
 new-born infant, 280
- Z.
- Zinc, oxide of, and hyoscymus in colliquat-
 ive sweats, 225
 — plaster, abortive treatment of smallpox
 by, 224

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SESSION OF 1854-5.

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The Faculty having determined to resume the *Winter Sessions* of this School, the next Course (the 37th) will be commenced on the first Monday in November next, and continue four months.

Preliminary Lectures will be given during the month of October.

A College *Clinic* has been established by the Faculty, in which many surgical operations and cases of disease are exhibited to the Class.

Fee for the full course,	\$70
To those who have attended two full courses in other colleges,	45
Matriculation and library ticket	5
Graduation fee,	25
Demonstrator's ticket,	10

All payable in *advance*.

Boarding and lodging, \$2 50 to \$3 50 per week.

LEXINGTON, KY., Aug. 16, 1854. **662** ROBERT PETER,
Dean of Med. Faculty, T. U.

UNIVERSITY OF LOUISIANA, MEDICAL DEPARTMENT.

The Annual Course of Lectures in this department will commence on Monday, November 13, and will terminate in the ensuing March.

JAMES JONES, M. D.,	Professor of Practice of Medicine.
J. L. RIDDELL, M. D.,	Professor of Chemistry.
WARREN STONE, M. D.,	Professor of Surgery.
A. H. CENAS, M. D.,	Professor of Obstetrics.
A. J. WEDDERBURN, M. D.,	Professor of Anatomy.
GUSTAVUS A. NOTT, M. D.,	Professor of Materia Medica.
THOMAS HUNT, M. D.,	Professor of Physiology and Pathology.
CORNELIUS C. BEARD, M. D.,	} Demonstrators of Anatomy.
SAMUEL P. CHOPPIN, M. D.,	

The rooms for Dissecting will be open on the third Monday in October.

The Faculty are Visiting Physicians and Surgeons of the Charity Hospital, and attend this institution from November to April.

The Students accompany the Professors in their visits, and, free of expense, enjoy extraordinary practical advantages.

There are, during the Session, about eight hundred persons prescribed for daily.

In 1853, the number of patients was thirteen thousand seven hundred and fifty-nine.

THOMAS HUNT, M. D., *Dean*.



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